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THE

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I. THE APATITE DEPOSITS OF CANADA.*

BY T. STERRY HUNT, L.L.D., F.R.S.

The presence of apatite in the Laurentian rocks of North America has long been known to mineralogists, and within a few years so much interest has been excited by the economic importance of deposits of this mineral, found in certain parts of Canada, that a brief history of our knowledge of these deposits may not be unacceptable to the members of the 'American Institute of Mining Engineers. It was in 1847 that the present writer was shown by a local collector of minerals some large crystals, which had been called beryl, found in North Burgess, in Ontario. These were at once recognized as apatite; and after a visit to the locality, this was described in the repoit of the geological survey of Canada for that year as likely to furnish an abundant supply of a valuable fertilizer: the opinion being then expressed that the fact of "the existence of such deposits as these will prove of great importance."

Specimens of apatite from this locality, collected by the writer, were shown among the economic minerals of Canada at the great

* Read before the American Institute of Mining Engineers, at Cleveland, Febuary, 1884, and reprinted from its Proceedings.

exhibitions of London and Paris in 1851 and 1855, and the mineral had already been found by explorers at several other points in the same region previous to 1863. In the *Geology of Canada*, published in that year, the writer resumed the results of his further studies of these deposits, and described the apatite as occurring in the Laurentian rocks, both distributed in crystals through carbonate of lime, and in "irregular beds running with the stratification and composed of nearly pure crystalline phosphate of lime." This was further said to occur in North Burgess, in several parallel "beds interstratified with the gneiss."*

In a subsequent report of the geological survey, in 1866, I again noticed the occurrence of the apatite in beds in the pyroxenic rocks often found associated with the gneiss. It was said, "the presence of apatite seemed characteristic of the interstratified pyroxenic rocks of this section. in which it was very frequently found in small grains and masses, alike in the granular and the micaceous schistose varieties." In these rocks, the apatite was said to mark the stracification, and to form, in one example, a bed, in some parts two feet thick, which was traced 250 feet along the strike of the pyroxenic rock. I at the same time described the occurrence of apatite, often with calcite, in "true vein-stones, cutting the bedded rocks of the country;" alike gneiss, pyroxenite, and crystalline limestone. These latter deposits were farther spoken of as well-defined veins, traversing vertically, and nearly at right angles, the various rocks; as often banded in structure, and including besides apatite both calcite and mica, occasionally with pyroxene, and more rarely with hornblende, wollastonite, zircon, quartz, and orthoclase. These veins were said to be very irregular, often changing rapidly in their course from a width of several feet to narrow fissures. It was added, "it is evident that this district can be made to supply considerable quantities of apatite;" and while the uncertainties arising from the irregularities of the veins were mentioned, it was said that "some of the deposits might probably be mined with profit."†

Before following farther this history, it may be stated that there are two districts in Canada which have, within the past few

* Loc. cit, pp. 592, 761.
† Loc. cit., pp. 204, 224, 229.

years, been found to contain deposits of apatite of economic importance; one in the province of Ontario, in which the above observations were made by the writer previous to 1866, including parts of the counties of Lanark, Leeds, and Frontenac; and the other, since made known, in the province of Quebec, chiefly in Ottawa county. In both cases it is found in the rocks of the Laurentian series, consisting of granitoid gneisses with bands of quartzite, of pyroxenite, and of crystalline limestone. These ancient and highly inclined strata, with a northeast strike, rise from beneath the horizontal paleozoic rocks near Kingston, and again pass beneath them near Perth. These overlying strata belonging to the Ottawa basin, hide, moreover, to the eastward, the apatite-bearing gneisses of this district; which, a short distance to the westward, are again concealed by the Taconian and other overlying pre-Cambrian groups in Hastings county. The gneissic belt is here seen chiefly in the the townships of Loughborough, Storrington, Bedford, North and South Crosby, and in North Burgess, where the apatite was first discovered.

The country presents a succession of small, isolated, rounded, rocky hills, alternating with numerous small lake-basins, hollowed out of the gneiss, and sometimes out of the interstratified limestones; the general trend both of the hills and the lakes being coincident with the strike of the rocks. These, though concealed in the valleys by considerable depths of alluvial soil, are seen in the hills to be hard and undecayed. These geographical features, as I have elsewhere pointed out, wer apparently determined by sub-aërial decay previous to the erosion which removed from them the softened and disintegrated portions, leaving the present outlines.*

When, after cutting the forest-growth which covers these hills of granitoid gneiss, fire is allowed to pass over the surface, destroying the undergrowth, the comparatively thin layer of soil is laid bare and is soon washed away by the rains; leaving the bald rocky strata exposed in a manner singularly favorable for geological study, but rendering the region sterile. To prevent this process of denudation it has become the practice in some parts of the country, after burning over the hillsides, to sow them, without

* See the author's paper on "Rock Decay Geologically Considered."—*Amer. Jour. Sciences*, Sept., 1883.

loss of time, with grass-seed, which, at once taking root, protects the soil from the destructive action of rains, and transforms i into good pasture-land. This system, which has been adopted to a considerable extent in parts of Frontenac county, Ontario, is worthy of record and of imitation in other regions.

The similar apatite-bearing gneisses, which are found to the north of the river Ottawa, a little northeast of the city of that name, are in Ottawa county, Quebec, and chiefly in the townships of Buckingham, Templeton, and Portland. They reproduce all the characteristics of the first mentioned district, and may be looked upon as a prolongation of it beneath the northwestern limb of the paleozoic basin already mentioned. Later observations, both in Ontario and in this latter district, where mining operations have been carried on within the past few years, have been recorded by Messrs. Broome and Vennor, and by Dr. Harrington,—the latter up to 1878. They have, however, added little to our knowledge of the conditions of occurrence of the mineral beyond what had already been set forth in 1863 and 1866.

I have, within the past few months, examined with some detail many of the apatite-workings in Ontario, which have served to confirm the early observations, and to give additional importance to the fact, already insisted upon in previous descriptions, that the deposits of apatite are in part bedded or interstratified in the pyroxenic rock of the region, and in part are true veins of posterior origin. These gneissic rocks, with their interstratified quartzose and pyroxenic layers, and included bands of crystalline limestone, have a general northeast and southwest strike, and are much folded; exhibiting pretty symmetrical anticlinals and synclinals, in which the strata are seen to dip at various angles, sometimes as low as 25° or 30°, but more often approaching the vertical. The bedded deposits of apatite, which are found running and dipping with these, I am disposed to look upon as true beds, deposited at the same time with the inclosing rocks. The veins, on the contrary, cut across all these strata and, in some noticeable instances, include broken angular masses of the inclosing rocks. They are, for the most part, nearly at right angles to the strike of the strata, and generally vertical, though to both of these conditions there are exceptions. One vein, which had yielded many hun dred tons of apatite, I found to intersect, in a nearly horizontal attitude, vertical strata of gneiss; and in rare cases what appear, from their structure and composition, to be veins, are found coinciding in dip and in strike with the inclosing strata.

The distinction between the beds and the veins of apatite is one of considerable practical importance,—first, as related to the quality of the mineral contained, and second, as to the continuity of the deposits. The apatite of the interbedded deposits is generally compactly crystalline, and free from admixtures, although in some cases including pyrites, and more rarely magnetic iron-ore, with which it may form interstratified layers. Many will recall in this connection the band of magnetite, with an admixture of granular apatite, found interstratified in parts of the great magnetic ore-deposit known as the Port Henry mine, near Lake Champlain, in Essex county, New York; where in certain layers formerly mined, the apatite made up about one-half the bulk. I have seen an example of a similar association of magnetite and apatite from Frontenac county, Ontario. The latter mineral is, however for the most part found included in the beds of pyroxene rock, already mentioned, which is generally pale green or grayish green in color, sometimes containing quartz and orthoclase, and distinctly gneissoid in structure.

The veins present more complex conditions; while they are often filled throughout their width by apatite as pure and as massive as that found in the beds, it happens not unfrequently that portions of such veins consist of coarsely crystalline sparry calcite, generally reddish in tint, holding more or less apatite in large or small crystals, generally with rounded angles, and often accompanied by crystals of mica, and sometimes of pyroxene and other minerals. Occasionally these mixtures, in which the carbonate of lime generally predominates, will occupy the whole breadth of the vein. These *lime-veins*, as they are called by the miners, sometimes include cavities from which the carbonate appears to have been dissolved by infiltrating waters, leaving free the inclosed crystals of apatite. In some cases, however, these veins present cavities which have apparently never been filled with solid matter, and exhibit drusy surfaces, with quartz, and more rarely with barytine and zeolites. The calcareous veins often carry so much carbonate of lime as to be valueless for commercial purposes, unless some cheap means for separating the apatite can be devised. It may be said, in general terms, that while some of these true veins throughout portions or the whole of their breadth yield good and pure apatite, others are of comparatively little value. The bedded masses, on the contrary, are free from carbonate of lime, and although they may occasionally contain small quantities of mica, pyroxene, hornblende, or pyrites, these are seldom present to an injurious extent.

The question of the continuity of these deposits of both classes is an important one. Veins filling fissures that have been formed in rocks are sometimes continuous for great lengths and to great depths, but experience shows that their extent varies very much for different regions and for different rocks. Inclined beds, which were once horizontal sheets, inclosed in strata that have since been folded, should be as persistent in depth as they are in length; and when traced in the outcrop for many hundreds of feet, may be expected, under ordinary circumstances, to continue downwards as far, unless a turn of the inclosing strata bring them up again to the surface. The inclosed beds of apatite in the regions already noticed are often traced for 500 to 1000 feet and more, and there is reason to believe that they are continuous for long distances. The workings upon them have, however, as yet been very superficial, generally from twenty to forty feet, and rarely exceeding 100 feet. The deepest mine, which is in Ottawa county, is now about 200 feet.

The ordinary thickness of the bedded masses of apatite may be said to vary from one to three and four feet, though not unfrequently expanding to eight and ten feet, and even more, and sometimes contracting to a few inches; the same layer being subject to considerable variations. In some cases the apatite in a bed is found to thicken and then to diminish, or to be divided by the interposition of the accompanying pyroxenic rock. The condition of the apatite in these cases recalls the thickening and thinning sometimes observed in a layer of coal among disturbed strata, where, as the result of great pressure attending the movements of the harder inclosing rocks, it is alternately attenuated and swollen in volume; in which case a thinning in one portion is necessarily compensated for by a thickening of the parts adjacent.

The thickness of the veins also, as above stated, is very variable, and the same vein in a distance of a few hundred feet will sometimes diminish from eight or ten feet to a few inches. We have already noticed the variable nature of the contents of these veins, which are sometimes filled with with solid and pure apatite, and at other times present bands or layers of this mineral, with others chiefly of calcite, of pyroxene crystals, or of a magnesian mica, occasionally mined for commercial purposes. While these veins have yielded in many cases considerable amounts of apatite, they have not the persistency of the beds. Their study presents many interesting facts in paragenesis, which I have described in detail in the report of the geological survey for 1866, already quoted, and more briefly in my *Chemical and Geological Essays* (pp. 208-213).

It is worthy of remark, that some of the first attempts at mining apatite in Canada were upon these veins, and that their irregularities contributed not a little to the discouragement which followed the early trials. The larger part of the productive workings are upon the bedded deposits. These, however, as already noticed, are for the most part opened only by shallow pits; a condition of things which is explained by the peculiar character and the frequency of the deposits, and also by the economic value of the apatite. This mineral, unlike most ordinary ores, is, in its crude state, a merchantable article of considerable value, and finds a ready sale at all times, even in small lots of five or ten tons. Like wheat, it can be converted into ready money, at a price which generally gives a large return for the labor expended in its extraction. Hence it is that farmers and other persons, often with little or no knowledge of mining, have, in a great number of places throughout the district described, opened pits and trenches for the purpose of extracting apatite, and at first with very satisfactory results. So soon, however, as the openings are carried to depths at which the process becomes somewhat difficult from the want of appliances for hoisting the materials mined, or from the inflow of surface-waters, which in wet seasons fill the open cuts, the workings are abandoned for fresh outcrops, never far off. In this way a lot of 100 acres will sometimes show five, ten or more pits, often on as many beds, from twelve to twenty feet deep; each of which may have yielded one or more hundred tons of apatite, and has been abandoned in turn, not from any failure in the supply, but because the mineral could be got with less trouble and cost at a new opening on the surface near by.

These conditions are scarcely changed when miners, without capital, and unprovided with machinery for hoisting or for pumping, are engaged, as has often been the case, to extract the mineral at a fixed price per ton. These, having no interest in the future of the mine, will work where they can get the material with the least expenditure of time and labor, and often will quit the opening for some one which is more advantageous. The very abundance and the value of the mineral mined has thus led to its careless, wasteful, and unskilful exploitation. It is the working of these causes, in the way just explained, which has thrown undeserved discredit on this mining industry, and more even than the injudicious schemes of speculators and stock-jobbers, has retarded its legitimate growth.

It is evident that the proper development of these deposits will require regular and scientific mining in place of the crude plan of open pits and trenches, which, from causes already explained, has hitherto, with few exceptions, been followed. As a basis for calculation in mining, it becomes necessary to establish some data as to the production and the value of the apatite-layers which we have described. The specific gravity of the mineral, as deduced from many specimens of massive Canadian apatite, is from 3.14 to 3.24. If we assume 3.20, this will give for the weight of a cubic foot of apatite almost exactly 200 pounds. A fathom of ground, carrying a bed or vein of apatite one foot in thickness, will thus contain thirty-six cubic feet, or 7200 pounds of apatite; equal to a little over three and one-fifth tons of 2240 pounds each. Allowing the fractional portion, equal to nearly seven per cent., for loss in mining (it will be noted that coarse and finely-broken apatite are equally merchantable), we shall have as the net product of a layer of apatite for a fathom of ground mined, three gross tons for each foot in thickness.

The apatite of these deposits is generally greenish in color, often clear sea-green, but more rarely reddish brown in tint. The massive varieties are sometimes coarsely crystalline and cleavable, but sometimes finely granular, The veins often yield crystals of large size.

