PROCEEDINGS

OF THE

Nova Scotian Institute of Science.

SESSION OF 1900-1901.

ANNUAL BUSINESS MEETING.

Legislative Council Chamber, Halifax, 19th November, 1900.

THE PRESIDENT, DR. A. H. MACKAY, in the chair.

THE PRESIDENT addressed the Institute as follows :—

GENTLEMEN,—It has been customary at our Annual Meetings for the retiring Presidént to make a summary review of the year's work—a sort of annual inventory. In following this custom, were I speaking to the general public, I would be required to give some kind of demonstration of the object and value of such work as we are doing in line with similar organizations in every civilized country. For those who see a fine mushroom grow in one night are generally unaware of the one hundred nights and the one hundred days during which its invisible white, silken, threadlike mycelial cells were tunnelling the surrounding earth in myriad lines with ceaseless activity, so that when the appropriate time should come tens of thousands of microscopic tubular lines of transport should simultaneously carry from every quarter the duly assimilated matter to build up and complete in a few hours the visible and generally appreciated fruit. Every great discovery or invention of modern times popularly considered great, is in like manner simply the fruit of the unostentatious, patient and continuous labor of a multitude of seekers after knowledge of the truth, the whole truth, and nothing but the truth, in one or more regions of the infinite domain in which we exist. Without these humble and severely accurate observations of fact and measurements of force going on from year to year there can be no longer expected at smaller or greater intervals in the future those brilliant generalizations which dazzle the multitude and form an epoch in the history of man.

We may perhaps have met with some indications that there are people who think that an Institute of Science such as ours should devote itself to the grand problems of human life in such a manner as to electrify the public, convince the sceptic, and reform human society on lines based on indubitable principles. Such persons seem to expect that if scientific men are of any use they could by the application of their thinking powers discover these grand principles and demonstrate them with the potency of universal conviction. They are evidently unaware of the most striking fact in the history of man, that from the beginning of society up to their own appearance in the role of thinkers, men have been trying to solve these problems by thinking, striving to draw knowledge out of brains into which the knowledge never entered. The deductive metaphysical philosophers of old are still being produced, more numerous than ever if not more powerful, and the ancient problems are not yet solved.

We have never yet gained any advantage by thinking out what nature should be. We have to find out what it is, and so far as we know what it is we can utilize it according to our limitations. And the solutions of the so-called grand problems are often dependent on what might be called the humblest facts. The grandness of a truth discovered cannot be known until the full train of its effects can be seen; so that to the truth seeker any truth may well be considered grand. It is a sound principle for each to seek whatever truth is nearest him, so that he may add it to the common stock which is now becoming the broad base of the so-called grand truths which humanity has learned to applaud after a period of suspicion.

This is the principle on which our Institute is working. The geologist is near to the discovery of new geological facts by reason of his previously obtained geological knowledge and his opportunities of studying for years his own local ground. He exercises his special powers with the result of obtaining further knowledge which through our publications are made the property of the truth-seekers throughout the world. And so with each of us. We have our own special opportunities for some kind of exact observations on points not hitherto exactly observed, and in making such observations we are as deserving as he who makes the final observation to complete a grand theory, providing we have brought the same energy and ingenuity to bear on our problems.

The discovery of grand principles—of great truths—is now more than ever before a composite work contributed to by many knowledge makers. The South American Indian who first by accident discovered the anti-malarial effect of the extract of Peruvian Bark, discovered a great fact without any special preparation and possibly without the aid of any previous more or less partial observer. But still, for over three centuries the *Hæmamœba vivax* and *Hæmamœba malariæ*, living jelly specks so infinite that a blood corpuscle is a meadow for them, got through the human skin (more than a Chinese wall for them), and into the blood stream, and from thence into the blood discs themselves, which they finally destroyed.

It was not until twenty years ago that Laveran discovered their presence in the life fluid, but how impossible would it have been for him to have discovered such organisms until the microscope had been improved to a high degree of excellence and microscopic methods had been discovered by other workers. Yet no one could show how the minutely microscopic animal more destructive to the human race than all the historical beasts of prey, found its way into the blood. Multitudes of observers finally seemed to relegate the home of the organism to the malarial swamps, but it could not be found in the swamps. These observers, however, made a very important contribution to the general stock of knowledge, for as the mosquitoes pass their larval stage in water, suspicion was finally extended to them. Yet people were taking great care to protect themselves from the malarial air which poisoned no one, while infected mosquitoes were allowed to inoculate them unsuspectingly on the adjacent dry lands. Danilewsky, Golgi, Antolisei, Grassi, Bignami, Bastianelli, Labbe, Mannaberg, Manson, Nuttall, Metchnikoff, Daniels, McCallum, and others, and finally Ronald Ross, worked on the humble mosquito until 1899 before the problem was solved.

Other specimens of *Hæmamœba* were found in the common mosquito and in other animals who were inoculated by the mosquito, and who in turn could infect sound mosquitos. Finally species of a genus of mosquitos, *Anopheles*, were found infected with the malaria *Hæmamœba* in a most unexpected form. Sound *Anopheles* were found to be infected by feeding upon the malarial patient, and infected ones communicated malarial fever to those whom they were allowed to bite. For about twenty years these men from every country of Europe were studying the life and particular habits of the mosquitos, each contributing something to help the others. But should it be asked what species of the mosquito we have in Nova Scotia, all we could say is that thirty different species are generally recognized on the continent. But we could not say how many are to be found here. We only presume that *Culex pipiens* is the common if not the only one.

If we had some observer studying the humble subject of our mosquitos, even were in only demonstrating the different species to be found in this Province, we should have some share in this important discovery of the close of the century. In the meantime our high flown deductive philosopher racking his brain in circling after grand truths, is circling still, as near and yet as far as ever from the mental mirage he is following. The grand truths oftentimes come from the most unexpected directions; therefore it is wise for us to hold all truth in esteem and worth the seeking.

The past year is also, to a marked extent, the beginning of a new epoch in the history of our Institute. The Provincial Museum, although not the property of the Institute, was built up by the members of the Institute, and was from the beginning its headquarters. But for the last few years it had become so crowded by the accumulation of material and the lack of a curator, such as it had during the lifetime of Dr. Honeyman, that it served neither as an efficient museum nor as a desirable meeting place. Besides, our rapidly accumulating library, coming mainly as exchanges from the leading scientific institutions and societies all over the world, could not at all be accommodated. For the last few years the Council had to procure temporary accommodation for it in the University building of Dalhousie, where there was proper library room for but a portion of its volumes.

The Provincial Government having seen the great importance of stimulating scientific study as the foundation of a safe and rapid industrial development of the country, and having the good fortune to be able to secure on good terms the fine building adjacent to the Province Building as an annex, with spare room beyond the immediate demands for offices, determined to provide the ways and means for the public utilization of all this hidden wealth. The Museum has been transferred to the new building and re-arranged on scientific lines under the curatorship of Mr. Harry Piers, who is rapidly making it a real Provincial Museum.