The mineral is essentially a fluor-apatite, containing not over two or three thousandths of chlorine and, in its purest state, about 92.0 per cent. of tricalcic phosphate. The analysis of a selected specimen gave me 91.2 per cent. of phosphate, but it is generally mingled with small portions of foreign matters, chiefly insoluble silicates, The analyses of seven specimens from different Canadian mines, published by Mr. C. G. Hoffman, in 1878, showed from 85.2 to 89 8 per cent. of phosphate.

The market-value of apatite, which, as is well-known, is chiefly consumed for the production of soluble phosphate by the manufacturers of artificial fertilizers, varies greatly, other things being equal, with its purity. Thus, while at present the price in England is 1*s.* 2*d.* the unit for apatite giving by analysis 75 per cent. of tricalcic phosphate, there is paid an addition of one-fifth of a penny for each unit of phosphate above that percentage, so that a sample yielding by analysis 80 per cent. is worth 1*s.* 3*d.* the unit. The price in the English market is subject to considerable fluctuations, having within the last four years been as high as 1*s.* 5½*d.*, and as low as 11*d.* the unit for 80 per cent. phosphate. The present may be considered as an average price.

The Canadian apatite shipped to England has yielded for various lots from 75 to 85 per cent., 80 per cent. being the average from the best-conducted mines, though lots from mines where care has been used in the dressing and selection of the mineral for shipment have yielded 84 and 85 per cent. Many of the smaller miners to which we have alluded, selling their product to local buyers, take little pains in dressing, and hence their product is apt to be lower in grade. It will be seen, from the rule adopted by foreign purchasers, that there is great profit in a careful selection and dressing of the mineral for market. The basis being 1*s.* 2*d.* the unit for 75 per cent., with a rise of one-fifth of a penny for each unit, it follows that while a ton of 75 per cent. apatite will bring only 87*s.* 6*d.*, a ton of 80 per cent. will command 100*s.*, and one of 85 per cent. 113*s.* 4*d.*

In the present state of the industry it is not easy to say what would be the cost of production. At the outcrop of the large masses of apatite, and in the open cuts and quarries already described, the cost of extraction and dressing is of course very variable, estimates in different deposits giving from $2 to $8 the ton. In Ottawa county, where, within the last four years, deposits have been opened and mined on a better system than heretofore, the figures of production and cost are instructive. According to the report of its manager in July 1882, the High Rock mine, in Buckingham, yielded, in 1880, 2400 tons, and in 1881, 2000 tons of apatite, An adjoining portion of land having been then acquired, the production of this company's mines in 1882 and 1883 is stated at 5000 tons annually; from eighty to ninety men being employed. The cost of the mineral is here given at $4 the ton, dressed, at the mine; in addition to which $3 is paid for carriage to the railroad or the river, and about $1 additional to Montreal, the port of shipment. The mines in the Ontario district are for the most part in or near to the waters of the Rideau canal, or some of the many lakes connected therewith, from which the freight to Montreal is $1.50 the ton. I am informed by a merchant, who is a purchaser and shipper of apatite, and is also engaged in mining it both in Ontario and Quebec, that the average cost for freight from Montreal to England, with selling-charges, is 20s. the ton; which, for apatite of 80 per cent., now worth 100s. the ton, would leave 80s., or $19.36. Deducting from this the cost of production and of transportation to Montreal, there remains a large profit.

The amount of apatite shipped from Montreal has gradually increased, and, according to published figures, attained in 1883, 17, 840 tons, of which, it is to be remarked that 1576 tons were delivered in Hamburg, and 650 in Stockholm, the remainder going to Liverpool, London, and other British ports. Of this about 15,000 tons were from Quebec, and the remainder from Ontario. It should be noticed that this was, with small exceptions, mined in 1882, and brought to the water-side during the winter season. It is estimated that the shipments of apatite for 1884 will equal 24,000 tons.

The methods of mining hitherto generally pursued in the apatite deposits of Canada, allow of many improvements which would materially reduce the average cost af production, and give a permanency to the industry which the present modes of working can never attain. The regularity and persistence of the bedded deposits. and of some of the veins, warrants the introduction of systematic mining by sinking, driving, and stoping; with the aid of proper machinery for drilling, as well as for hoisting and pumping. The careful dressing and selection of the apatite for the market is also an element of much importance in the exploitation of these deposits. The cost of labor in the apatite-producing districts is comparatively low, and there are great numbers of beds now superficially opened, upon which regular mining operations, conducted with skill and a judicious expenditure of capital, should prove remunerative. It must be added, that the areas in question have as yet been very partially explored, and that much remains to be discovered within them, and also there is reason to believe in outlying districts; so that in the near future the mining of apatite in Canada will, it is believed, become a very important industry.

II. The Origin of Crystalline Rocks.*

By T. Sterry Hunt, LL.D., F.R.S.

The author began by remarking that the problem of the origin of those rocks, both stratified and unstratified, which are made up chiefly of crystalline silicates, is essentially a chemical one. He then proceeded to review the history of the once famous dispute between the vulcanist and the neptunist schools in geology, as to whether granite and other crystalline rocks were formed by igneous or by aqueous agencies, and showed from recent writers that the controversy is not yet settled. He noticed, of the igneous school, both the plutonic and the volcanic hypothesis of the origin of these rocks, and then proceeded to consider the metamorphic and metasomatic hypotheses, which would derive them, by supposed chemical changes, from materials either of igneous or of aqueous origin.

The hypothesis of Werner was next explained. This conceives all such rocks to have been successively deposited in a crystalline form from a chaotic watery liquid, which surrounded

* Abstract of a paper read before the Royal Society of Canada at Ottawa, May 20, 1884.

the primitive earth, and at an early time held in solution the whole of the materials of these rocks. The inadequacy of all of these hypotheses was pointed out, though it was said that Werner's is the one nearest the truth.

The author conceives that the crystalline rocks were formed by deposition from waters which successively dissolved and brought from subterranean sources the mineral elements. Their formation is illustrated by that of granitic veins and that of zeolites—processes regarded as survivals of that which produced the earlier rocks. The true zeolites are but hydrated feldspars, while the minerals of the pectolite group correspond to the protoxyd-silicates of these ancient rocks, often remarkably concretionary in aspect.

The source of the elements of these rocks, according to the new hypothesis here proposed, was in the superficial layer which was the last congealed portion of an igneous globe consolidating from the centre. In this primitive stratum, porous from contraction, and impregnated with water, resting upon a heated anhydrous nucleus, and cooled by radiation, an aqueous circulation would be set up, giving rise to mineral springs. The water of these dissolved and brought to the surface, there to be deposited, the quartz, feldspars, and the other silicates which, through successive ages, built up the great groups of crystalline stratified rocks.

Exposed portions of the primitive silicated material would be subject to atmospheric decay and disintegration, giving rise to aqueous sediments of superficial origin, which would become intercalated with the deposits from subterranean sources. The reactions between the mineral solutions from below and these superficial materials were doubtless important in this connection, probably giving rise to certain common micaceous minerals; while dissolved silicates allied to pectolite, by their reaction with the magnesian salts which then passed into the waters of the ocean, would generate species like serpentine and pyroxene.

This process of continued upward lixiviation of the primitive chaotic layer would result in the production of a great overlying body of stratified acidic rocks, leaving below a basic residual and much diminished portion, the natural contraction of which would cause corrugations in the superincumbent stratified mass, such as are everywhere seen in these ancient rocks.

The source of volcanic rocks is partly in this lower basic and more or less exhausted stratum of comparatively insoluble and ferriferous silicates, whence come melaphyres and basalts; partly in the secondary or acidic zone, which, softened by the combined agency of heat and water, may give rise to granitic and trachytic rocks; and partly, it is conceived, in later aqueous deposits of superficial origin, which may also be brought within the influence of the central heat.

This attempt to explain the genesis of crystalline rocks by the continued solvent action of subterranean waters on a primitive stratum of igneous origin the author designates as the *crenitic* hypothesis, from the Greek *κρηνη* a fountain or spring. A preliminary statement of it was made by him to the National Academy of Sciences at Washington, April 15, 1884, and appears in the *American Naturalist* for June.

III. The Cambrian Rocks of North America.*

By T. Sterry Hunt, LL.D., F.R.S.

The writer gave his reasons for limiting the term Cambrian to the Lower and Middle Cambrian of Sedgwick, which contain the first fauna of Barrande. For the Upper Cambrian or Bala group, holding the second fauna, wrongly claimed by some as a lower member of the Silurian, and by others called Cambro-Silurian, he prefers the term Ordovician, now accepted by many British and continental geologists. This includes in New York the Chazy, Trenton, Utica and Loraine divisions; the Oneida marking the base of the true Silurian or third fauna. The Cambrian rocks of the great North-American basin may be studied in four typical areas: 1. the Appalachian; 2. the Adirondack; 3. the Mississippi; 4. the Cordillera area. To the first of these belongs the immense volume of greatly disturbed sediments along the whole eastern border of the basin, constituting the First Graywacke and the Sparry Limerock of Eaton; being the Upper Taconic of Emmons, and the Potsdam group and Quebec group of Logan. They are distinct from the unconformably

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* Abstract of a communication to the Boston Soc. Nat. History' Feb. 20, 1884.

underlying Taconian quartzites, marbles and schists (Lower Taconic) which the author regards as pre-Cambrian, and the still older crystalline schists of the Atlantic belt, including those, chiefly of Huronian age, which have been called " Altered Quebec group."

The name of Taconic cannot be retained for the Appalachian Cambrian, which was, as early as 1861, correctly claimed by Emmons as belonging to the period of the first fauna. The Hudson-River group, as originally defined, included the whole of the Appalachian Cambrian, with some portions of the underlying Taconian, and others of overlying Ordovician strata, from which, in the Appalachian area, their characteristic limestones are wholly or in great part absent. It is solely from this association with the Cambrian Graywacke of strata of Loraine age that the Hudson-River group has come to be regarded as the palæontological equivalent of the Loraine shales.

In the stable and little disturbed area around the Adirondack mountains, including the Champlain and Ottawa basins, the Cambrian is represented only by the quartzites and magnesian limestones of the Potsdam and Calciferous divisions, which are shallow-water deposits, corresponding apparently to small portions only of Cambrian time. The physical conditions of the Mississippi area appear to have been similar to that of the Adirondack region. As seen to the west of Lake Superior, the lowest Potsdam beds of Hall rest unconformably upon the great Keweenian or copper-bearing series. This, although containing, as the speaker had elsewhere shown, some evidence of organic life, probably worm-burrows and sponges, cannot be claimed for the Cambrian till it shall have been shown to contain a Cambrian fauna.

In this north-western area we find, moreover, beneath the Cambrian horizon, representatives of the Laurentian and of the Norian; the latter in the typical norites, or so-called gabbros, which, near Duluth, are directly overlaid by the Keweenian, elsewhere resting on Laurentian or on Huronian rocks. There are also found in Wisconsin petrosilex-rocks of Arvonian type, with quartzites, rising from beneath the Cambrian sandstones. Typical Montalban and Huronian rocks also occur around this area, besides the group which the speaker long since called Animikie, This great series, of many thousand feet, which is overlaid unconformably by the Cambrian sandstones, and also, according to Irving, by the Keweenian, consists chiefly of quartzites and argillites, with beds of magnetite. The remains of a sponge have been detected in a calcareous mass got by the speaker at Thompson, Minnesota, from the argillites of this series, which, in a late communication to the National Academy of Sciences, he has referred to the Lower Taconic or Taconian horizon. He now stated his conclusion, drawn from various facts, that, while these Animikie rocks rest unconformably in this region upon the Huronian, the two series have hitherto been confounded under the common name of Huronian.

[The Cambrian areas of the Atlantic coast, as seen in Massachusetts, New Brunswick and Newfoundland, not being included in the great North American basin have been omitted in the present notice. As regards the Cambrian of the Cordillera area, the studies of Arnold Hague and C. D. Wolcott in the Eureka District in Nevada, just published in the Third Annual Report of the U. S. Geological Survey, show that the paleozoic column to the top of the coal measures, as there displayed, includes, not less than 30,000 feet, marked by only one stratigraphical break, which occurs in the rocks of the second fauna. The series begins with the Prospect Hill quartzite, which, although its base is not seen, shows a thickness of 1500 feet. This is succeeded by 3000 feet of more or less magnesian limestone known by the same local name, followed by the Secret Canon shales, the Hamburg limestones and the Hamburg shales, making, in all, a series of nearly 8000 feet which are referred to the Cambrian.

The first organic remains in this series are met with in what are described as the Olenellus shales, which lie between the basal quartzite and its overlying limestones, and contain a fauna closely related to that of the slates of Georgia, in Vermont, called Lower Potsdam by Billings. In the succeeding limestones and slates already named there is found at various horizons an abundant fauna resembling that of the Potsdam of the Mississippi area. We have thus in the Cordilleras, in conformable succession, the Lower and Upper Potsdam divisions of the east. The forms of the first fauna, pass upwards from the Hamburg slates ome distance into the great overlying Pogonip limestone, which is 2700 feet in thickness, and contains higher up a fauna compared with that of the Chazy, together with some forms characteristic of the Trenton, the succession thus showing a gradual passage from the first to the second fauna.]

For the better understanding of the above remarks, and of the relation of the American Cambrian in its eastern area to the Ordovician and Silurian above and the Taconian and still older Eozoic series below, we append a tabular view from the author's memoir on *The Taconic Question, Part I.*, recently published in the first volume of the Transactions of the Royal Society of Canada. In this table are seen the classification and nomenclature proposed by Amos Eaton half a century since, together with the more recent names. The Potsdam sandstone, which in parts of the Adirondack area forms the base of the Cambrian, had not at that time been recognized.

(*See next page.*)

Classification and Nomenclature of North American Rocks.