Students will already find it well classified, so as to show the products of the country of scientific and economic interest to their best advantage. The numerous blanks are being filled as rapidly as specimens can be secured, and each object is in the process of being labelled so as to give not only its name but a summary of such information respecting it as is most likely to be of use.

On the adjacent flat the Government has provided ample library accommodation for the Library of the Institute and the books from the Legislative Library bearing on science and the arts, with a reading room. There is also sufficient accommodation for the Library of the Mining Society of the Province. In this manner all these scientific collections increasing from day to day, all these libraries also increasing from day to day, are made available freely to students, miners, manufacturers, and the public generally.

Under the capable management of Mr. Piers, these institutions are not only sure to give satisfaction to the Government, but to the public, who are thus admitted to invaluable privileges which previously even members of the Institute could not avail themselves of without much loss of time and inconvenience to others. The Government, in assuming the charge of this composite Library, are able to open to the public the invaluable, modern, and rapidly growing library of the Institute; and the members of the Institute, on the other hand, have also gained thereby easy access to their own literature. This co-operation of interests is of mutual benefit, and the Science Library and the Museum are likely to become an important centre for the scientific students of the city and the Province. The Museum is already open, and in a short time the Library will be in working condition.

There are also signs that the scientific side of educational work throughout the Province is improving, notwithstanding the defects common to our schools and colleges throughout the continent. May the time be not far distant when our Institute may have more recruits to undertake the infinite range of work before us—in discovering the yet hidden truths of nature lying around us on every hand within our own Province, without a knowledge of which we cannot expect to solve indubitably what people call the great problems of the world.

The President referred with regret to the loss of two invaluable associate members, Captain Trott, of the Cable S. S. "Minia," and Rev. Arthur C. Waghorne, who had done so much in the botanical exploration of Newfoundland.

A vote of thanks was presented to the PRESIDENT for his address, and for his services during his term of office.

The TREASURER's report was presented, and having been audited and found correct, was received and adopted.

The thanks of the Society were presented to MR. SILVER for his services as TREASURER.

In the absence of the LIBRARIAN, the report on the Library was read by DR. MACGREGOR. The report was received and adopted.

In recognition of the services of MR. BOWMAN as librarian for several years, it was resolved that he be elected a life-member.

The thanks of the Institute were presented to the HON. ROBERT BOAK, President of the Legislative Council, for granting the use of the Council Chamber; to HIS WORSHIP THE MAYOR, for the use of the City Council Chamber; to the BOARD OF GOVERNORS OF DALHOUSIE COLLEGE, for the use of a room in the College building for the purpose of accommodating the society's Library; and to the SECRETARY OF THE SMITHSONIAN INSTITUTION, Washington, for continuing to admit the Institute to the privileges of the Bureau of International Exchanges.

The following were elected officers for the ensuing year (1900-1901):—

President.—A. H. MACKAY, ESQ., LL. D., F. R. S. C., *ex officio* F. R. M. S.
Vice-Presidents.—F. W. W. DOANE, ESQ., C. E.; and HENRY S. POOLE, ESQ., F. G. S., F. R. S. C.
Treasurer.—WILLIAM C. SILVER, ESQ.
Corresponding Secretary.—PROF. J. G. MACGREGOR, D. SC., F. R. S.
Recording Secretary.—HARRY PIERS, ESQ.
Librarian.—MAYNARD BOWMAN, ESQ., B. A.
Councillors without office.—ALEXANDER MCKAY, ESQ.; EDWIN GILPIN, JR., ESQ., LL. D., F. R. S. C.; MARTIN MURPHY, ESQ., D. SC.; PROF. EBEN MACKAY, PH. D.; WATSON L. BISHOP, ESQ.; RODERICK MCCOLL, ESQ., C. E.; H. W. JOHNSTON, ESQ., C. E.
Auditors.—WILLIAM MCKERRON, ESQ., and G. W. T. IRVING, ESQ.

FIRST ORDINARY MEETING.

Legislative Council Chamber, Halifax, 19th November, 1900.

The PRESIDENT, DR. MACKAY, in the chair.

The meeting was held after the adjournment of the Annual Business Meeting.

It was announced that J. R. DeWolfe, Esq., M. D., Halifax, and Walter H. Prest, Esq., M. E., Bedford, N. S., had been elected corresponding members.

Owing to the lateness of the hour, the reading of Mr. Fletcher's paper "On Geological Nomenclature of Nova Scotia: New Glasgow Conglomerate," was deferred.

Second Ordinary Meeting.

Legislative Council Chamber, Halifax, 10th December, 1900.

The President in the chair.

It was announced that Miss A. Louise Jaggar, Smith Cove, Digby Co., N. S., had been elected an associate, and Charles Henry Davis, Esq., C. E., New York, U. S. A., an ordinary member.

The President read a paper by Hugh Fletcher, Esq., of the Geological Survey of Canada, entitled, "Geological Nomenclature of Nova Scotia: New Glasgow Conglomerate." (See Transactions, p. 323.)

The paper was illustrated by a large geological map, by Mr. Poole, of the locality described.

Third Ordinary Meeting.

City Council Chamber, Halifax, 14th January, 1901.

The President in the chair.

Henry S. Poole, Esq., F. R. S. C., presented "A Description of the Davis Calyx Drill."

The subject was discussed by Messrs. Bishop and Anderson, and Drs. Murphy and MacKay.

Dr. MacKay read a paper by Walter H. Prest, Esq., M. E., "On Drift Ice as an Eroding and Transporting Agent." (See Transactions, p. 333.)

The paper was discussed by Drs. MacKay and Murphy, Prof. H. W. Smith, and Messrs. Poole and Piers.

FOURTH ORDINARY MEETING.

City Council Chamber, Halifax, 18th February, 1901.

The PRESIDENT in the chair.

It was announced that PROF. EVERETT W. SAWYER, of Acadia College, Wolfville, and PROF. F. C. SEARS, Director of the N. S. School of Horticulture, Wolfville, had been elected associate-members.

A communication was read by the RECORDING SECRETARY, from the ENTOMOLOGICAL SOCIETY OF BELGIUM, announcing the death of its Honorary President, the BARON DE SELYS-LONGCHAMPS.

On motion it was resolved that the N. S. Institute of Science express its deep sympathy with the Entomological Society of Belgium in connection with the irreparable loss which the society has sustained through the death of its Honorary President, the Baron de Selys-Longchamps.

A communication was also read from the ZOOLOGICAL-BOTANICAL SOCIETY OF VIENNA announcing the celebration of its fiftieth anniversary.

On motion it was resolved that the N. S. Institute of Science offer its cordial congratulations to the Zoological-Botanical Society of Vienna, on the celebration of its fiftieth anniversary and the completion of fifty years of fruitful work, and express the hope that the society's efforts for the advancement of science may, in the future, as in the past, be crowned with success.

PROF. H. W. SMITH, B. Sc., of the Provincial Agricultural School, read two papers, entitled, (1) "Rotation of Leguminous Crops," and (2) "The Preservation and Use of the Tops of Turnips and other Root Crops."

The subjects were discussed by HON. T. R. BLACK, DR. MACKAY, MR. G. MARSHALL, and others.