Eaton's Nomenclature (1832.)				Later Names.	
III. Lower Secondary.	3	Corniferous Lime-rock		Upper Helderberg	**Devonian.**
		Geodiferous Lime-rock		Niagara	**Silurian.**
	2.	Millstone-grit	**Second Graywacke.**	Oneida—Medina	
	1.	Graywacke slate		Utica—Loraine	**Ordovician.** (Upper Cambrian.)
II. Transition.	3.	Metalliferous Lime-rock		Chazy—Trenton	
		Sparry Lime-rock. Calcifer. Sand-rock.			
	2.	Millstone-grit	**First Graywacke.**	**Upper Taconic.** (Quebec Group.)	**Cambrian.** (Middle and Lower Cambrian)
		Graywacke-slate			
	1.	Argillite		**Lower Taconic.**	**Taconian.**
I. Primitive.	3.	Granular Lime-rock			
	2.	Granular Quartz-rock			
	1.	Gneiss and other Crystalline Rocks		**Huronian. Montalban. Laurentian. Norian. Arvonian.**	

IV. The Eozoic Rocks of North America.*

By T. Sterry Hunt, LL.D., F.R.S.

According to the author there is found among the pre-Cambrian strata of North America an invariable succession of crystalline stratified rocks, which have been by him divided into several great groups, the constituents of which become progressively less massive and less crystalline until we reach the sediments of paleozoic time, of which the Cambrian is regarded as the basal member. Since all of these pre-Cambrian rocks, with the exception, perhaps, of the lowest or fundamental gneiss, present evidences, direct or indirect, of the existence of organic life at the time of their deposition, it seems proper to include them under the general title of Eozoic, proposed by Sir J. W. Dawson. That of Archæan, employed by some geologists to designate these pre-Cambrian rocks, appears too indefinite in its signification, and, moreover, is not in accordance with the nomenclature generally adopted for the great divisions succeeding. These Eozoic rocks include both the Primitive and the Transition divisions of Werner.

We distinguish at the base of the Eozoic series a massive and essentially granitoid gneiss. To this fundamental rock, sometimes called the Ottawa gneiss, and of unknown thickness, succeeds what has been named in Canada the Grenville gneissic series, made up in great part of gneiss somewhat similar to that last mentioned, with intercalations of hornblendic gneiss, of quartzite, of pyroxenite, of serpentine, of magnetite, and of crystalline limestones, the latter often magnesian, occasionally graphitic, and sometimes attaining thicknesses of a thousand feet or more. This Grenville series, the strata of which are generally highly inclined, has an aggregate volume of not less than 15,000 or 20,000 feet, and appears to rest unconformably upon the fundamental or Ottawa gneiss. This gneissic series, with its intercalated limestones, some of which contain *Eozoön canadense*, was the typical Laurentian of Logan and Hunt, named by them in 1854, with which they included, at that time, however, not only the underlying fundamental gneiss, but an upper granitoid and gneissoid series,

* Abstract of a paper read before the British Association for the Advancement of Science, Montreal, Sept. 1, 1884.

composed in large part of plagioclase feldspars, chiefly labradorite. These three divisions of the Eozoic system were thus confounded under the common name of Laurentian until, in 1862, the last was separated under the provisional name of Upper Laurentian, the two other divisions united being called Lower Laurentian. The synonym of Labradorian was subsequently, for a time, employed by Logan to designate the upper division, until 1870, when the present writer proposed for it the name of Norian, retaining that of Laurentian for the two lower divisions. It will probably be found desirable to separate the typical Laurentian or Grenville series, as studied and mapped by Logan, Hunt and Dawson, from the less-known fundamental or Ottawa gneiss, and to make of this latter a distinct group. The name of Middle Laurentian, sometimes given to the typical Laurentian, loses its significance with the disappearance of that of Upper Laurentian, now replaced by Norian.

The Norian series is made up in great part of granitoid or gneissoid rocks, composed essentially of plagioclase feldspars, without quartz but with a little pyroxene or hypersthene, often with titanic iron ore, and apparently identical with the norites of Norway. With these rocks are, however, found alternations of gneiss, of quartzite and of crystalline limestone, scarcely different from those of the Laurentian. We therein find also a granitoid rock made up of pink orthoclase and quartz, with bluish labradorite. This Norian series is found in many places covering considerable areas, and apparently resting in discordant stratifications upon the typical Laurentian. Its thickness has been estimated at over 10,000 feet.

There is found in certain localities a series of stratified rocks, composed essentially of petrosilex or hälleflinta, often passing into a quartziferous porphyry. There are found with it strata of vitreous quartzite, and thin layers of soft micaceous schists, besides great beds of hematite, and more rarely layers of crystalline limestone. This group, which has a thickness of many thousand feet was at first included by the writer in the lower part of the succeeding Huronian series, which, however, apparently overlies it unconformably. Its relations with the preceding groups have not been observed, but, as it appears to be identical, both in position and in character, with what, in 1878, was first called Arvonian in Wales, we designate it by that name.

Next in order comes the group to which the writer, in 1855, gave the name of Huronian. It differs from those preceding by the frequent presence of schistose rocks, and of conglomerates which contain fragments of the underlying gneisses. These characters, which are common to the Huronian and the two succeeding groups, led some earlier geologists in America to include them among Transition rocks. The Huronian contains a considerable proportion of epidote, hornblende and pyroxene, and is marked by varieties of diabasic rocks often called gabbros, which are truly stratified, but are not to be confounded with the norites of the Norian series, to which the name of gabbro is also frequently given. The Huronian series moreover includes imperfect gneisses, quartzites, dolomite, serpentine and steatite, besides large amount of chloritic, micaceous and argillaceous schists. Its thickness is estimated at about 18,000 feet, and it is often found resting unconformably upon the gneiss of the Laurentian. The Huronian appears to be identical with much of what has since been called Pebidian in the British Isles, and with the true *pietri verdi* group of the Alps, there found in many parts between the ancient gneisses below and a younger series of gneisses and mica-schists.

There is met with in North America a similar series to these, to which, in 1870, the writer gave the name of Montalban, because it is found largely developed in the White Mountains of New England. This series contains fine-grained white gneisses, sometimes porphyritic, but distinct from the granitoid gneisses of the Laurentian, and passing into granulites on the one hand, and into very quartzose coarse-grained mica-schists on the other. It also includes hornblendic gneisses and black hornblende-schists, together with serpentine, chrysolite rocks, dichroite-gneiss and crystalline limestones. The mica-schists of the series often contain garnets, andalusite, fibrolite, cyanite, and staurolite; while in the granitic veins which traverse the series are found tourmaline, beryl and cassiterite. The total thickness of the Montalban is apparently much greater than that assigned to the Huronian, upon which it sometimes rests unconformably, or, as is often the case in the absence of this, directly upon the Laurentian. We come next to a series composed essentially of quartzites, limestones, and micaceous and argillaceous schists. The quartzites, occasionally conglomerate, are sometimes vitreous, sometimes granular and often micaceous, passing into mica-schists very distinct from those of the Montalban. The mica is often damourite or sericite, and gives rise to unctuous glossy schists passing into argillites, which sometimes contain a feldspathic admixture. The limestones of this series, often magnesian, are crystalline and include statuary marbles and cipolinos. We find in the schists, which are intercalated alike among the quartzite and the limestones of this series, masses of serpentine and of ophicalcite, and occasionally of chloritic and hornblendic minerals, as well as siderite, magnetite and hematite, the iron-oxyds being often mingled with the quartzites. These last are sometimes flexible and elastic, and the whole series much resembles the Itacolumitic group of Brazil. It has a thickness in different parts of North America of from 4,000 to 10,000 feet, and is found lying unconformably alike upon the Laurentian, the Huronian and the Montalban. There are found in the quartzites of this series the impressions of Scolithus, and in the limestones other undetermined forms. This is the Lower Taconic series of the late Dr. Emmons, which we distinguish by the name of Taconian. Some late writers have by mistake confounded it with the Upper Taconic of the same author, a distinct group, which Emmons declared to be the equivalent of the primordial (Cambrian) of Barrande. The Taconian is widely spread in eastern North America, and appears to be also represented around Lake Superior by what has been called the Animikie series. There is reason to believe that, on account of certain lithological resemblances between the Taconian and the Huronian, the two have been in some localities confounded, and that portions which have been called Huronian are really Taconian. The latter, the writer has elsewhere compared with a great series of similar schists and quartzites, including serpentine, anhydrite, dolomite and marbles, greatly developed in northern Italy, where it overlies the younger gneissic and mica-schists series, and has been by various observers successsively referred to the mesozoic, the paleozoic and the eozoic periods.

It now remains to say something upon the relations of these different crystalline eozoic series to the Cambrian which succeeds them. Forty years since there were two schools among American geologists. One of these schools admitted the existence, between the ancient gneissoid rocks (subsequently named Laurentian and

Norian) and the fossiliferous limestones of the second fauna (Ordovician), of nothing more than those Cambrian subdivisions known as the Potsdam sandstone and the Calciferous sandrock, which in the vicinity of the Adirondack Mountains separate these ancient crystalline rocks from the Ordovician strata.

The other school of stratigraphists recognized the existence, in this interval, in the region to the east of Lake Champlain, of several series of crystalline rocks, including what we have already described under the names of Arvonian, Huronian, Montalban and Taconian, besides a younger series of uncrystalline sediments of great thickness, designated by Eaton as the First or Transition Graywacke. This was by him declared to be separated by the limestones of the second fauna from the Secondary Graywacke, a series closely resembling the Transition Graywacke. The first-mentioned school denied the existence of the Transition Graywacke, and maintained that the group thus designated was identical with the Secondary Graywacke. The geologists of this school further supposed that all the different series of crystalline rocks just named were nothing more than the same Secondary Graywacke, with the addition of the underlying fossiliferous limestones, in a state of alteration more or less profound; the series in different areas assuming successively the character of Taconian, Montalban, Huronian, and even, as some imagined, of Laurentian.

The recent progress of American stratigraphy has fully justified the views of the second named and older school, that of Eaton. It has been shown that the First or Transition Graywacke of this author, which was the Upper Taconic of Emmons, and includes the primordial or Cambrian fauna, rests in unconformable stratification upon the various crystalline series named, and that all of these great groups belong to Eozoic time. The Quebec group of Logan, as well as what he called the Potsdam group, is this same Cambrian or Transition Graywacke. The Hudson River group also, as first described by Vanuxem and by Mather, and, later, by Logan up to 1860 (when he proposed for it the name of the Quebec group) is nothing else than this same Cambrian Graywacke, with the addition, in certain localities, of a portion of Laconian, and in others of schistose beds containing the second fauna (Utica and Loraine shales). The above explanation becomes necessary for the reason that the Canadian geologists

(Logan and the present writer) formerly described, in accordance with the views of the first-named school, certain crystalline schists chiefly Huronian, as altered rocks of the Hudson River group, and later (from 1860 to 1867) as of the Quebec group.

The cupriferous series of the basin of Lake Superior (the distinctness of which was maintained by the writer in 1873, when he called it the Keweenaw group, a name which he subsequently changed to Keweenian), which has a thickness probably greatly exceeding 20,000 feet, was also by Logan referred to the Quebec group. It has, however, been shown by later observers that the fossiliferous sandstones which rest in horizontal layers upon the inclined strata of the Keweenian, belong to the Cambrian, and hold the fauna of the Potsdam. The conglomerates of the Keweenian cupriferous series contain portions alike of Laurentian Arvonian, Huronian and Montalban rocks, and appear, according to the latest observations, to overlie the schists which we have referred to the Taconian. The sandstones and argillites of the Keweenian, which are interstratified with great masses of melaphyre, are uncrystalline. It remains to be determined whether the intermediate Keweenian series has greater affinities with the Taconian than with the Cambrian.

We have thus sought to include provisionally the whole vast system of Primitive and Transition crystalline rocks, from the fundamental granitoid gneiss upward, under the names of Laurentian, Norian, Arvonian, Huronian, Montalban and Taconian, The Arvonian or petrosilex group intervenes between the Laurentian and the Huronian, but the peculiar characters of the Norian, and its localization to some few limited areas in Europe and North America, make it difficult for us, as yet, to define its precise relations to the Arvonian. The Norian however, probably, like the Arvonian, occupies a horizon between Laurentian and Huronian. Much time may pass, and many stratigraphical studies must be made before the precise relations of the Huronian and the succeeding Montalban can be defined. It seems probable in the present state of our knowledge that the Montalban series was, in many cases, deposited over areas where the Huronian had never been laid down. Notwithstanding the great geographical extent, and the importance of these two series, neither can claim that universality which probably belonged to the primitive granitic substratum, a universality soon interrupted by the appearance of dry land, which preceded Huronian time.

V. Address before the British Association.

Montreal, August 27, 1884.

By the Right Hon. Lord Rayleigh, M.A., D.C.L., F.R.S., President.

Ladies and Gentlemen,—It is no ordinary meeting of the British Association which I have now the honor of addressing. For more than fifty years the Association has held its autumn gathering in various towns of the United Kingdom, and within those limits there is, I suppose, no place of importance which we have not visited. And now, not satisfied with past successes, we are seeking new worlds to conquer. When it was first proposed to visit Canada, there were some who viewed the project with hesitation. For my own part I never quite understood the grounds of their apprehension. Perhaps they feared the thin edge of the wedge. When once the principle was admitted, there was no knowing to what it might lead. So rapid is the development of the British empire, that the time might come when a visit to such out-of-the-way places as London or Manchester could no longer be claimed as a right, but only asked for as a concession to the susceptibilities of the English. But, seriously, whatever objections may have at first been felt soon were outweighed by the consideration of the magnificent opportunities which your hospitality affords of extending the sphere of our influence and of becoming acquainted with a part of the Queen's dominions which, associated with splendid memories of the past, is advancing daily, by leaps and bounds, to a position of importance such as not long ago was scarcely dreamed of. For myself, I am not a stranger to your shores. I remember well the impression made upon me, seventeen years ago, by the wild rapids of the St. Lawrence, and the gloomy grandeur of the Saguenay. If anything impressed me more, it was the kindness with which I was received by yourselves, and which I doubt not will be again extended, not merely to myself but to all the English members of the Association. I am confident that those who have made up their minds to cross the ocean will not repent their decision, and that, apart altogether from scientific interests, great advantage may be expected from this visit. We Englishmen ought to know more than we do of matters relating to the colonies, and anything which tends to bring the various parts of the empire into closer contact can hardly be overvalued. It is pleasant to think that this Association is the means of furthering an object which should be dear to the hearts of all of us; and I venture to say that a large proportion of the visitors to this country will be astonished by what they see, and will carry home an impression which time will not readily efface.