A vote of thanks was presented to PROF. SMITH for his communications.

Owing to the lateness of the hour, the reading of MR. POOLE's paper on "Stigmaria Structure," and of DR. MACKAY's "Note on Gravel taken by the mushroom-anchor of the 'Mackay-Bennett,'" were postponed.

Fifth Ordinary Meeting.

City Council Chamber, Halifax, 18th March, 1901.

The President in the chair.

It was announced that George M. Edwards, Esq., B. Sc., Halifax, had been elected an ordinary member.

A communication was read from the Royal Society of Canada, asking the Institute to appoint a delegate to attend the Society's meeting to be held at Ottawa on May 21st. The matter was referred to the Council.

On motion, the Council was directed to prepare a resolution expressive of regret at the death of the late Dr. J. R. DeWolfe, one of the Institute's oldest members.

[The resolution, subsequently prepared, was as follows :—

"*Resolved*, That the Council place on record its deep sense of the loss sustained by the Institute through the death of Dr. DeWolfe, who was well known in his profession, was elected a member of the Institute on 26th October, 1863, was for a number of years a member of the Council and also second Vice-President, always took a deep interest in and actively furthered the aims of the society, and was at the date of his death the oldest living member.

Further resolved, That the Secretary be directed to send a copy of the resolution to the family of the deceased, and to express to them the sympathy of the Institute in their bereavement."]

Henry S. Poole, Esq., F. R. S. C., read a paper "On a Polished Section of Stigmaria showing an axial cellular structure." (See Transactions, p. 345.)

A. H. MacKay, Esq., Ll. D., presented the results of a microscopic examination of the specimen of Stigmaria. (See Transactions, p. 346.)

The subject was discussed by Messrs. Bishop and A. McKay.

A. H. MacKay, Esq., Ll. D., read a "Note on Gravel taken by the mushroom-anchor of the 'Mackay-Bennett,' cable steamer, from the bottom of the Atlantic, 40 miles west of Sable Island."

The paper was discussed by Mr. Poole.

WATSON L. BISHOP, ESQ., read a paper on "The Star-nosed Mole," and exhibited specimens of the young. (See Transactions, p. 348.)

PROF. J. G. MACGREGOR, D. SC., communicated a paper "On the use of the Wheatstone Bridge with Alternating Currents."

SIXTH ORDINARY MEETING.

City Council Chamber, Halifax, 5th April, 1901.

The PRESIDENT in the chair.

A communication was read from the ENGINEERS' SOCIETY OF WESTERN NEW YORK, Buffalo, offering the use of the society's rooms to any member of the Institute who may visit the Pan-American Exposition. The SECRETARY was directed to make a suitable reply.

The following paper was communicated :—

THE RARE EARTHS: THEIR SCIENTIFIC IMPORTANCE AS REGARDS THE PERIODIC LAW.—BY W. H. MAGEE, PH. D, *High School, Parrsboro, N. S.*

AT a meeting of scientists, it is, of course, unnecessary for me to apologize for the fact that the subject of my paper is one in which the general public takes no interest. There are few, even among chemists, who take more than a passing interest in the so-called Rare Earths. You, however, who are seekers after truth, are aware that even in what might seem the most despicable of materials there are startling discoveries awaiting the patient investigator who will delve into the hidden mysteries and bring to light truth, not only of rare interest to the scientific circle whose sympathies he enjoys, but of advantage to the general public which, though impatient of the labor and details, is ever ready to avail itself of, and to liberally reward, results.

The term Rare Earths, is, if not a misnomer, at least misleading, since there are earths or oxides not classed in the group which are as rare, if not rarer, than these themselves. The usually accepted definition of a rare earth is "a substance precipitable by oxalic acid from a weakly acid solution and having the formula $R_2 O_3$ in which R stands for the element of the oxide." This definition, however, if rigidly adhered to, excludes Ce., Th. and Zr., which are usually ranked with the rare earths and, being applied ever so generally, would place Tl., Ga., Ge., In., etc., among the ordinary or at least not rare earths.

The full list of these earth elements includes Ce., Zr., Th., La., Sc., Yt. and Yb., which are looked upon by chemists as actually elemental, and Pd., Nd., Sm., Ho., Er., Ter., Th., De., Dp., Ph., and even others which appear to differ from each other as oxides and may, some of them at least, be elemental, but are probably in most cases mixtures of two or more elements. They are not, however, known in the elemental condition but only in the form of oxides and salts. Some few have been reduced to the metallic condition yielding then grayish-white metals, but in such small quantities, and with such doubts regarding their purity, that slight advantages have been derived from the reduction.

Before considering the properties of these substances and discussing their importance in the periodic system, it will be well to look into their history. They were first brought to the knowledge of the chemical world during that period of remarkable activity at the close of the 18th and beginning of the 19th centuries. Probably the first time that any mineral containing these oxides in any considerable quantity was noticed was in 1751, when Cronstedt obtained from an iron mine in Sweden a sample of the mineral now known to mineralogists as Cerite, a silicate of Ce., La. and Di., containing as impurities or accessories, however one chooses to consider them, small quantities of other rare oxides, together with iron, alumina, lime and traces of Mn., and even other minerals. This mineral was first analysed by D'Elhuyar in the laboratory of the noted chemist Bergmann, and stated to be a silicate of Fe. and Ca. It may seem remarkable that, even in those early days of chemistry---this was in 1784—such an error as the mistaking of the trivalent oxides for the very common substance lime should occur, but if the experience of such a noted analytic chemist as Plattner, so late as 1846, be considered, all wonder ceases. This chemist analysed several times the mineral Pollux from Elba and, despite all his care, and he was renowned as an analyst, he could only get his results to foot up to 92.75 per cent., nor could any one explain the matter until Bunsen recognized a new metal, Caesium, in the water of the Durkheim salt wells and proved it to be of the alkali group thus closely resembling Na. and K Plattner had been reckoning Cs. with an atomic weight of 132 as K. with an atomic weight of 39, and neither he nor his contemporaries seemed capable of proposing the very simple explanation that there must be present a new element. This experience of Plattner's and its explanation probably saved Winkler from a similar error in 1886 and gave him the credit of the discovery of a new element. Repeatedly analysing Argryodite, as he was chemist of a silver mining company in the Freiburg district in Germany, he met with a constant loss of 7 per cent. in his analyses, and a close search with refined methods enabled him to announce to the world the new metal Germanium.

But I have digressed. The matter rested after D'Elhuyar's time until the fame of the two great analysts, Berzelius and Klaproth, induced some one to send to each of them a sample of the mineral. These chemists soon decided that lime was not the main constituent and that though iron was present it was only in mere traces. Both set themselves to solving the problem and almost simultaneously announced to the world the existence of a hitherto unknown element. There was considerable discussion as to which could claim the precedence, but the scientific world has yielded the palm to Berzelius by adopting his name, Cerium, instead of ochroit-erde proposed by Klaproth. In tracing out the history of this interesting mineral however, we have really passed the date when the apple of discord was thrown among the chemical family. The date of the discovery of an oxide containing the unknown element Ce. was 1804, the date of the discovery of the first of the rare earths was 1789 when Klaproth isolated Zirconia. If this be disputed, for Zr. does not fulfil all the conditions of a rare earth, we must yet anticipate 1804, for in 1794 Gadolin, a Finnish chemist, gave to the world Yttria, the oxide of Yttrium which fulfils in every respect the conditions of our definition. This element was discovered in a mineral from Ytterby, in Sweden, which mineral has since been named in honor of this chemist Gadolinite.