To be connected with this meeting is, to me, a great honour, but also a great responsibility. In one respect, especially, I feel that the Association might have done well to choose another President. My own tastes have led me to study mathematics and physics rather than geology and biology, to which naturally more attention turns in a new country, presenting, as it does, a fresh field for investigation. A chronicle of achievements in these departments by workers from among yourselves would have been suitable to the occasion, but could not come from me. If you would have preferred a different subject for this address, I hope, at least, that you will not hold me entirely responsible.

At annual gatherings like ours, the pleasure with which friends meet friends again is sadly marred by the absence of those who can never more take their part in our proceedings. Last year my predecessor in this office had to lament the untimely loss of Spottiswoode and Henry Smith, dear friends of many of us, and prominent members of our Association. And now again, a well-known form is missing. For many years Sir W. Siemens has been a regular attendant at our meetings, and to few, indeed, have they been more indebted for success. Whatever the occasion, in his presidential address of two years ago, or in communications to the Physical and Mechanical Sections, he had always new and interesting ideas, put forward in language which a child could understand, so great a master was he of the art of lucid statement in his adopted tongue. "Practice with Science" was his motto. Deeply engaged in industry, and conversant, all his life, with engineering operations, his opinion was never that of a mere theorist. On the other hand, he abhorred rule of thumb, striving always to master the scientific principles which underlie rational design and invention.

It is not necessary that I should review in detail the work of Siemens. The part which he took, during recent years, in the development of the dynamo machine must be known to many of you. We owe to him the practical adoption of the method, first suggested by Wheatstone, of throwing into a shunt the coils of the field magnets, by which a greatly improved steadiness of action is obtained. The same characteristics are observable throughout—a definite object in view and a well directed perseverance in overcoming the difficulties by which the path is usually obstructed.

These are, indeed, the conditions of successful invention. The world knows little of such things, and regards the new machine or the new method as the immediate outcome of a happy idea. Probably, if the truth were known, we should see that, in nine cases out of ten, success depends as much upon good judgment and perseverance as upon fertility of imagination. The labors of our great inventors are not unappreciated, but I doubt whether we adequately realize the enormous obligations under which we lie. It is no exaggeration to say that the life of such a man as Siemens is spent in the public service; the advantages which he reaps for himself being as nothing in comparison with those which he confers upon the community at large.

As an example of this it will be sufficient to mention one of the most valuable achievements of his active life—his introduction, in conjunction with his brother, of the regenerative gas furnace, by which an immense economy of fuel (estimated at millions of tons annually) has been effected in the manufacture of steel and glass. The nature of this economy is easily explained. Whatever may be the work to be done by the burning of fuel, a certain temperature is necessary. For example, no amount of heat in the form of boiling water, would be of any avail for the fusion of steel. When the products of combustion are cooled down to the point in question, the heat which they still contain is useless as regards the purpose in view. The importance of this consideration depends entirely upon the working-temperature. If the object be the evaporation of water or the warming of a house, almost all the heat may be extracted from the fuel without special arrangements. But it is otherwise when the temperature required is not much below that of combustion itself, for then the escaping gases carry away with them the larger part of the whole heat developed. It was to meet this difficulty that the regenerative furnace was devised. The products of combustion, before dismissal into the chimney, are caused to pass through piles of loosely stacked fire-brick, to which they give up their heat. After a time the fire-brick upon which the gases first impinge, becomes nearly as hot as the furnace itself. By suitable valves the burnt gases are then diverted through another stack of brickwork, which they heat up in like manner, while the heat stored up in the first stack is utilized to warm the unburnt gas and air on their way to the furnace. In this way almost all the heat developed at a high temperature during the combustion is made available for the work in hand.

As it is now several years since your presidential chair has been occupied by a professed physicist, it may naturally be expected that I should attempt some record of recent progress in that branch of science, if indeed such a term be applicable. For it is one of the difficulties of the task that subjects as distinct as Mechanics, Electricity, Heat, Optics and Acoustics, to say nothing of Astronomy and Meteorology, are included under Physics. Any one of these may well occupy the life-long attention of a man of science, and to be thoroughly conversant with all of them is more than can be expected of any one individual, and is probably incompatible with the devotion of much time and energy to the actual advancement of knowledge. Not that I would complain of the association sanctioned by common parlance; a sound knowledge of at least the principles of general Physics is necessary to the cultivation of any department. The prodominance of the sense of sight, as the medium of communication with the outer world, brings with it dependence upon the science of Optics; and there is hardly a branch of science in which the effects of temperature have not (often without much success) to be reckoned with. Besides, the neglected borderland between two branches of knowledge is often that which best repays cultivation; or, to use a metaphor of Maxwell's, the greatest benefits may be derived from a cross fertilization of the sciences. The wealth of materia is an evil only from the point of view of one of whom too much may be expected. Another difficulty incident to the task, which must be faced but cannot be overcome, is that of estimating rightly the value, and even the correctness of recent work. It is not always that which seems at first the most important that proves in the end to be so. The history of science teems with examples of discoveries which attracted little notice at the time, but afterwards have taken root downwards and borne much fruit upwards.

One of the most striking advances of recent years is in the production and application of electricity upon a large scale—a subject to which I have already had occasion to allude in connection with the work of Sir W. Siemens. The dynamo machine is indeed founded upon discoveries of Faraday, now more than half a century old; but it has required the protracted labors of many inventors to bring it to its present high degree of efficiency. Looking back at the matter, it seems strange that progress should have been so slow. I do not refer to details of design, the elaboation of which must always, I suppose, require the experience of actual work to indicate what parts are structually weaker than they should be, or are exposed to undue wear and tear. But with regard to the main features of the problem, it would almost seem as if the difficulty lay in want of faith. Long ago it was recognized that electricity derived from chemical action is (on a large scale) too expensive a source of mechanical power, notwithstanding the fact that (as proved by Joule in 1846) the conversion of electrical into mechanical work can be effected with great economy. From this it is an evident consequence that electricity may advantageously be obained from mechanical power; and one cannot help thinking that if the fact had been borne steadily in mind, the development of the dynamo might have been much more rapid. But discoveries and inventions are apt to appear obvious when regarded from the standpoint of accomplished fact; and I draw attention to the matter only to point the moral that we do well to push the attack persistently when we can be sure beforehand that the obstacles to be overcome are only difficulties of contrivance, and that we are not vainly fighting unawares against a law of Nature.

The present development of electricity on a large scale depends, however, almost as much upon the incandescent lamp as upon the dynamo. The success of these lamps demands a very perfect vacuum—not more than about one millionth of the normal quantity of air should remain—and it is interesting to recall that, twenty years ago, such vacua were rare even in the laboratory of the physicist. It is pretty safe to say that these wonderful results would never have been accomplished had practical applications alone been in view. The way was prepared by an army of scientific men whose main object was the advancement of knowledge, and who could scarcely have imagined that the processes which they elaborated would soon be in use on a commercial scale and entrusted to the hands of ordinary workmen.

When I speak in hopeful language of practical electricity, I do not forget the disappointment within the last year or two of many over sanguine expectations. The enthusiasm of the inventor and promotor are necessary to progress, and it seems to be almost a law of nature that it should overpass the bounds marked out by reason and experience. What is most to be regretted is the advantage taken by speculators of the often uninstructed interest felt by the public in novel schemes by which its imagination is fired. But looking forward to the future of electric lighting, we have good ground for encouragement. Already the lighting of large passenger ships is an assured success, and one which will be highly appreciated by those travellers who have experienced the tedium of long winter evenings unrelieved by adequate illumination. Here, no doubt, the conditions are in many respects especially favorable. As regards space, life on board ship is highly concentrated, while unity of management and the presence on the spot of skilled engineers obviate some of the difficulties that are met with under other circumstances. At present we have no experience of a house-to-house system of illumination on a great scale and in competition with cheap gas; but preparations are already far advanced for trial on an adequate scale in London. In large institutions, such as theatres and factories, we all know that electricity is in successful and daily extending operation.

When the necessary power can be obtained from the fall of water instead of from the combustion of coal, the conditions of the problem are far more favorable. Possibly the severity of your winters may prove an obstacle, but it is impossible to regard your splendid river without the thought arising that the day may come when the vast powers now running to waste shall be bent into your service. Such a project demands of course the most careful consideration, but it is one worthy of an intelligent and enterprising community.

The requirements of practice react in the most healthy manner upon scientific electricity. Just as in former days the science received a stimulus from the application to telegraphy, under which everything relating to measurement on a small scale acquired an importance and development for which we might otherwise have had long to wait, so now the requirements of electric lighting are giving rise to a new development of the art of measurement upon a large scale, which cannot fail to prove of scientific as well as practical importance. Mere change of scale may not at first appear a very important matter, but it is surprising how much modification it entails in the instruments, and in the processes of measurement. For instance, the resistance coils on which the electrician relies in dealing with currents whose maximum is a fraction of an ampere, fail altogether when it becomes a question of hundreds, not to say thousands, of ampers.

The powerful currents, which are now at command, constitute almost a new weapon in the hands of the physicist. Effects, which in old days were rare and difficult of observation, may now be produced at will on the most conspicuous scale. Consider for a moment Faraday's great discovery of the "Magnetization of Light," which Tyndall likens to the Weisshorn among mountains, as high, beautiful, and alone. This judgment (in which I fully concur) relates to the scientific aspect of the discovery, for to the eye of sense nothing could have been more insignificant. It is even possible that it might have eluded altogether the penetration of Faraday, had he not been provided with a special quality of very heavy glass. At the present day these effects may be produced upon a scale that would have delighted their discoverer, a rotation of the plane of polarization through 180° being perfectly feasible. With the aid of modern appliances Kundt and Röntgen in Germany, and H. Becquerel in France, have detected the rotation in gases and vapours, where, on account of its extreme smallness, it had previously escaped notice.

Again, the question of the magnetic saturation of iron has now an importance entirely beyond what it possessed at the time of Joule's early observations. Then it required special arrangements purposely contrived to bring it into prominence. Now in every dynamo machine, the iron of the field-magnets approaches a state of saturation, and the very elements of an explanation of the action require us to take the fact into account. It is indeed probable that a better knowledge of this subject might lead to improvements in the design of these machines.

Notwithstanding the important work of Rowland and Stoletow, the whole theory of the behaviour of soft iron under varying magnetic conditions is still somewhat obscure. Much may be hoped from the induction-balance of Hughes, by which the marvellous powers of the telephone are applied to the discrimination of the properties of metals as regards magnetism and electric conductivity.

The introduction of powerful alternate currents, in machines by Siemens, Gordon, Ferranti, and others, is likely also to have a salutary effect in educating those so-called practical electricians whose ideas do not easily rise above ohms and volts. It has long been known that when the changes are sufficiently rapid, the phenomena are governed much more by induction, or electric inertia, than by mere resistance. On this principle much may be explained that would otherwise seem paradoxical. To take a comparatively simple case, conceive an electro-magnet wound with two contiguous wires, upon which acts a given rapidly periodic electro-motive force. If one wire only be used, a certain amount of heat is developed in the circuit. Suppose now that the second wire is brought into operation in parallel—a proceeding equivalent to doubling the section of the original wire. An electrician accustomed only to constant currents would be sure to think that the heating effect would be doubled by the change, as much heat being developed in each wire separately as was at first in the single wire. But such a conclusion would be entirely erroneous. The total current, being governed practically by self-induction of the circuit, would not be augmented by the accession of the second wire, and the total heating effect, so far from being doubled, would, in virtue of the superior conductivity, be halved.

During the last few years much interest has been felt in the reduction to an absolute standard of measurements of electromotive force, current, resistance, etc., and to this end many laborious investigations have been undertaken. The subject is one that has engaged a good deal of my own attention, and I should naturally have felt inclined to dilate upon it, but that I feel it to be too abstruse and special to be dealt with in detail upon an occasion like the present. As regards resistance, I will merely remind you that the recent determinations have shown a so greatly improved agreement, that the Conference of Electricians assembled at Paris, in May, felt themselves justified in defining the ohm for practical use as the resistance of column of mercury of 0 ° C., one square millimetre in section, and 106 centmetres in length—a definition differing by a little more than one per cent. from that arrived at twenty years ago by a committee of this Association.

A standard of resistance once determined upon can be embodied in a "resistance coil," and copied without much trouble, and with great accuracy. But in order to complete the electrical system, a second standard of some kind is necessary, and this is not so easily embodied in a permanent form. It might conveniently consist of a standard galvanic cell capable of being prepared in a definite manner, whose electro-motive force is once for all determined. Unfortunately, most of the batteries in ordinary use are for one reason or another unsuitable for this purpose, but the cell introduced by Mr. Latimer Clark, in which the metals are zinc in contact with saturated zinc-sulphate and pure mercury in contact with mercurous sulphate, appears to give satisfactory results. According to my measurements, the electro-motive force of this cell is 1,435 theoretical volts.

We may also conveniently express the second absolute electrical measurement necessary to the completion of the system by taking advantage of Faraday's law that the quantity of metal decomposed in an electrolytic cell is proportional to the whole quantity of electricity that passes. The best metal for the purpose is silver deposited from a solution of the nitrate or of the chlorate. The results recently obtained by Professor Kohlrauseh and by myself are in very good agreement, and the conclusion that one ampere flowing for one hour decomposes 4.025 grains of silver, can hardly be in error by more then a thousandth part. This number being known, the silver-voltameter gives a ready and very accurate method of measuring currents of intensity, varying from 1-10 ampere to four or five amperes.