In 1818 Berzelius announced the discovery of a new oxide, Thoria, in some rare minerals from the neighbourhood of Fahlun, Sweden. This discovery he confirmed in 1828 when he found the same oxide in a mineral from Brewig, in Norway. Before going into the history of the very remarkable period which followed, let us see just what was known up to 1835. Ceria, Zirconia, Thoria and Yttria were recognized as distinct oxides, each supposed to contain a distinct element. Only one of these, however, Yttria, belongs to the rare earths, if we keep to the strict letter of our definition. Such, then, was the knowledge of the rare earths; they were ordinary oxides of no more interest than lime or baryta, nay, not so much, for they were of no practical use, they were *rare*, and so of no interest except to seekers after curios.

In 1837-38 a young Swedish chemist, a pupil of Berzelius, took up the neglected earths and under his magic touch, for he was a genius, new truths were rapidly unfolded and a new interest was given to this portion of the chemical field, an interest which has constantly increased, and under the influence of which research will go on until these most subtle elements yield to the scientists truths even more deeply and cunningly concealed than those which are being discovered in the realms of electricity and bacteriology. I think it is no exaggeration to say that nothing would give more pleasure to the chemical world than to find a solution to the mystery which surrounds these rare earths, now rare no longer, if by the word, we mean scarce, but truly rare if we consider it as meaning costly or worthy as regards the chemical truth concealed among them. This chemist was Mosander—a name probably unknown outside the chemical world, and not to all chemists. To the advanced inorganic chemist, however, he is the pioneer in the field, since he was the first to show the immense possibilities which lay concealed in the little then known of these peculiar earths.

Beginning an examination of Ceria he soon announced that it was not a simple oxide but a compound of at least two. This was in 1838. In 1843 he announced that one of these two could be still further decomposed and so from the earth Ceria, long considered a simple earth, there resulted a pale yellow oxide, ceria proper, a brownish white oxide, lanthanum, and a dark brown oxide, didymia, the first yielding yellow, white, and red salts, the second white or colorless, and the third pink salts. As a result of this discovery, an immediate attack was made on the other rare earths. Mosander himself in the following year announced Erbia and, later, Terbia, as earths separable from Yttria; these yield, Yttria colorless salts, Erbia yellow, and Terbia rose colored, a coincidence with the compounds from the Ceria earths. In 1860 Berlin, as a result of long research, announced that Mosander had been mistaken as regards Yttria, but later work has shown that the Swedish chemist had not spoken heedlessly, for Bahr and Bunsen, by a very brilliant piece of work, proved the presence of Erbia in so-called Yttria, and in 1873 Cleve and Hoglund confirmed this. About this time Delafontaine again determined the existence of Terbia. Later, Delafontaine claimed the discovery of an earth, which he called Phillipia, in the Yttria, but this is not as yet acknowledged by chemists. Then came a classical research by Marignac, a Swiss chemist, in which, after separating out several apparently distinct earths, he finally isolated Ytterbium, which is undoubtedly a distinct element, though some chemists, keeping in view the many surprises in this field; still withheld acknowledgment. In 1879, Nilson, another remarkable Swedish chemist, isolated Scandium, and since that time Cleve claims to have found in Erbia a threefold group for one member of which he retains the name Erbia, calling the others Thulia and Holmia.

In the meantime, from a sample of Didymium obtained from the mineral Samarskite, first found in North Carolina, a new oxide, Samaria, was separated in 1878 by Boisbaudran and Delafontaine, acting separately, the latter calling it Decipium. Finally, in 1883, Didymium, which since 1842 had held its place as an element, and from which a metal had been isolated and which had played a prominent part in several quite bitter chemical discussions, all parties basing their arguments on its being an element, was, by Welsbach, a chemist of Vienna, best known as the inventor of the Auer or Welsbach Light, disintregated into what, for want of better name, are called, (or perhaps I should say into tentative substances,) Praseodidymium and Neodidymium. Still more recently, Bettendorf has obtained evidence of the presence of still another oxide in Yttria, which he proposes to name in honor of the original discover of Yttria, Gadolinum.

It has been twice thought that Zirconia was not elemental, once in 1845, when Svanberg thought he had isolated Noria, and again in 1866-7, when Sorby thought he had found Jargonia. Both were subsequently proven to be Zirconia, or it was shown, at least, that there was not enough evidence to consider them elemental earths.

You will note then, that from the two original earths, the Yttria of 1789 and the Ceria of 1804, not less than eleven earths have been isolated and probably two or three more, though the evidence is less conclusive. When I mention that the knowledge of these, though very accurate, is less than that known of our ordinary elements at, say 100 years ago, it will be seen that a wide field exists here for investigators.

Why is so little known concerning them? The answer might be hazarded that it is because of their rarity. This is not so, however, as several of them have been proved to exist in considerable quantities. The trouble lies in their close resemblance to one another, chemical reagents acting similarly toward them all, and thus the only means of separating them is by taking advantage of the difference in basicity of their compounds. This is a very slow process and uncertain, for, being fractional, it is only made exact by numberless repetitions, and so it is extremely difficult to get pure material on which to experiment. This same trouble, too, is one of the chief reasons for the appearance on several occasions of pseudo elements which, obtained with extreme difficulty, seemed to have a fair claim to separate existence, and which required considerable time and skill to prove their non-existence.

Let me illustrate. You are all aware that in the case of our ordinary elements there are sharp points of separation. HCl. throws down from a silver salt solution all (or nearly all, for this reservation must be made in the light of refined methods) the silver as AgCl. H_2S throws down from solution a large number of sulphides even in acid solution we must grant, but for every one of these elements there is some known reagent or some exact method of treatment, which affects one and only one of these elements. There are, undoubtedly, difficulties in exact separations, but a fair analytical chemist can always separate them. With the rare earths, however, each reagent seems to act so similarly that there is no sharp line of demarcation, and the only methods applicable to their separation are slow and remarkably difficult of application. Absolutely quantitative analytical processes are unknown, and no results published in the various mineralogical books as giving the composition of the minerals containing them are reliable. I put forward no claims to superiority as an analytical chemist, but I was occupied from October 1st to the Christmas vacation, with all the advantages of a well equipped laboratory at my disposal, in obtaining 11 grams of pure Ceria, using a method proclaimed as the best to date, but acknowledged to need, as my experience also confirms, a seven times repetition to ensure so-called purity, and leaving behind the suspicion that, as it was purified according to the standard of a vanishing test, it was even then not absolutely pure. Yet this subject has received some of the best thought of the ablest chemists of the world during the past 50 years. Bahr, Bunsen, Rammelsburg, Wolf, Wing, Gibbs, Wöhler, Popp, Crookes, Marignac, Delafontaine, Boisbaudran, Nilson, Cleve, Kruss, Bettendorf, Welsbach, in fact all the advanced inorganic chemists of the past half century. There is no discouragement, the fight goes on with that grim determination to succeed which only the scientist knows. What have they accomplished for the world? Not much in this line! But if these were all the scientist strove for, our discoveries and advance would be of a low order. Indirectly, the close study and wide experience with reagents and methods has led to many useful results, but we need not linger over this. Throughout all the period during which Ni. an Co. have been known, there was no ready and direct method of separating them; but a few years since, on an unsuspected corner of the reagent shelf, an organic compound α-nitroso β-naphthol was found to instantly and completely separate them from each other. Such being the case, we may some day expect research to be rewarded and the mysterious doors to be opened.