The beautiful and mysterious phenomena attending the discharge of electricity in nearly vacuous spaces have been investigated and in some degree explained by De la Rue, Crookes, Shuster, Moulton, and the lamented Spottiswoode, as well as by various able foreign experimenters. In a recent research Crookes has sought the origin of a bright citron-colored band in the phosphorescent spectrum of certain earths, and, after encountering difficulties and anomalies of a most bewildering kind, has succeeded in proving that it is due to yttrium, an element much more widely distributed than had been supposed. A conclusion like this is stated in a few words, but those only who have undergone similar experience are likely to appreciate the skill and perseverance of which it is the final reward.

A remarkable observation by Hall of Baltimore, from which it appeared that the flow of electricity in a conducting sheet was disturbed by magnetic force, has been the subject of much discussion. Mr. Shelford Bidwell has brought forward experiments tending to prove that the effect is of a secondary character, due in the first instance to the mechanical force operating upon the conductor of an electric current when situated in a powerful magnetic field. Mr. Bidwell's view agrees, in the main, with Mr. Hall's division of the metals into two groups according to the direction of the effect.

Without doubt the most important achievement of the older generation of scientific men has been the establishment and application of the great laws of Thermo-dynamics, or, as it is often called, the Mechanical Theory of Heat. The first law, which asserts that heat and mechanical work can be transformed one into the other at a certain fixed rate, is now well understood by every student of physics, and the number expressing the mechanical equivalent of heat resulting from the experiments of Joule, has been confirmed by the researches of others, and especially of Rowland. But the second law, which practically is even more important than the first, is only now beginning to receive the full appreciation due to it. One reason of this may be found in a not unnatural confusion of ideas. Words do not always lend themselves readily to the demands that are made upon them by a growing science, and I think that the almost unavoidable use of the word "equivalent" in the statement of the first law is partly responsible for the little attention that is given to the second.

For the second law so far contradicts the usual statement of the first, as to assert that equivalents of heat and work are not of equal value. While work can always be converted into heat, heat can only be converted into work under certain limitations. For every practical purpose the work is worth the most, and when we speak of equivalents, we used the word in the same sort of special sense as that in which chemists speak of equivalents of gold and iron. The second law teaches us that the real value of heat, as a source of mechanical power, depends upon the temperature of the body in which it resides; the hotter the body in relation to its surroundings, the more available the heat.

In order to see the relations which obtain between the first and the second law of Thermo-dynamics, it is only necessary for us to glance at the theory of the steam-engine. Not many years ago calculations were plentiful, demonstrating the inefficiency of the steam-engine on the basis of a comparison of the work actually got out of the engine with the mechanical equivalent of the heat supplied to the boiler. Such calculations took into account only the first law of Thermo-dynamics, which deals with the equivalents of heat and work, and have very little bearing upon the practical question of efficiency, which requires us to have regard also to the second law. According to that law the fraction of the total energy which can be converted into work depends upon the relative temperatures of the boiler and condenser, and it is, therefore, manifest that, as the temperature of the boiler cannot be raised indefinitely, it is impossible to utilize all the energy which, according to the first law of Thermo-dynamics, is resident in the coal. On a sounder view of the matter, the efficiency of the steam-engine is found to be so high that there is no great margin remaining for improvement. The higher initial temperature possible in the gas-engine opens out much wider possibilities, and many good judges look forward to a time when the steam-engine will have to give way to its younger rival.

To return to the theoretical question, we may say with Sir W. Thompson, that, though energy cannot be destroyed, it ever tends to be dissipated, or to pass from more available to less available forms No one who has grasped this principle can fail to recognize its immense importance in the system of the Universe. Every change—chemical, thermal, or mechanical—which takes or can take place in Nature does so at the cost of a certain amount of available energy. If, therefore, we wish to enquire whether or not a proposed transformation can take place, the question to be considered is whether its occurrence would involve dissipation of energy. If not, the transformation is (under the circumstances of the case) absolutely excluded. Some years ago, in a lecture at the Royal Institution, I endeavored to draw the attention of chemists to the importance of the principle of dissipation in relation to their science, pointing out the error of the usual assumption that a general criterion is to be found in respect of the development of heat. For example, the solution of a salt in water is, if I may be allowed the phrase, a downhill transformation. It involves dissipation of energy, and can therefore go forward; but in many cases it is associated with the absorption rather than with the development of heat. I am glad to take advantage of the present opportunity in order to repeat my recommendation, with an emphasis justified by actual achievement. The foundations laid by Thomson now bear an edifice of no mean proportions, thanks to the labors of several physicists, among whom must be especially mentioned Willard Gibbs and Helmholtz. The former has elaborated a theory of the equilibrium of heterogeneous substances, wide in its principles, and, we cannot doubt, far-reaching in its consequences. In a series of masterly papers Helmholtz has developed the conception of *free energy* with very important applications to the theory of the galvanic cell. He points out that the mere tendency to solution bears in some cases no small proportion to the affinities more usually reckoned chemical, and contributes largely to the total electro-motive force. Also in our own country Dr. Alder Wright has published some valuable experiments relating to the subject.

From the further study of electrolysis we may expect to gain improved views as to the nature of the chemical reactions, and of the forces concerned in bringing them about. I am not qualified —I wish I were—to speak to you on recent progress in general chemistry. Perhaps my feelings towards a first love may blind me, but I cannot help thinking that the next great advance, of which we have already some foreshadowing, will come on this side. And if I might without presumption venture a word of recommendation it would be in favor of a more minute study of the simpler chemical phenomena. . .

Under the head of scientific mechanics it is principally in relation to fluid motion that advances may be looked for. In speaking upon this subject I must limit myself almost entirely to experimental work. Theoretical hydro-dynamics, however important and interesting to the mathematician, are eminently unsuited to oral exposition. All I can do to attenuate an injustice, to which theorists are pretty well accustomed, is to refer you to the admirable reports of Mr. Hicks, published under the auspices of this Association.

The important and highly practical work of the late Mr Froude in relation to the propulsion of ships is doubtless known to most of you. Recognizing the fallacy of views then widely held as to the nature of the resistance to be overcome, he showed to demonstration that, in the case of fair-shaped bodies, we have to deal almost entirely with resistance dependent upon skin-friction and at high speeds upon the generation of surface-waves by which energy is carried off. At speeds which are moderate in relation to the size of the ship, the resistance is practically dependent upon skin-friction only. Although Professor Stokes and other mathematicians had previously published calculations pointing to the same conclusion, there can be no doubt that the view generally entertained was very different. At the first meeting of the Association which I ever attended, as an intelligent listener, at Bath, in 1864, I well remember the surprise which greeted a statement by Rankine that he regarded skin-friction as the only legitimate resistance to the progress of a well-designed ship. Mr. Froude's experiments have set the question at rest in a manner satisfactory to those who had little confidence in theoretical prevision.

In speaking of an explanation as satisfactory in which skin-friction is accepted as the cause of resistance, I must guard myself against being supposed to mean that the nature of skin-friction is itself well understood. Although its magnitude varies with the smoothness of the surface, we have no reason to think that it would disappear at any degree of smoothness consistent with an ultimate molecular structure. That it is connected with fluid viscosity is evident enough, but the *modus operandi* is still obscure.

Some important work bearing upon the subject has recently been published by Professor O. Reynolds, who has investigated the flow of water in tubes as dependent upon the velocity of motion and upon the size of the bore. The laws of motion in capillary tubes, discovered experimentally by Poiseuille, are in complete harmony with theory. The resistance varies as the velocity, and depends in a direct manner upon the constant of viscosity. But when we come to the larger pipes and higher velocities with which engineers usually have to deal, the theory which presupposes a regularly stratified motion evidently ceases to be applicable, and the problem becomes essentially identical with that of skin-friction in relation to ship propulsion. Professor Reynolds has traced with much success the passage from the one state of things to the other, and has proved the applicability under these complicated conditions of the general laws of dynamical similarity as adapted to viscous fluids by Professor Stokes. In spite of the difficulties which beset both the theoretical and experimental treatment, we may hope to attain before long to a better understanding of a subject which is certainly second to none in scientific as well as practical interest.

As also closely connected with the mechanics of viscous fluids I must not forget to mention an important series of experiments upon the friction of oiled surfaces, recently executed by Mr. Tower for the Institution of Mechanical Engineers. The results go far towards upsetting some ideas hitherto widely admitted. When the lubrication is adequate the friction is found to be nearly independent of the load, and much smaller than is usually supposed, giving a coefficient as low as 1–1000. When a layer of oil is well formed the pressure between the solid surfaces is really borne by the fluid, and the work lost is spent in shearing, that is in causing one stratum of the oil to glide over another.

In order to maintain its position, the fluid must possess a certain degree of viscosity, proportionate to the pressure; and even when this condition is satisfied it would appear to be necessary that the layer should be thicker on the ingoing than on the outgoing side. We may, I believe, expect from Professor Stokes a further elucidation of the processes involved. In the meantime, it is obvious that the results already obtained are of the utmost value, and fully justify the action of the Institution in devoting a part of its resources to experimental work. We may hope, indeed, that the example thus wisely set may be followed by other public bodies associated with various departments of industry.

I can do little more than refer to the interesting observations of Professor Darwin, Mr. Hunt, and Mr. Forel on Ripplemark. The processes concerned would seem to be of a rather intricate character, and largely dependent upon fluid viscosity. It may be noted, indeed, that most of the still obscure phenomena of hydro-dynamics require for their elucidation a better comprehension of the laws of viscous motion. The subject is one which offers peculiar difficulties. In some problems in which I have lately been interested, a circulating motion presents itself of the kind which the mathematician excludes from the first when he is treating of fluids destitute altogether of viscosity. The intensity of this motion proves, however, to be independent of the coefficient of viscosity, so that it cannot be correctly dismissed from consideration as a consequence of a supposition that the viscosity is infinitely small. The apparent breach of continuity can be explained, but it shows how much care is needful in dealing with the subject, and how easy it is to fall into error.

The nature of gaseous viscosity, as due to the diffusion of momentum, has been made clear by the theoretical and experimental researches of Maxwell. A flat disc moving in its own plane between two parallel solid surfaces is impeded by the necessity of shearing the intervening layers of gas, and the magnitude of the hindrance is proportional to the velocity of the motion and to the viscosity of the gas, so that under similar circumstances this effect may be taken as a measure, or rather definition, of the viscosity. From the dynamical theory of gases, to the development of which he contributed so much, Maxwell drew the startling conclusion that the viscosity of a gas should be independent of its density,—that within wide limits the resistance to the moving disc should be scarcely diminished by pumping the gas, so as to form a partial vacuum. Experiment fully confirmed this theoretical anticipation—one of the most remarkable to be found in the whole history of science, and proved that the swinging disc was retarded by the gas as much when the barometer stood at half an inch as when it stood at thirty inches. It was obvious, of course, that the law must have a limit, that at a certain point of exhaustion the gas must begin to lose its power; and I remember discussing with Maxwell, soon after the publication of his experiments, the whereabouts of the point at which the gas would cease to produce its ordinary effect. His apparatus, however, was quite unsuited for high degrees of exhaustion, and the failure of the law was first observed by Kundt and Warburg, as pressures below 1 mm. of mercury. Subsequently the matter has been thoroughly examined by Crookes, who extended his observations to the highest degrees of exhaustion as measured by MacLeod's gauge. Perhaps the most remarkable results relate to hydrogen. From the atmospheric pressure of 760 mm. down to about $\frac{1}{2}$ mm. of mercury the viscosity is sensibly constant. From this point to the highest vacua, in which less than one-millionth of the original gas remains, the coefficient of viscosity drops down gradually to a small fraction of its original value. In these vacua Mr. Crookes regards the gas as having assumed a different ultra-gaseous condition; but we must remember that the phenomena have relation to the other circumstances of the case, especially the dimensions of the vessel, as well as to the condition of the gas.

Such an achievement as the prediction of Maxwell's law of viscosity has, of course, drawn increased attention to the dynamical theory of gases. The success which has attended the theory in the hands of Clausius, Maxwell, Boltzmann and other mathematicians, not only in relation to viscosity, but over a large part of the entire field of our knowledge of gases, proves that some of its fundamental postulates are in harmony with the reality of nature. At the same time it presents serious difficulties, and we cannot but feel that while the electrical and optical properties of gases remain out of relation to the theory, no final judgment is possible. The growth of experimental knowledge may be trusted to clear up many doubtful points, and a younger generation of theorists will bring to bear improved mathematical weapons. In the meantime we may fairly congratulate ourselves on the possession of a guide which has already conducted us to a position which could hardly otherwise have been attained.

In optics attention has naturally centred upon the spectrum. The mystery attaching to the invisible rays lying beyond the red has been fathomed to an extent that a few years ago would have seemed almost impossible. By the use of special photographic methods Abney has mapped out the peculiarities of this region with such success that our knowledge of it begins to be comparable with that of the parts visible to the eye. Equally important work has been done by Langley, using a refined invention of his own based upon the principle of Siemens' pyrometer. This instrument measures the actual energy of the radiation, and thus expresses the effects of various parts of the spectrum upon a common scale, independent of the properties of the eye and of sensitive photographic preparations. Interesting results have also been obtained by Becquerel, whose method is founded upon a curious action of the ultra-red rays in enfeebling the light emitted by phosphorescent substances. One of the most startling of Langley's conclusions relates to the influence of the atmosphere in modifying the quality of solar light. By the comparison of observations made through varying thicknesses of air, he shows that the atmospheric absorption tells most upon the light of high refrangibility; so that, to an eye situated outside the atmosphere, the sun would present a decidedly bluish tint. It would be interesting to compare the experimental numbers with the law of scattering of light by small particles, given some years ago as the result of theory. The demonstration by Langley of the inadequacy of Cauchy's law of dispersion to represent the relation between the refrangibility and wave-length in the lower part of the spectrum must have an important bearing upon optical theory.

The investigation of the relation of the visible and ultra-violet spectrum to various forms of matter has occupied the attention of a host of able workers, among whom none have been more successful than my colleagues at Cambridge, Professors Liveing and Dewar. The subject is too large both for the occasion and for the individual, and I must pass it by. But, as more closely related to optics proper, I cannot resist recalling to your notice a beautiful application of the idea of Doppler to the discrimination of the origin of certain lines observed in the solar spectrum. If a vibrating body have a general motion of approach or recession the waves emitted from it reach the observer with a frequency which in the first case exceeds, and in the second case falls short of, the real frequency of the vibrations themselves. The consequence is that, if a glowing gas be in motion in the line of sight, the spectral lines are thereby displaced from the position that they would occupy were the gas at rest—a principle which, in the hands of Huggins and others, has led to a determination of the motion of certain fixed stars relatively to the solar system. But the sun is itself in rotation, and thus the position of a solar spectral line is slightly different according as the light comes from advancing or from the retreating limb. This displacement was, I believe, first observed by Thollon: but what I desire now to draw attention to is the application of it by Cornu to determine whether a line is of solar or atmospheric origin. For this purpose a small image of the sun is thrown upon the slit of the spectroscope, and caused to vibrate two or three times a second, in such a manner that the light entering the instrument comes alternately from the advancing and retreating limbs. Under these circumstances a line due to absorption within the sun appears to tremble, as the result of slight alternately opposite displacements. But if the seat of the absorption be in the atmosphere it is a matter of indifference from what part of the sun the light originally proceeds, and the line maintains its position in spite of the oscillation of the image upon the slit of the spectroscope. In this way Cornu was able to make a discrimination which can only otherwise be effected by a difficult comparison of appearances under various solar altitudes.

The instrumental weapon of investigation, the spectroscope itself, has made important advances on the theoretical side. We have for our guidance the law that the optical power in gratings is proportional to the total number of lines accurately ruled, without regard to the degree of closeness, and in prisms that it is proportional to the thickness of glass traversed. The magnificent gratings of Rowland are a new power in the hands of the spectroscopist, and as triumphs of mechanical art seem to be little short of perfection. In our own report for 1882 Mr. Mallock has described a machine constructed by him, for ruling large diffraction-gratings, similar in some respects to that of Rowland.

The great optical constant, the velocity of light, has been the subject of three distinct investigations by Cornu, Michelson, and Forbes. As may be supposed, the matter is of no ordinary difficulty, and it is therefore not surprising that the agreement should be less decided than could be wished. From their observations, which were made by a modification of Fizeau's method of the toothed wheel, Young and Forbes drew the conclusion that the velocity of light *in vacuo* varies from color to color, to such an extent that the velocity of blue light is nearly two per cent. greater than that of red light. Such a variation is quite opposed to existing theoretical notions, and could only be accepted on the strongest evidence. Mr. Michelson, whose method (that of Foucault) is well suited to bring into prominence a variation of velocity with wave-length, informs me that he has recently repeated his experiments, with special reference to the point in question, and has arrived at the conclusion that no variation exists comparable with that asserted by Young and Forbes. The actual velocity differs little from that found from his first series of experiments, and may be taken to be 299,800 kilometres per second.

It is remarkable how many of the playthings of our childhood give rise to questions of the deepest scientific interest. The top is or may be understood, but a complete comprehension of the kite and of the soap-bubble would carry us far beyond our present stage of knowledge. In spite of the admirable investigations of Plateau, it still remains a mystery why soapy water stands almost alone among fluids as a material for bubbles. The beautiful development of color was long ago ascribed to the interference of light, called into play by the gradual thinning of the film. In accordance with this view the tint is determined solely by the thickness of the film and the refractive index of the fluid. Some of the phenomena are however so curious as to have led excellent observers, like Brewster, to reject the theory of thin plates, and to assume the secretion of various kinds of coloring matter. If the rim of a wine-glass be dipped in soapy water, and then held in a vertical position, horizontal bands soon begin to show at the top of the film, and extend themselves gradually, downwards. According to Brewster these bands are not formed by the "subsidence and gradual thinning of the film," because they maintain their horizontal position when the glass is turned round its axis. The experiment is both easy and interesting; but the conclusion drawn from it cannot be accepted. The fact is that the various parts of the film cannot quickly alter their thickness, and hence when the glass is rotated they re-arrange themselves in order of superficial density, the thinner parts floating up over, or through the thicker parts. Only thus can the tendency be satisfied for the centre of gravity to assume the lowest possible position.

When the thickness of a film falls below a small fraction of the length of a wave of light, the color disappears and is replaced by an intense blackness. Professors Reinold and Rücker have recently made the remarkable observation that the whole of the black region soon after its formation is of uniform thickness, the passage from the black to the colored portions being exceedingly abrupt. By two independent methods they have determined the thickness of the black film to lie between seven and fourteen millionths of a millimetre; so that the thinnest films correspond to about one-seventieth of a wave-length of light. The importance of these results in regard to molecular theory is too obvious to be insisted upon.

The beautiful inventions of the telephone and the phonograph, although in the main dependent upon principles long since established, have imparted a new interest to the study of acoustics. The former, apart from its uses in every-day life, has become in the hands of its inventor, Graham Bell, and of Hughes, an instrument of first-class scientific importance. The theory of its action is still in some respects obscure, as is shown by the comparative failure of the many attempts to improve it. In connection with some explanations that have been offered, we do well to remember that molecular change in solid masses are inaudible in themselves, and can only be manifested to our ears by the generation of a to-and-fro motion of the external surface extending over a sensible area. If the surface of a solid remains undisturbed, our ears can tell us nothing of what goes on in the interior.

In theoretical acoustics progress has been steadily maintained, and many phenomena which were obscure twenty or thirty years ago have since received adequate explanation. If some important practical questions remain unsolved, one reason is that they have not yet been definitely stated. Almost everything in connection with the ordinary use of our senses presents peculiar difficulties to scientific investigation. Some kinds of information with regard to their surroundings are of such paramount importance to successive generations of living beings, that they have learned to interpret indications which, from a physical point of view, are of the slenderest character. Every day we are in the habit of recognising, without much difficulty, the quarter from which a sound proceeds, but by what steps we attain that end has not yet been satisfactorily explained. It has been proved that when proper precautions are taken we are unable to distinguish whether a pure tone (as from a vibrating tuning-fork held over a suitable resonator) comes to us from in front or from behind. This is what might have been expected from an *a priori* point of view; but what would not have been expected is that with almost any other sort of sound, from a clap of the hands to the clearest vowel sound, the discrimination is not only possible but easy and instinctive. In these cases it does not appear how the possession of two ears helps us, though there is some evidence that it does; and even when sounds come to us from the right or left, the explanation of the ready discrimination which is then possible with pure tone is not so easy as might at first appear. We should be inclined to think that the sound was heard much more loudly with the ear that is turned towards than with the ear that is turned from it, and that in this way the direction was recognized. But if we try the experiment, we find that, at any rate with notes near the middle of the musical scale, the difference of loudness is by no means so very great. The wave-lengths of such notes are long enough in relation to the dimensions of the head to forbid the formation of anything like a sound-shadow in which the averted ear might be sheltered.

In concluding this imperfect survey of recent progress in physics, I must warn you emphatically that much of great importance has been passed over altogether. I should have liked to speak to you of those far-reaching speculations, especially associated with the name of Maxwell in which light is regarded as a disturbance in an electro-magnetic medium. Indeed at one time I had thought of taking the scientific work of Maxwell as the principal theme of this address. But, like most men of genius, Maxwell delighted in questions too obscure and difficult for hasty treatment, and thus much of his work could hardly be considered upon such an occasion as the present. His biography has recently been published, and should be read by all who are interested in science and in scientific men. His many-sided character, the quaintness of his humor, the penetration of his intellect, his simple but deep religious feeling, the affection between son and father, the devotion of husband to wife, all combine to form a rare and fascinating picture. To estimate rightly his influence upon the present state of science, we must regard not only the work that he executed himself, important as that was, but also the ideas and the spirit which he communicated to others. Speaking for myself, as one who in a special sense entered into his labors, I should find it difficult to express adequately my feeling of obligation. The impress of his thoughts may be recognized in much of the best work of the present time. As a teacher and examiner, he was well acquainted with the almost universal tendency of uninstructed minds to elevate phrases above things; to refer, for example, to the principle of the conservation of energy for an explanation of the persistent rotation of a fly-wheel, almost in the style of the doctor in "La Malade Imaginaire," who explains the facts that opium sends you to sleep by its soporific virtue. Maxwell's endeavor was always to keep the facts in the foreground, and to his influence, in conjunction with that of Thomson and Helmholtz, is largely due that elimination of unnecessary hypothesis, which is one of the distinguishing characteristics of the science of the present day.

In speaking unfavorably of superfluous hypothesis, let me not be misunderstood. Science is nothing without generalizations. Detached and ill-assorted facts are only raw material, and, in the absence of a theoretical solvent, have but little nutritive value. At the present time, and in some departments, the accumulation of material is so rapid that there is danger of indigestion. By a fiction as remarkable as any to be found in law, what has once been published, even though it be in the Russian language, is usually spoken of as "known," and it is often forgotten that the re-discovery in the library may be a more difficult and uncertain process than the first discovery in the laboratory. In this matter we are greatly dependent upon annual reports and abstracts, issued principally in Germany, without which the search for the discoveries of a little-known author would be well-nigh hopeless. Much useful work has been done in this direction in connection with our Association. Such critical reports as those upon Hydrodynamics, upon Tides and upon Spectroscopy, guide the investigator to the points most requiring attention, and in discussing past achievements contribute in no small degree to future progress. But though good work has been done, much yet remains to do.

If, as is sometimes supposed, science consisted in nothing but the laborious accumulation of facts, it would soon come to a standstill, crushed, as it were, under its own weight. The suggestion of a new idea or the detection of a law, supersedes much that had previously been a burden upon the memory, and, by introducing order and coherence, facilitates the retention of the remainder in an available form. Those who are acquainted with the writings of the older electricians will understand my meaning when I instance the discovery of Ohm's law as a step by which the science was rendered easier to understand and to remember. Two processes are thus at work side by side, the reception of new material and the digestion and assimilation of the old; and as both are essential we may spare ourselves the discussion of their relative importance. One remark, however, should be made. The work which deserves, but I am afraid does not always receive, the most credit, is that in which discovery and explanation go hand in hand, in which not only are new facts presented, but their relations to old ones is pointed out.

In making ourselves acquainted with what has been done in any subject, it is good policy to consult first the writers of highest general reputation. Although in scientific matters we should aim at independent judgment, and not rely too much upon authority, it remains true that a good deal must often be taken upon trust. Occasionally an observation is so simple and easily repeated, that it scarcely matters from whom it proceeds; but as a rule it can hardly carry full weight when put forward by a novice whose care and judgment there has been no opportunity of testing, and whose irresponsibility may tempt him to "take shots," as it is called. Those who have had experience in accurate work know how easy it would be to save time and trouble by omitting precautions and passing over discrepancies, and yet, even without dishonest intention, to convey the impression of conscientious attention to details. Although the most careful and experienced cannot hope to escape occasional mistakes, the effective value of this kind of work depends much upon the reputation of the individual responsible for it.

In estimating the present position and prospects of experimental science, there is good ground for encouragement. The multiplication of laboratories gives to the younger generation opportunities such as have never existed before, and which excite the envy of those who have had to learn in middle life much that now forms part of an undergraduate course. As to the management of such institutions there is room for a healthy difference of opinion. For many kinds of original work, especially in connection with accurate measurement, there is need of expensive apparatus ; and it is often difficult to persuade a student to do his best with imperfect appliances when he knows that by other means a better result could be attained with greater facility. Nevertheless it seems to me important to discourage too great reliance upon the instrument-maker. Much of the best original work has been done with the homeliest appliances ; and the endeavor to turn to the best account the means that may be at hand develops ingenuity and resource more than the most elaborate determinations with ready-made instruments. There is danger otherwise that the experimental education of a plodding student should be too mechanical and artificial, so that he is puzzled by small changes of apparatus, much as many school-boys are puzzled by a transposition of the letters in a diagram of Euclid.

From the general spread of a more scientific education, we are warranted in expecting important results. Just as there are some brilliant literary men with an inability, or at least a distaste practically amounting to inability, for scientific ideas, so there are a few with scientific tastes whose imaginations are never touched by merely literary studies. To save these from intellectual stagnation during several important years of their lives is something gained, but the thorough-going advocates of scientific education aim at much more. To them it appears strange, and almost monstrous, that the dead languages should hold the place they do in general education ; and it can hardly be denied that their supremacy is the result of routine rather than of argument. I do not myself, take up the extreme position. I doubt whether an exclusively scientific training would be satisfactory ; and where there is plenty of time and a literary aptitude I can believe that Latin and Greek may make a good foundation. But it is useless to discuss the question upon the supposition that the majority of boys attain either to a knowledge of the languages or to an appreciation of the writings of the ancient authors. The contrary is notoriously the truth; and the defenders of the existing system usually take their stand upon the excellence of its discipline. From this point of view there is something to be said. The laziest boy must exert himself a little in puzzling out a sentence with grammar and dictionary, while instruction and supervision are easy to organize and not too costly. But when the case is stated plainly, few will agree that we can afford so entirely to disregard results. In after-life the intellectual energies are usually engrossed with business, and no further opportunity is found for attacking the difficulties which block the gateways of knowledge. Mathematics, especially if not learned young, are likely to remain unlearned. I will not further insist upon the educational importance of mathematics and science, because with respect to them I shall probably be supposed to be prejudiced. But of modern languages I am ignorant enough to give value to my advocacy. I believe that French and German, if properly taught, which I admit they rarely are at present, would go far to replace Latin and Greek from a disciplinary point of view, while the actual value of the acquisition would, in the majority of cases, be incomparably greater. In half the time usually devoted, without success, to the classical languages, most boys could acquire a really serviceable knowledge of French and German. History, and the serious study of English literature, now shamefully neglected, would also find a place in such a scheme.