I must now, however, in the development of my plan, state the methods most in vogue for separating any rare earth from a mixture of them. Suppose we have a mixture of all or nearly all of these earths, and this is the state in which we usually get them from the minerals containing them, and that we have, say 5 litres, in solution. We remove 10c.c. and precipitate all the earths by means of a standard solution of ammonia, noting carefully the exact amount required. From this can readily be calculated the amount necessary to precipitate the 5 litres. This being determined we take enough of a somewhat weak solution of ammonia to precipitate one-tenth of the earths and add it as rapidly as possible, with violent agitation, that it may be brought in contact with as large a portion of the solution as possible at once. This precipitates the most basic portion to a large extent. The mixture is allowed to settle and the supernatant liquid is drawn off; after which the precipitate is carefully washed and the washings are added to the liquid. This is again treated with ammonia, another tenth being thrown down. This process is repeated till the entire amount is precipitated. The first two or three precipitates are then united, then the next two or three and so on, and each group is again treated in a similar manner, till after some hundreds of repetitions there collects at one end of the series a considerably basic, and at the other a considerably acid, hydroxide. The various precipitations are checked by atomic weight determinations, and when an hydroxide is obtained in which the entire ten precipitates yield identically the same atomic weights, it is considered as an elemental earth, the argument being that no two elements will be at all likely to possess the same basic qualities. This will probably give you an idea of the time expended and the difficulty experienced in working in this field. Ammonia is by no means the only reagent so employed, but every one likely to produce different qualities of precipitates is tried.

Here, then, we have a group of elements whose compounds act differently toward chemical reagents from all other bodies. They resemble the alkalies and alkaline earths, *i. e.*, the Li. and Gl. families in their action toward H_2S, and the B. and Fe. families in their action towards $(NH_4)_2S$. They resemble the last two and all the other families except the Li. and Gl. families in their action toward NH_4OH. They differ from all other elements in their action toward Oxalic acid. All oxalates are more or less soluble in acids; but outside the rare earths, the solubility is perfect. Calcium oxalate is considered an insoluble precipitate, but the reaction must be alkaline, the least trace of mineral acid setting up solution, the presence of oxalic acid prevents the precipitation of alumina, etc., but here we have a group which precipitates at once to oxalic acid or to a soluble oxalate in an acid solution, and if only faintly acid, say 1 or 2 per cent, the cxalates separate out completely, in fact a mere trace is soluble in a 5 per cent acid. Thus we can separate them easily as a group. If we render the supernatant liquid more and more acid, we can gradually get out portions of which the member first removed will differ considerably from that last removed, but probably not in a lifetime would one get one member absolutely free from every other member. Therein lies the difficulty, and so it is with every reagent to some degree. Some reagents shorten the work, and a number of persons working together, by being able to do more work get more rapid results, but the field is one of great difficulty.

There are other troubles, however, in the path of the investigator in these fields. All who have worked practical chemistry are aware that there are qualitative tests by means of which we can detect the presence or prove the absence of any particular metal or acid. Ammonia acts towards a solution of a copper salt as it acts towards nothing else. HCl. gives a white precipitate to silver as well as to lead and mercurous salts, but the chloride of lead is soluble in hot water and can be washed out, that of silver dissolves in ammonia and can in turn be removed while at the same time the mercurous chloride turns black but remains insoluble owing to the formation of a compound with the ammonia so that it is easily determined whether neither one, two or three, or which one is present. This makes the work of Mosander the more remarkable, as in his time there was nothing to enable him to suspect the different rare earths except abstract reasoning from slight color changes.

In 1858, however, Gladstone, the London chemist, noted on examining the light, which had passed through a solution of Didymium salt, with a spectroscope, that in certain parts of the spectrum there were dark color bands although the solution might be perfectly colorless. These have been proved to be due to the absorption of some of the light while passing through the solution. This fact of absorption is not of itself very remarkable. All colored solutions absorb more or less light, $KMnO_4$

solution, purple in color, absorbs certain light, colored glasses absorb light, etc., but all manganese salts, colored or uncolored, do not absorb light, nor if the solutions are of different colors, though of the same element, do they absorb the same portions of light. Gases absorb light, and on looking towards the sun with a spectroscope, faint bands are to be seen in the spectrum. Certain metals when heated, give certain varieties of light as, Na yellow, potassium blue, copper green, etc., and these same substances, converted into vapour, absorb the same light that in their white hot condition they emitted, but here we have the phenomenon of a colorless solution acting like a colored solution or like a gas. This of course gave a test for Di. as soon as the absorption bands were mapped and thoroughly defined. This to the rare earth chemist was a valuable discovery, but in its application it calls forth the highest skill of the chemist, for he must be able to fix upon the exact bands and say this is given by Di., this by Er., etc. Here is displayed a fresh peculiarity of the rare earths, for while there are no elements outside these which give bands, there are several within the group which do, viz.,—Di. gives 31 well defined bands, Sm. 12 not so well defined (and some of them disputed, the subject is being thoroughly investigated by Boisbaudran), Ho. 16, Er. 8; Yb. shows none in the visible part of the spectrum, but many have been mapped in the ultra red portion.

The qualitative application of this knowledge is as follows:—A mixture, say of Di. and Ce., is to be freed from Di., this being most familiar to myself. The oxalate, first well-washed in dilute HCl, is dried and ignited to the condition of an oxide, and then dissolved in HNO_3 and a preliminary examination is made with the spectroscope to make sure of the presence of Di., and incidently to judge of its abundance. The intensity of that band known as the α band is especially noted. It lies near the Na. band (yellow.) The mixed solution is then subjected to one of the separation processes, and as strong a solution in as deep a layer as possible, is examined with the spectroscope and the intensity of the α band is noted. This process is repeated until the most intense band has entirely disappeared. When I remind you, however, that the removal of the Di. by what was undoubtedly the best method known until lately, required a seven times repetition the Di. bands growing gradually fainter and dying out one by one until what had been the most intense finally faded entirely from view it will be seen that this fractional method forces one to the conclusion that possibly all the Di. has not been removed, but that a more powerful spectroscope in the hands of very experienced men might rediscover, in what appeared free from Di, traces of it yet present. This has actually happened more than once, and was what caused Prof. Dennis and myself to lay so much stress upon our new separation process, for the removal of Di. from Ce. We believe that the new method removes the Di. at once and entirely; for with the best spectroscope at our disposal we could find no trace of Di. The process in other words is not fractional but immediate. I can perhaps illustrate this better by an example, familiar perhaps to all of us. Ferro- and Ferri-Cyanides of K. long served as accurate tests of Fe., later Potassium Sulphocyanate was found to detect ferric Fe, when the Cynanides failed to do so. This reagent gives to a solution containing ferric Fe. in solution, a blood red or, in weaker solution, a wine color. When, however, some chemist proposed to add ether to the solution after testing for Fe. and failing to obtain a color, he found on closing the test-tube, and shaking violently that from a solution that was colorless after adding KCNS, a red color was extracted by the ether. This of course gives a very dolicate test for Fe., a delicacy unsought for a few years since. The Di. test is probably not so delicate at present.

But just here comes in one more of the evil features of the rare earth work, for the test that serves to prove the absence of Di must serve also as the test for La. which has no absorption bands, since La. being more strongly basic than Di., when the latter is known to be removed the former must have been previously gotten rid of. Of course the spark spectra could be employed, and, unless some easier method is discovered, must be employed in very accurate work, but it is tedious and requires special apparatus and precludes all workers, but those who have the advantages of the finest university laboratories, or are themselves wealthy. It needs, moreover, a much longer training than is needed to use the absorption band method. It is seldom employed as a test. The other method, applicable also in every case, but slow and requiring the very highest chemical skill to ensure results is to make equivalent weight determinations. This has so far been done gravimetrically, but methods are being sought by which it may be done volumetrically, which will be a great shortening, and in skilled hands, if the methods are good, will yield excellent results.

Having now given a fair idea of what the rare earths are, how they act chemically, and the difficulty of experimental work with them, I will proceed to speak of their occurrence, and then give some reasons for the immense interest any work in this line creates in the advanced chemical world. For a long time the earths were supposed to be what their name implies, really rare. There were reasons for this opinion. The earliest known specimens were among the last discovered in that period of intense chemical activity, the end of the 18th and the beginning of the 19th centuries, a time honored by such names as Lavoisier, Davy, Cavendish, Priestly, Dalton, Scheele, Berzelius, Vaquelin, Klaproth, and the elder Rose's. The lack of refined chemical methods, especially among those who had most to do with new minerals, the lack of sharp qualitative tests, and the fact that in ordinary analytical methods it was easy to mistake these for iron or alumina, all tended to the strengthening of this belief. Still during all this time the ablest chemical minds turned again and again to the subject, and from pure love of the truth sought for the solution of their mysteries. There is scarcely a great chemist who has not at some time attacked the knotty question, and seldom, as we must acknowledge, did they obtain other than negative results, and, as you know, these are seldom published—a mistake, by the way, as we could avoid many pitfalls and save valuable time did we know the experience of others along the same lines. When the discoveries of Mosander were published, new interest was created, and that indefatigable worker, Rammelsberg, better known possibly to the mineralogical than to the chemical world, examined many rocks for traces of these elements. Thanks to his efforts, seconded by Hermann, Wöhler and many other chemists, as their time permitted, and to the improved general as well as particular methods, the rare earths were found here and there and, we can now add, almost everywhere. It would now seem that like Fe. they are everywhere present, only in very small quantities. Zr. is lately, by microscopic method, proved to be present in every rock. Ce. is a common companion of Zr., and with Ce. there are always present La. and Di. and usually others. Norway and Sweden, the land in which they were first discovered, produce but small amounts of them now. In Brazil Monazite sand can be shovelled up on the seashore, it is a phosphate of Ce., La., Di., and Th. In Llano Co., Texas, Sipylite is found in considerable quantities, as also Gadolinite and other similar minerals. Along the Atlantic seaboard from Virginia to Georgia, in New Jersey and New York, in Massachusetts, in Renfrew Co., Ont., and elsewhere in Canada, in Colorado, along the Andes, in India, and Australia, along the Ural Mountains, in Germany, in England, and undoubtedly in many other places when thorough investigation has been made. From Dana's latest published textbook of mineralogy, the unabridged edition of 1892, I, a few years ago, made out a list of 62 minerals which contain the Ce. group, so-called, viz. :—Ce., Di., and La. Never did I find our own province recorded as having produced a single specimen. This I do not believe to be correct, when so many rare and peculiar minerals exist here, where there are rocks of every geological age, and where every one of the ordinary elements except the Pt. group has been found, I cannot but believe that the presence of the rare earths has been overlooked. I am not conversant with Prof. Hind's papers, but it would be interesting to know whether, in his numerous analyses of the minerals of the Province, he ever sought for the presence of the rare earths. It would not be surprising if he had, and yet failed to find them, for the methods of testing for them are not given in the ordinary text books of analytical chemistry and in the larger qualitative works of Prescott and Johnson, Fresenius, etc., very little attention is paid to them ; they are mentioned in footnotes or in fine print, and only the most advanced chemists are likely to pay any attention to them. This is partly because they are of little importance to the ordinary analyst, and partly because the field is so difficult ; and advanced chemists will of course go to the original papers. Still this all tends to the overlooking of these earths. A chemist might even take the B. Sc. degree, with Chemistry as his main subject, in any English or American university and know little beyond the fact of the existence of and the probable rarity of these elements. He might even obtain a Doctorate in Chemistry, and, unless his attention were especially called to the subject, know little of them. They are out of the ordinary line of travel. I am not saying this merely to fill in the time and make a long paper, as some may be tempted to think, but to show that, even if Prof. Hind did not look for these elements, and I am strongly inclined to think he did not, that it would not be casting any reflections on his skill as a chemist, nor slurs on his reputation as an analyst. They are considered out of the line of any one except the chemist who specializes along these lines. I need scarcely say that this is a mistake, to some extent at least. None but an advanced specialist in inorganic chemistry is likely to work with the earths, at least until more is known concerning them, but any ordinary chemist might easily look for their presence. I trust, if any especially heavy minerals or peculiar ones are known to members of this institute, the same being of provincial origin, they would apply the simple test I have mentioned—precipitation by oxalic acid in weakly acid solution, or send a small sample to me when I would be pleased to report the presence or absence of the earths. If the suspected mineral contains Di. a direct vision spectroscope will detect it at once by simply looking through it at the mineral. Now, finally, to give a little attention to what, according to my heading, ought to be the most important part of my paper. Why are the rare earths of especial importance to the chemical world? Why did such a chemist as Krüss give up so much of his too-soon ended life to their study? Why do Brauner, Nilson, Cleve, Boisbaudren, Debray, Crookes, and scores of lesser lights give all the time they can spare to solve the mystery? Why did Crookes, when a few pounds of Sipylite, so far a rare mineral, were found not long since in Texas, cable to reserve it all for himself at any price? Why did chemists like Marignac and Bunsen in the latter part of their life, with all their vast accumulations of scientific knowledge and their tried analytical skill, give their finest work to the unravelling of this problem? For two reasons chiefly. The desire to discover the truth, the aim of every true scientist, coupled with the knowledge that here was a field to test the mettle of the bravest and ablest, but also, and perhaps more important for proving the falsity of, or on the other hand, rounding out the periodic system of the elements.