There is one objection often felt to a modernized education as to which a word may not be without use. Many excellent people are afraid of science as tending towards materialism. That such apprehension should exist is not surprising, for, unfortunately, there are writers speaking in the name of science, who have set themselves to foster it. It is true that among scientific men, as in other classes, crude views are to be met with as to the deeper things of Nature; but that the life-long beliefs of Newton, of Faraday and of Maxwell are inconsistent with the scientific habit of mind, is surely a proposition which I need not pause to refute. It would be easy, however, to lay too much stress upon the opinions of even such distinguished workers as these. Men who devote their lives to investigation cultivate a love of truth for its own sake, and endeavor instinctively to clear up, and not, as is too often the object in business and politics, to obscure, a difficult question. So far the opinion of a scientific worker may have a special value; but I do not think that he has a claim, superior to that of other educated men, to assume the attitude of a prophet.

In his heart he knows that underneath the theories that he constructs there lie contradictions which he cannot reconcile. The higher mysteries of being, if penetrable at all by human intellect, require other weapons than those of calculation and experiment.

Without encroaching upon grounds appertaining to the theologian and the philosopher, the domain of natural science is surely broad enough to satisfy the wildest ambition of its devotees. In other departments of human life and interest, true progress is rather an article of faith than a rational belief; but in science a retrograde movement is, from the nature of the case, almost impossible. Increasing knowledge brings with it increasing power, and great as are the triumphs of the present century, we may well believe that they are but a foretaste of what discovery and invention have yet in store for mankind. Encouraged by the thought that our labors cannot be thrown away, let us redouble our efforts in the noble struggle. In the Old World and in the New, recruits must be enlisted to fill the place of those whose work is done. Happy should I be if, through this visit of the Association, or by any words of mine, a large measure of the youthful activity of the West could be drawn into this service. The work may be hard, and the discipline severe; but the interest never fails, and great is the privilege of achievement.

VI. The Development Theory: A Review.*

By E. W. Claypole, Akron, O.

This little work is an unpretending but useful compilation intended to put within the reach of intelligent readers some of the fundamental facts on which rests the "Evolution Theory." As the writers truly remark in their preface, "few, except special students of the natural or physical sciences, really know what is meant by organic evolution. Ask," they say, "the average graduate of a high school, an academy, or even of one of our minor colleges to state his conception of the theory. One is ashamed to quote the ready but unmeaning reply, which not seldom would be: Oh, I only know that Darwin thought we were de-

* The Development Theory by J. Y. & Fanny D. Bergen. Lee & Shepard, Boston.

cended from monkeys." With this object the writers have wisely abstained altogether from indulging in theorizing or fine writing, and have confined themselves to stating facts in the simplest language, and leaving these facts to speak for themselves.

The first chapter states the subject, setting forth the rival theories of creation by evolution and by special act. In the second chapter some of the most striking known cases of the development of new species are quoted, and the imaginary nature of the line between species and varieties is forcibly brought out. We are tempted to quote one by way of illustration:

"At Steinheim, in Wurtemberg,* Germany, was once a small lake, and its waters grew countless little shell-fish, many of them water-snails like those of the rivers and lakes at the present day. By the appropriation of the dissolved limestone in the waters of the lake, generation after generation of these snails built up their shells, only to let them fall to the bottom on the death of the little inhabitant. By this slow process a layer of shell-mud was formed, which has since hardened into chalk. About forty distinct layers of this chalk may be distinguished, containing the perfectly preserved remains of many shells. And now for the wonderful part of the story. The shells of each layer remain much the same throughout its thickness, but toward the limit of each, or before the beginning of the next, the shells are observed to vary, so as to approach the form which will be found in the next layer. And not only are the shells of the lowest layer so different from those of the highest that if the intermediate forms had not been discovered they would certainly be called different species, but there are many among the intermediate forms themselves which, if they had been found separate from the others, would have been counted distinct species."

A diagram accompanies this extract, showing a gradual but steady change of form from flat, discoid, planorbiform shells at the base of the deposit to others with elevated spire at the summit. No two of the series would probably pass as the same species among conchologists, as every one acquainted with the fine splitting prevalent among American Unios and Melanians will

* Schmidt, Descent and Darwinism, pp. 97, 98.

readily admit. In the third chapter are mentioned the causes which, so far as is known, can and do develope variation. They are summed up in the following sentence:—"Any change in the circumstances, *i.e.*, the sum total of the influences affecting any organism, will be likely to work some alteration in that organism or its descendants, or both." In confirmation of this position the well-known instance of the brine-shrimp (*Artemia—Branchipus*) is brought forward. A more obvious one, that of the Axolotl, we extract, somewhat condensed.

"The Mexican Axolotl is a tadpole-like creature of considerable size which lives in the water, breathes by gills, and is reproduced from eggs. In its native country this creature is not known to change its form.

"Now in 1867 it was observed at the Jardin des Plantes, in Paris, that some of these animals cast their skins after crawling out of the water, and began a new existence in the shape of a common Salamander (*Amblystoma*). This astonishing change from Axolotl to Salamander is accomplished in from fourteen to sixteen days, and, it seems, may always be brought about in healthy specimens by placing them in shallow water and gradually diminishing the supply. The Salamanders so produced lay eggs which hatch into tadpole-like larvæ that soon grow into Salamanders." Several other cases of the same kind are given by the authors for which we must refer the reader to the work itself.

In reference to this part of the subject it is only fair to remark that not a few cases can be quoted in which organisms have refused, and do constantly refuse, to vary in spite of very great changes of environment. Darwin called attention strongly to this fact, and others have since done the same. Yet, allowing full weight to the objection, it forms no insuperable barrier in the path of the evolutionist. We know not what counteracting influences may be at work to prevent the expected variation. Even if with Darwin and others we attribute the refusal to vary to a resistance on the part of the organism, yet that resistance may be an outcome of past conditions, possibly a deep-seated and long enduring habit. Strong and well established facts proving variancy under varying conditions cannot be set aside by negative facts. Similar difficulties are encountered by all great generalization. The Copernican theory of the universe stumbled at first among many such obstacles, but one by one they have been removed, and we may reasonably anticipate the same result in the case of the theory of Organic Evolution.

"Natural Selection" forms the topic of the fourth chapter. This doctrine, called by Herbert Spencer the "survival of the fittest," determines which among the numerous varieties produced by environment shall live and which shall die. As the Spartan council inspected each new-born infant, and decided whether it should be reared as a citizen-soldier of the state or exposed on Mount Taygetus to die of starvation, so natural selection sits in judgment over every nurseling, and according to circumstances pronounces its doom. If in harmony with its surroundings, it may live; if in discord, it must die. If more suited to its environment than its ancestors, it may even multiply and crowd them out of existence. "Variation," says Prof. Asa Gray, "is the wind, but natural selection is the rudder that guides the organism." The mode in which natural selection acts in thus directing the future form of the organism, is well illustrated by the authors with numerous and obvious examples.

In connection with this important branch of the subject we may remark that human selection is now in many cases far more influential than natural selection, in determining the survival of varieties. But the principles of human selection are often very different from those of nature apart from man. Nature preserves only those most fit to take care of themselves in the struggle for existence. Man often preserves those who would have no chance in such a warfare. The weak in mind or body, whom nature and Sparta would have condemned to speedy death, are kept alive; sentiment forbids their abandonment to their natural fate. The wisdom of this policy is sometimes called in question by sanitarians, but, whether wise or unwise, it is established beyond overthrow. Beyond doubt the prevailing practice tends to entail a terrible burden on our civilization. Prof. A. M. Bell has recently called attention to the danger of creating a race of deaf-mutes, by the constant preservation and perpetuation of such persons. It is however too wide a subject to be here discussed, and leads to other and greater problems with which future sanitarians and legislators must deal. If the sympathies of man will not allow the laws of nature to take their course cannot the production of such forms be avoided or prevented?

Among domesticated animals human selection is by far the most potent factor. "It was said that in England the introduction of short-horned cattle operated almost like a pestilence in the destruction of the earlier and less-improved breeds. The comparison means that the owner of the old sorts killed them off for meat at such a rate as to thin them out as fast as the cattle-plague could have done." Yet the short horns would be ill-fitted to hold their own in a free-fight for existence among long-horned competitors. Man, however, finds that they come more quickly to maturity, are consequently cheaper to raise, and their meat is of fine quality. Hence he decides that they shall live, and the long-horned races go down before them.

One of the most interesting chapters in the book is that which treats of mimicry, a doctrine which maintains that among the processes of natural selection is one whereby varieties accidentally possessing characters belonging to other species, and deriving benefit therefrom, transmit that character to their offspring, and a species arises that resembles or mimics the other. This is one of the most curious fields in the domain of evolution, and one which will reward, as well as any, patient and careful observation. Very little is at present known on the subject, but that little is strongly suggestive to the thoughtful mind of the intensity of the struggle for existence, and the minuteness of the difference on which success or failure may depend. We must refer the reader to the work for examples of this doctrine and for others illustrating the keen competition among plants and insects for mutual advantages in the fertilization of the former and the feeding of the latter.

The marvellous testimony of embryology to evolution is next considered; and some of the leading well-ascertained facts detailed. These point like index-fingers to a common ancestry for all living beings, they point along converging roads to a spot at some epoch in the past, where all these diverging ways started from a common centre. No one in this country has contributed so much to the embryological argument in favor of evolution as Prof. L. Agassiz, himself one of its most determined opponents. The close and accurate study of the antenatal history of organic beings has revealed a multitude of facts which have been generalized by Haeckel into the law, that "the history of the development of the individual portrays the history of the development of its tribe." Thus, then among the successive changes of the egg from its first formation to the exclusion or birth of the animal, we may read with our own eyes a concise summary of its development through a long series of ancestors, whose remains are entombed in the rocks or destroyed for ever by geologic catastrophes. Embryology thus enables us to read in miniature and close at hand, an epitome of the greater record which the lapse of time has removed almost beyond our ken, and its accidents have in great part effaced.

Confirmatory of the testimony of embryology is that of geology, which forms the subject of the next chapter. These two records confirm and supplement each other. It is not probable that the former can ever supply all the details of the past history of any species. It is certain that from the record of the latter many chapters are missing, and will never perhaps be recovered. But geology is supplying ample evidence that the deductions of embryology are well founded, and embryology is furnishing unmistakable indications of the line which descent has followed in the production of existing species. Both records are as yet very incomplete, and call for years of patient labor and thought to lessen their imperfection. But every advance in the same direction, every new fact points the same way. "Missing links" come to light connecting species with species in the past, and every one adds vastly to the force of the cumulative argument. With the discovery of every one, the gaps remaining become less important, and before long the induction may become sufficient to warrant the acceptance of a universal inference by every unbiassed mind.

"Wherever the known incompleteness of the geological record, 'prevents our explaining' a difficulty, it becomes the believer in the development theory frankly to acknowledge that the riddle is too intricate to be solved by any means at his command. And yet, until an evolutionary rise of species had been assigned as an explanation of the succession of higher and higher animals and plants through the geologic ages, what adequate reason for this progress could be given? Strike out from our present conception of the organic world all notion of actual relationship by descent, and what have we left but a mighty list of extinct creatures whose rise, progress and disappearance are far more unaccountable than those of the genii of the Arabian Nights."

We omit the chapter on the geographical distribution of animals and plants—a new-born science which under the labors of Wallace and others, is yielding valuable results in the same direction, and pass on to that on the origin and antiquity of man. This will prove to many readers the most interesting in the volume. Those who believe with the authors in the Evolution of Man from a lower form, will find some facts on which they can base a rational faith. Those who reject such an ancestry for themselves would do well to calmly and dispassionately consider the evidence before giving way to prejudice. Our authors write:—"In order to gain a clear conception of the geological relation of man, let us recapitulate the life-history of the individual from the beginning. We shall find the future human being a mere nucleated cell, a little speck of albuminous jelly, the mammalian egg. So closely do the form, the size, and the structure of this little cell remind us of the amœba-cell that Haeckel's inference is most natural. The ancestors of the higher beings must be regarded as one-celled beings similar to the amœbæ which at the present day occur in our rivers, pools and lakes. The incontrovertible fact that each human being develops from an egg which, in common with those of all animals, is a simple cell clearly proves that the most remote ancestors of man were primordial animals, of this sort, of a form equivalent to a simple cell. When therefore the doctrine of the descent of man is condemned as 'horrible, shocking, and immoral,' the unalterable fact, which can be proved at any moment under the microscope, that the human egg is a simple cell in no way different from those of other mammals must equally be pronounced horrible, shocking, and immoral."

The close resemblance of structure between man and the higher apes so often pointed out is brought forward, and the sole cause of man's superiority is shewn to lie in his brain, there being not a few serious deficiencies in other organs which set him at a disadvantage in comparison with some of the lower animals. Several of these are mentioned, the vermiform appendage, the coccyx or rudimentary tail, and the eye, in which Helmholz has pointed out six decided optical defects. We may here remark that a valuable and interesting paper by Dr. Clevenger on this topic may be found in the American *Naturalist* for 1884 containing additional facts and establishing some physical disadvantages of the erect position in man.

Dwelling for a short time on the low and degraded moral condition of savage man, the author adds:

"The most inhuman monster of crime that ever was condemned by a court and executed by an officer of the law would, among such tribes as those of the Australian natives, pass for the embodiment of all excellencies, and rise to an uncontested chieftainship. Yet out of such elements, and from the midst of such degradation, scientists must conclude that the human race as it is now has risen."

A few pages on the fossil remains of man conclude the work, showing that archæology has already amassed sufficient facts to prove man's presence on earth for a vastly longer time than was formerly supposed, or, at all events, the existence of a creature, man, ape, or intermediate, capable of forming and using weapons and tools of chipped flint. Bones are as yet very scarce, but implements are abundant, of whose artificial nature no doubt can exist. As the worker was before his work, so the maker of these tools, whatever he was, must have existed before they were made.

Twenty-five years ago, the antiquity of man and his development from lower animals were subjects mentioned only with bated breath as awful possibilities of which heretical scientists were beginning to speak. But the world moves, Kent's cavern and many others have been ransacked, river-gravels have been searched, lake-beds examined, and peat-mosses and kitchen-middens dug over, until now the wealth of evidence is bewildering to all but the antiquarian specialist. The proof of man's antiquity is unassailable, and that for his development is rapidly becoming unanswerable.

In closing this sketch of a subject for which the little book noticed at the outset has furnished a text, we need only say that to the general reader who wishes to obtain some firm foundation of fact regarding the theory of evolution, and who has neither the time nor the technical knowledge necessary for the perusal of strictly scientific works we heartily recommend it. To college-students and scholars in high schools also, who do not wish to go out into life unacquainted with the grandest generalization of our day, we also advise its perusal. And all who wish to make acquaintance with the fundamental facts of evolution, whether called by the name of Darwin or any other, and who are not too blinded by prejudice to read and reason fairly, we commend this little work, the composition of a period of enforced seclusion from active life on the part of one of its authors.

VII. THE LATE J. GEORGE JEFFREYS, M.D., F.R.S.

BY SIR J. W. DAWSON.

A late British mail brings the intelligence of the death of this eminent naturalist, probably the oldest British zoologist next to Owen, and at the time of his decease generally recognised as the most eminent conchologist in Great Britain. Dr. Jeffreys was born at Swansea in January, 1809. He was a zealous collector of shells from his youth, and was one of the earliest scientific dredgers on the shores of Great Britain. He took a leading part in connection with some of the sea-expeditions of the English and French governments, and at the time of his death was busily engaged in the investigation of the mollusca dredged by the Porcupine. Besides his great work in British conchology, he was the author of a large number of memoirs in the Transactions of the Royal, Linnæan and other Societies. He was a man of broad general views, as well as of attention to details, and was especially interested in the relation of the subject to physical geography and the history of genera and species in geologic time. In connection with this he took much interest in the discoveries recently made in this country, and some of his most recent papers had reference to the relations of European and American mollusca. He was in correspondence with many of the leading workers in modern and pleistocene mollusks in the United States and Canada. When in Canada some years ago he was the guest of Sir William Logan, and spent much time in inspecting the collections of Dr. Carpenter at McGill College, and in examining the dredgings of Mr. Whiteaves in the collections of the Natural History Society. He had given some attention to the study of entomology, but his life-work related to the natural history and distribution of the mollusca. As to his kindly and genial nature and friendly and public-spirited disposition, all of his many friends on both sides of the Atlantic well agree with the statements in the following extract from the obituary in the London *Times:*—

"Almost from its foundation he was a constant and prominent attendant at the meetings of the British Association; he was president of the biological section in 1877, and one of the vice-presidents at the meeting in Swansea in 1880. With his keen interest in the promotion of biological research generally, he was one of the founders of the Marine Biological Association of Great Britain. For forty-five years he has been a Fellow of the Royal Society, and latterly one of the guiding spirits at the Royal Society Club. At the age of twenty he was elected to the Linnæan Society. Of the Geological Society he was treasurer for many years, and of many foreign societies he was an honorary member. Dr. Jeffreys, while strong in his own opinions, was one of the most genial of men, and a man of many friends. He was not a Darwinian in the full sense of that term; he thought the evidence of his shells was against the doctrine, but his opposition had nothing of bitterness. Dr. Jeffreys occupied several important public positions during his lifetime. He was a J.P. of Glamorganshire and Breconshire, as also J.P. and D.L. of Hertfordshire, for which county he served as High Sheriff in 1877. In recent years he has lived mostly at Ware Priory, Herts, where he delighted to exercise a liberal hospitality. Dr. Jeffreys had a son and five daughters, one of whom is married to Professor H. N. Moseley."

VIII. Report of the Geological Society of London.

March 25, 1885.—Prof. T. G. Bonney, D.Sc., LL D., F.R.S., President, in the Chair. The following communications were read: "On an almost perfect Skeleton of *Rhytina gigas* —*Rhytina stelleri* ('Steller's sea-cow') obtained by Mr. Robert Damon, F.G.S., from the Pleistocene Peat-deposits on Behring Island." By Henry Woodward, LL.D., F.R.S., F.G.S.

The author spoke of the interest which palæontologists must always attach to such animals as are either just exterminated, or are now in course of rapid extirpation by man or other agents. He referred to the now rapid destruction of all the larger mammalia, and expressed his opinion that the African elephant, the giraffe, the bison and many others, will soon be extirpated unless protected from being hunted to death. The same applies to the whale and seal-fisheries. He drew attention to a very remarkable order of aquatic animals, the Sirenia, formerly classed with the Cetacea by some, with the walruses and seals by others, and by De Blainville with the elephants. He particularly drew attention to the largest of the group, the Rhytina, which was seen alive and described by Steller in 1741. It was then confined to two islands (Behring Island and Copper Island). In forty years (1780) it was believed to have been entirely extirpated. It was a toothless herbivore, living along the shore in shallow water, and was easily taken, being without fear of man. Its flesh was good, and it weighed often three or four tons.

The author then described some of the leading points in the anatomy of Rhytina, and indicated some of the characters by which the order is distinguished. He referred to the present wide distribution of the Sirenia:—*Manatus* with three species,* namely, *M. latirostris* occupying the shores of Florida and the West Indies; *M. americanus*, the coasts of Brazil and the great rivers Amazon and Orinoco; *M. senegalensis*, the west coast of Africa and the rivers Senegal, Congo, etc.

Halicore with three species,* namely *H. tabernaculi*, the Red

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* Dr. Murie affirms, that, after examination of many specimens, he believes that only two species exist at the present day, one of Manatus, and one of Halicore.

Sea and east coast of Africa; *H. dugong*, Bay of Bengal and East Indies; *H. australis*, North and East Australia.

The fossil forms number thirteen genera and twenty-nine species, all limited to England, Holland, Belgium, France, Germany, Austria, Italy, Malta, and Egypt, and to the United States and Jamaica.

The author gave some details as to the dentition of fossil species, of which Halitherium and Prorastomus are the two most remarkable types.

Lastly, with regard to the geographical area, occupied at the present day by the Sirenia, the author pointed out that two lines drawn 30° N. and 30° S. of the Equator, will embrace all the species now found living. Another line drawn at 60 N. will show between 30° and 60° N. the area once occupied by the twenty-nine fossil species.

He looked upon Rhytina as a last surviving species of the old tertiary group of Sirenians, and its position as marking an "outlier" of the group now swept away.† The greater northern extension of the group seems good evidence of the once warm climate enjoyed by Europe, Asia, and America in the tertiary period.

IX. Proceedings of the Natural History Society.

for 1883-84.

(*Continued from page 64.*)

The *Fourth Meeting* of the session was held on Monday evening, March 31st, Dr. T. Sterry Hunt in the chair. After discussion concerning the Society's Journal, the following were appointed an editing committee: Dr. Hunt, Dr. Harrington, J. T. Donald, D. P. Penhallow and D. A. P. Watt.

Messrs. F. B. Caulfield, C. J. Young, J. J. Robson and E. O. Robert were elected ordinary members of the Society, and Messrs. A. Inglis and Thos. Devine were proposed for membership.

† Only three or four examples of the reconstructed skeletons of Rhytina are known at the present day, viz.:—a nearly perfect skeleton in the St. Petersburg Museum (described by Nordmann and Brandt), a less perfect one at Stockholm (obtained by Nordenskiold) and the one now under consideration, obtained by Mr. Damon.

The President then read a paper on, "The Genesis of Crystalline Rocks," after which the meeting adjourned.

The *Fifth Meeting* was held April 28th, Mr. J. H. Joseph, vice-president, in the chair.

Mr. W. F. Ferrier was appointed to represent the Society at the third annual meeting of the Royal Society of Canada. Messrs. Sumner, Beaudry, Marler and Shearer were appointed a committee to superintend all others relating to the Society's annual field-day.

A suitable minute referring to the death of Mr. F. W. Hicks, formerly recording secretary to the Society, was placed on record.

Messrs. A. Inglis and Thos. Devine were elected ordinary members, and the Bishop of Huron was proposed for honorary membership.

Two papers were then read; one by Mr. F. B. Caulfield, who explained the characteristics and habits of the British game-birds presented to the Society last year by Mr. Jowett of Sheffield, England, and one by Mr. G. L. Marler on "An unread Leaf in Botany."

The *Annual Meeting* was held on May 26th. The president, Dr. Hunt, occupied the chair. After routine business Mr. John S. Shearer presented the Report of Council for the Session 1883–84.

Mr. G. L. Marler read the treasurer's report and financial statement.

Mr. Wm. Muir read the report of the Museum and library committee.

The editor of the Society's Journal reported, but his report was referred back to the editing committee. The other reports were adopted and ordered to be printed in the journal.

Mr. P. S. Ross referred to the small amount received for membership fees during the year, and suggested that some better method of collection be adopted.

The election of officers was then proceeded with and resulted as follows:—

President, Dr. T. Sterry Hunt.

Vice-Presidents, Dr. J. W. Dawson, Dr. B. J. Harrington, Dr. W. H. Hingston; Messrs. J. H. Joseph, L. A. Hugnet Latour,

J. H. R. Molson, Rev. R. Campbell, Edward Murphy and Dr. Osler.

Corresponding Secretary, Dr. J. Baker Edwards.

Recording Secretary, Geo. Sumner.

Treasurer, G. L. Marler.

Cabinet-keeper and Librarian, Wm. Muir.

Council, J. S. Shearer, J. Bemrose, M. H. Brissette, W. T. Costigan, J. S. Brown, P. S. Ross, J. T. Donald, R. W. McLachlan.

Library Committee, Dr. Wanless, H. Graham, E. T. Chambers Dr. McLaren, J. S. Brown.

Editing Committee, Dr. Hunt, Dr. Harrington, J. T. Donald, D. P. Penhallow, Dr. Wanless.

Mr. G. L. Marler gave notice of a motion to the effect that young men under eighteen years of age be allowed to become associate members upon payment of a yearly fee of one dollar.

A vote of thanks being tendered to the retiring officers the meeting adjourned.

Report of Council, May 26th, 1884.

Your Council have to report that during the season now closing the society has elected twelve members.

The number of persons who have visited the Museum during the year is about 3,300. The lectures of the Sommerville course were delivered as follows: February 14th, Prof. Penhallow, On Tea; February 21st, Prof. Bovey, on Conservation of Force; February 28th, Dr. Major, on The Voice; March 6th, A. T. Taylor, Esq., on Health in our Homes; March 13th, Dr. T. Sterry Hunt, on The Food of Plants; March 20th, Dr. W. George Beers, on A Child's Teeth.

These lectures were well attended, and well received, and the thanks of the Society are due to the gentlemen who delivered them.

It is now a settled fact that the British Association for the Advancement of Science are to meet in Montreal on the 27th of August next, and this Society [illegible] do all in its power (as they did when the American Association visited Montreal in August, 1882) to make the meeting of the British Association successful. The use of our building has been placed at the disposal of the various committees. It is our pleasing duty to record the election from our number of Dr. J. W. Dawson, Dr. T. Sterry Hunt, and Dr. Hingston as vice-presidents of the British Association.

The Society held their *Annual Field-day* on the 8th of June, 1883, at Rougemont, through the kind invitation of Mr. George Whitfield. The day was fine, and a large number availed themselves of the invitation. The start was made from the G.T.R. station at 9 a.m., by the South Eastern Railway, arriving at Rougemont station about 10.45. Mr. Whitfield's farm, which was soon reached, is beautifully situated at the foot of the Mountain, and from his house a fine view of the country for miles around was visible, giving the visitors an excellent opportunity of seeing the highly cultivated land that lay as it were at their feet.

A light collation was provided by our kind hostess, Mrs. Whitfield, after which the party divided into groups, some bent on seeing the country from the top of the mountain, while others started to examine the geology and the botany of the neighborhood.

At noon the several parties returned, as prearranged, to inspect the farm and the numerous herds of high-bred cattle. The whole company then sat down to a bountiful and well-served dinner, at the close of which our esteemed President, Dr. Hunt moved a hearty vote of thanks to Mr. and Mrs. Whitfield, heartily responded to by all present.

During the afternoon Dr. Hunt gave an able and interesting lecture on the geology of that part of the country. The collections made during the day having been brought in and examined, prizes were awarded as follows:—

Collection of named plants (Ladies' prize) Miss E. Martin. Collection of named plants (Mens' prize) Mr. E. Blackader. For collection of unnamed plants Miss Carsley and Miss Cooper, equal. Entomological prize, Mr. R. C. Holden; Geology and Mineralogy, Mr. G. R. Martin. The day being well nigh-spent the party prepared for returning, first giving thanks to Mr. and Mrs. Whitfield for the pleasant day they had spent, and soon after reached the station and the train, arriving home shortly after 9 p.m.

An effort should be made by the Society to increase its membership and secure a greater interest in its proceedings, which would enable us at the same time, in some measure, to keep up the income, in view of the withdrawal of the Government Grant; and a committee on membership should be appointed by the incoming officers for that purpose.

Six regular meetings of the Society have been held during the year, and eight meetings of the council.

Report of the Cabinet-Keeper and Librarian for the year ending May 18th, 1883

Your Treasurer will report to you the state of our finances and Mr. Muir will report on the Museum and Library.

The whole respectfully submitted.

Work on the building: The old counter-desk in the lecture hall has been taken down and replaced by a light and handsome reading-stand. Shelves have been fitted up in the library for the reception of periodicals, pamphlets, etc.

Work in the museum: Mr. Caulfield has cleaned and arranged the exotic insects; this finishes the work on the entomological collections, all of which are now in good order.

The following is a list of the donations to the Museum during the year, with the names of the donors:—

Osprey (*Pandion haliaetus*) Acadian Owl (*Nyctale acadica*) Blue Bird (*Sialia sialis*)	Mr. W. L. Marler, St. Johns, P.Q.
Wood Duck (*Aix sponsa*) Velvet Duck (*Melanetta velvetina*)	Mr. L. J. McDonald, St. Johns, P.Q.
Herring Gull (*Larus argentatus*) Bonaparte Gull (*Larus philadelphiæ*)	Mr. G. L. Marler.

By purchase,

European Hen Harrier. (*Circus cyaneus.*)