The scientist ever seeks to bring the subject which he studies under the power of mathematics. He recognizes that this is the most powerful of instruments with which to work. All branches of science have, however, to pass the observational stage before laws can be deduced and classifications brought about. Though several chemical facts can be discussed mathematically, the subject as a whole is but emerging from the observational stage. Botany is still in this stage as also Bacteriology, the latter, of course, far behind the former; and possibly it may be claimed that the former is nearer mathematical control than chemistry. Its classification is certainly superior, but its classification, at least the one now in use, is a superficial one and readily arrived at. Not so Chemistry. The atom and even the molecule, evades our grasp and laughs at our skill, the balance alone conquers them and even here we grasp them but lightly. Their existence, even, is being disputed so evasive are they, and those who would claim their existence are confronted by metaphysical reasonings to prove them only hallucinations. It has been a long and weary search since Dalton propounded his atomic theory but the reward seems nearer. Thompson, or I should say Lord Kelvin, is fixing limits for their size and weight, while Mendelejeff and Meyer have propounded a theory of classification. The subtle points will yet be chained and their properties scrutinized.

For a long time, ever since chemistry became a science in fact, the need of a proper method of classification has been felt. There was no order, no opportunity therefore to apply mathematics, there was no comprehensive and easy means of grasping the subject, each element and almost every compound must be studied by itself. So greatly was the need felt that, whenever a new property common to a few or several elements was noticed, attempts were made to make it the basis of a classification. Berzelius thought he had discovered a method but this was soon found wanting, not being founded on sufficient data; when Faraday discovered the relations of the elements to the poles of the electric battery it was supposed to be settled, but this soon showed itself as a common property for all, extending from one end of the list of elements to the other and giving no special point where it could be said one class ended and another began, it soon resolved itself into the older metals and non-metals, basigens and acidigens and so failed,—it was founded on the too narrow basis of a single property. Inklings of the truth were, however, obtained from time to time. Dobereiner seems to have made the first suggestion which has led to the present system. He classified many of the elements into triads, taking as a basis a property certainly common to all, viz,—weight. He first noted that in many cases the weight of one element was the mean of that of two others usually resembling it, secondly, in other cases three elements with very similar properties possess very nearly equal atomic weights, viz,—Li., Na., K., and S., Se., Te., for examples of the first and Fe., Co., Ni., and Rl., Rh., Pd., as examples of the second. The comparisons were continued by Pettenkofer, Dumas and others, clearer and still clearer signs of universal order appearing as the atomic weights were more and more accurately calculated. Newlands was able between 1860 and 1866 to arrange the elements in octads, but the gaps were so many and the table so fragmentary, and moreover so many elements were forced to stand aside that his friends jocularly suggested that he try arranging the elements according to the first letters of their names. Had Newlands possessed the full courage of his convictions England would have received the credit of the greatest advance in Chemistry since Liebig and Wöhler founded Organic Chemistry. It was left, however, for bolder minds. Lothar Meyer, and

Mendelejeff attacked the question more courageously. The former was probably the first in the field, but the latter must be considered the true parent of the system, for while the former made up a table and pointed out many resemblances between the elements, some of which indeed escaped Mendelejeff, the latter not only proposed a table of the elements, but boldly altered the atomic weights of certain of the elements when they did not conform to his table, and did not merely lay them one side to await what the future might decide concerning them. He did more than this, he said in effect "My classification is correct but there are many spaces where elements are wanting; this does not effect the table it simply means that some elements are as yet undiscovered. I shall describe three of these," said he, "and without claiming to be a prophet, will indicate where they are likely to be found." He named them provisionally Eka-boron, Eka-silicon and Eka-cadmium. Within a few years two of these were discovered and their properties agreed almost identically with those which he had suggested. He altered the atomic weight of Ce. from 92 to 140, U. from 120 to 240, and made other changes. His prophecies were unnoticed or jeered at; his suggested changes were ridiculed. He fought his cause single-handed but his triumph was complete, and came quickly. In less than ten years from his announcement of the Law, the specific heat of Ce. was redetermined by means admitting of very slight error, and the atomic weight was proved to be 140 or nearly so, much nearer 140 than 92. Uranium was by the same means soon proved to have the proposed weight, viz,—240. Chemists then began to examine his predictions more respectfully and were soon surprised (if chemists are ever guilty of surprise) when in 1879-80 Nilson, followed by Cleve, proved the existence of Eka-boron under the name of Scandium; and when in 1886 Winkler proved Eka-silicon to exist as Germanium, Mendelejeff's triumph was complete. Few now doubt the truth of the law, and it has become a powerful weapon in the hands of the investigator. The line of the classification is complete, the actual basis is probably not known as yet, it may be the atomic weights, as is most usually assumed; it may be a common element as is being quietly proposed, though as yet unsupported by experimental evidence; it may be some property as yet unsuspected but that the order is nearly or quite correct no one doubts.

But where comes in the importance of the rare earths? A glance at either form of table will show blanks. No one doubts that these will be filled in. Whence? Undoubtedly in most cases from the rare earths. Ni. and Co. according to Krüss' work seem to conceal an element which may be found to have an atomic weight of about 100 and the earths conceal many. Within a few years Di. has been split up, one component showing absorption bands and the other failing to do so. As already pointed out, two earths once considered simple have yielded at least twelve and when the means of the separation, when the reagent or method is finally found, then the vacant spaces will be filled.

But it is not only that the rare earths will probably fill these vacant spaces in the table that gives them importance, their similarity is such in regard to action towards reagents that they seem to run contrary to the law. If so many of them are of the formula $R_2 . O_3$, they cannot be distributed over the table but will mass in groups and destroy the table. Of course if the table is incorrect the sooner it is proven the better so that the mind of the inorganic chemist may be directed elsewhere for comparisons, and it is just possible that in this very thing lies the importance. Still the periodic law seems to rest on good foundation.

The great importance then seems to lie just here. These rare earths exist, of this there can be no doubt. The best chemical skill that the world has possessed have been working upon them for over a century, and have so far been unable to confidently state their number and actual properties. The more work that is put upon them the greater the number of them seems to be. If the ones now claimed are all real there is not room for them in the law, *i.e.* spaces are wanting for their apparent weight. Until this question can be settled a mystery hangs over this portion of the Periodic system. The unravelling of this may work an entire change in our ideas of the elements. Their subtle resemblances have suggested to me more than once, while pondering over them, that in these lies the key to the simple elements which many chemists believe to be the foundation of the so-called elements. As in the Marsh-gas series the time comes when the Hydrogen-Carbon chain becomes too heavy for the bonds or affinity to sustain the weight, so in our inorganic field something of the same kind may result. The hypothetical elements may in certain numbers of atoms or in certain arrangement of atoms yield such similar properties that the one compound is distinguishable with difficulty from another. Time and high chemical skill alone can unravel the mystery, but so long as things remain as they are there remains an element of uncertainty in the periodic law. We have fortunately the key to the organic compounds and can read causes for resemblances and differences. The study of these has been of vast importance to the commercial world, but here is a field unwrought, not for want of workers, but by the very difficulty of the work challenging attack. I believe that here lies the key which once found will unlock many of the mysteries of the chemical world. And one thing is certain, the skill required to explain the mystery will give such power and grasp to the discoverer that he will with ease unroll the panorama of the elemental field and place it under man's open vision.

The unfolding of the mystery of the rare earths is not only necessary, then, to complete the Periodic system but they evidently conceal some chemical truth not known or imperfectly understood, and so not properly applied by chemists. Moreover, judging from the number of elements claimed as rare earths and their resemblance to each other, it is possible that they will overcrowd the Periodic system and compel its modification or rejection. In either case the examination will lead to large additions to the world's scientific knowledge, to *truth*, the aim of all true scientists.

The subject was discussed by several of those present, and a vote of thanks was presented to DR. MAGEE.

WATSON L. BISHOP, ESQ., exhibited a collection of Nova Scotian birds' eggs, and made remarks thereon.

SEVENTH ORDINARY MEETING.

Legislative Council Chamber, Halifax, 13th May, 1901.

The PRESIDENT in the chair.

On motion of PROF. E. HAYCOCK, seconded by PROF. E. MACKAY, it was resolved that the the N. S. Institute of Science recognize as subordinate branches, local organizations of its members in particular sections of the Province, formed for the purpose of encouraging scientific study and investigation; providing that such orgonizations are active and report yearly at the annual business meeting of the Institute. Members of such recognized branch societies who pay a yearly fee of one dollar to the parent society shall be entitled to all the rights and privileges of ordinary members of the Institute.

The resolution was referred to the council with directions to carry out the terms of the resolution, and to make any further recommendations thereon to the business meeting.

The following two papers were read by title :—

(1). Further Explorations in the Torbrook Iron District.—By EDWIN GILPIN, JR., ESQ., LL. D., &c.

(2). Discription of Fish-like Tracks from the fine-grained Siliceous Mudstones of the Knoydart formation (Eo-Devonian) of Antigonish County, Nova Scotia.—By HENRY M. AMI, ESQ., D. Sc., of the Geological Survey of Canada. (See Transactions, p. 330.)

PROF. ERNEST HAYCOCK, M. A., of Acadia College, then read two papers :—

(1). The Geological History of the Gaspereau Valley, N. S. (See Transactions, p. 361.)

(2). Fossils, possibly Triassic, in Glaciated Fragments in the Boulder Clay of King's County, N. S. (See Transactions, p. 376.)

These papers were discussed by the PRESIDENT and Messrs. A. MCKAY, POOLE, and DOANE.

A vote of thanks was presented to PROF. HAYCOCK for his interesting communications.

The following papers were then presented :—

(1). Phenological Observations for 1900.—By A. H. MACKAY, ESQ., LL. D., &c. (See Transactions, p. 379.)

(2). Rainfall Notes, Nova Scotia.—By F. W. W. DOANE, ESQ., C. E. (See Transactions, p. 399.)

The Council was authorized to receive as having been read by title, such papers as may be presented too late for this meeting. [Under this resolution a paper subsequently submitted by D'ARCY WEATHERBE, ESQ., C. E., on "Recent Developments with the Calyx Drill in the Nictaux Iron Field," was accepted by the Council. (See Transactions, p. 350.)]

HARRY PIERS,
Recording Secretary.

SKETCH OF THE LIFE OF J. M. JONES.

(See Frontispiece.)

John Matthew Jones was born at Frontfaith Hall, Montgomery, Wales, on 7th October, 1828. He was a son of Admiral Sir Charles Thomas Jones, K. C. B., his mother having been formerly Miss Jane Helen Satton.

In 1840 he went to Osmestry in Shropshire, England, a grammar-school under the superintendence of the Rev. Stephen Doane, and subsequently he received instruction from a private tutor, the Rev. John Whitly, rector of Wargrove near Warrington, Lancashire. He became a barrister of the Middle Temple, London, but being possessed of independent means, did not practice his profession. For some time he was a captain in the Royal Montgomery Rifles.

In June, 1850, while on his way with his brother to the latter's shooting-box in Scotland, he was wrecked in the steamship "Orion" off Portpatrick. Over one hundred persons were drowned, but Mr. Jones and his brother were among those who were saved.

He came to America about 1854 with his eldest brother who was flag-lieutenant to Admiral Milne, intending to shoot in the Rocky Mountains. He landed at New York, but was only able to proceed as far as London, Ontario, when an outbreak of cholera forced him to go to Halifax. He finally decided to reside in the latter town where, about the same time, his relative the Earl of Mulgrave, was stationed as governor.

He spent some time in the Bermudas, where his researches into the natural history of those islands resulted in the publication about 1859 of a volume entitled "The Naturalist in Bermuda."

At Halifax he resided for some time at "Ashbourne," a charming country place surrounded by fields and woods, at Dutch Village not far from the city. Near him lived the late Andrew Downs, well-known as an ornithologist, whose grounds were arranged as a zoological park; while in the city were several men who were beginning to take a keen interest in the study of the natural history of Nova Scotia.

In this country home, Mr. Jones's opportunities were excellent for observing nature and making extensive collections of the fauna of the province, to the investigation and gathering of which the greater part of his time was given. At "Ashbourne" he had a private museum in a building erected for the purpose, and in 1866 the number of specimens it contained was estimated at from seven to eight thousand.

He was an enthusiastic collector, and the cabinets of the British Museum, the Smithsonian Institution, and the Provincial Museum of Nova Scotia, were enriched by his generosity. He took great interest in the international exhibition at London in 1862, the provincial fisheries department being placed under his management.

During the winter months Mr. Jones usually resided in Bermuda, at his place called "The Hermitage," Smith's Parish, and gave further attention to the study of the natural history of that locality.

Mr. Jones married Mary, daughter of Colonel W. J. Myers of the 71st Highlanders, of Halifax, by whom he had seven sons and four daughters.

He was a Fellow of the Linnean Society of London, an original Fellow of the Royal Society of Canada, and one of the founders and ablest supporters of the Nova Scotian Institute of Natural Science, of which he soon became president.

Mr. Jones died on his sixtieth birthday, 7th October, 1888, at 114 Tower Road, Halifax.

Among his publications may be mentioned the following :—

The Naturalist in Bermuda; a sketch of the geology, zoology and botany of that remarkable group of islands; together with meteorological observations. Illus. London, 1859 (?), pp. 192.

Extract from the Bermuda "Royal Gazette," relating to the recent capture of a large species of Gymnetrus.—*Proceedings of the Zoological Society*, (Lond.), 1860, (part xxviii), pp. 185-187.

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A Fortnight in the Backwoods of Shelburne and Weymouth. *Ib.*, vol ii, pt. 1, pp. 48-60; 1867.

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With Goode (George Brown), ed.—Contributions to the Natural History of the Bermudas. Washington, 1884.

H. P.