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History of Geology

George Frederic Matthew: Invertebrate Paleontologist

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George Frederick Matthew (1837-1923) was a highly competent amateur geologist and paleontologist in Atlantic Canada. Though his formal schooling ending after grammar school, he became a self-taught acknowledged authority of the geology and fossils in the maritimes. His contributions to the knowledge of the earth's history in the area surrounding his home town of Saint John led to world-wide recognition and membership in a number of scientific societies.

Last year marked the one hundred-fiftieth anniversary of the birth of George F. Matthew, an important figure in the history of Canadian geology and paleontology. A native of Saint John, New Brunswick, his scientific career spanned over seventy years, from 1851 to a few months before his death from pneumonia on April 14, 1923. During that time he became without a doubt one of the foremost authorities of the geology and the fossils of the maritime provinces, particularly of the area surrounding his home city of Saint John.

George Frederic Matthew (Figure 1) was born on August 12, 1937 to George and E. Eliza (Harris) Matthew of Saint John, New Brunswick. A pharmacist, the elder George Matthew was able to afford the expense of six to eight dollars per quarter to send his son to the Saint John Grammar school (Miller, 1987). This proved to be the extent of Matthew's formal education, however, and in 1853, at the age of sixteen, he entered public service in the Saint John Custom House, eventually advancing to the positions of Chief Clerk in 1879 and Surveyor in 1893 (Bailey, 1923a). Matthew remained with the Custom House for sixty years, retiring in February 1915. Of Matthew's career with the Custom House, his long-time friend and fellow geologist Loring Woart Bailey wrote that "had it not been for political considerations in which he played no part, there can be little doubt that he would have been made Collector of the Port, a position which it was generally felt should have been offered to him" (Bailey, 1923a, p. viii) Bailey noted, no doubt correctly, that Matthew's position as a clerk in the Custom House was not one which would offer much time or opportunity for the scientific investigations which had already begun to claim so much of his attention and which would eventually bring him much distinction.

In 1851 (Parks, 1922), two years before he began working at the Custom House, Matthew began his studies of the rocks of Saint John, work which ultimately led him to publish (1863) a revision of Dr. J. William Dawson's (later Sir William Dawson) subdivisions of the strata of St. John County. This was Matthew's first scientific publication and it marked the emergence of a competent native geologist in Atlantic Canada. I do not mean to imply, however, that Matthew was the first geologist to examine the formations in the maritimes, for he was not. Abraham Gesner had done so many years before, in the 1820s, and he was followed by Dawson in the 1840s.

Gesner's work may have influenced Matthew to take up his own studies in geology. Gesner, New Brunswick's first Provincial Geologist, opened a museum in Saint John in 1842, exhibiting his own collection of over 1500 minerals, fossils, and rocks. (Miller, 1987) This exhibit stimulated a number of young men, Matthew and Charles F. Hartt among them, to form the Steinhammer Club, an organization which was devoted to the study of local rock formations. The Steinhammer Club worked closely with Dawson, contributing information for his works on

Figure 1 *Portrait of George F. Matthew. (Courtesy New Brunswick Museum).*

Acadian geology, and it was Dawson who advised the members to organize a natural history society in Saint John. On January 29, 1862, the Natural History Society in New Brunswick was founded, with Hartt and Matthew among the Charter Members (Botsford, 1888).

Matthew's interests were varied. As a charter member of both the Natural History Society of New Brunswick and the Royal Society of Canada he contributed dozens of articles to the Natural History Society's *Bulletin* and *Transactions* of the Royal Society of Canada on a variety of subjects, ranging from geology to archeology and the study of the artifacts of Native American peoples. Bailey notes that Matthew was also an accomplished botanist, and indeed in 1884 Matthew donated his herbarium of over two thousand species of foreign plants to the museum of the Natural History Society of New Brunswick (Botsford, 1888). Matthew's main interest was geology, however, and the vast majority of the many papers of which he was author in the *Transactions* of the Royal Society dealt with two subjects: the fauna and correlations of the Cambrian system as revealed in New Brunswick, and the age and relations of the plant-bearing beds found in the vicinity of Saint John and elsewhere along the Bay of Fundy Coast (Figure 2). Interest in his paleontological work leads me to focus on these writings, in particular those having to do with invertebrates.

Paleontology and Development

As a paleontologist, Matthew practiced a rather straightforward method of systematics based on morphology as exhibited in the fossils. His dozens of papers on the trilobites and brachiopods of the St. John Group are filled with descriptions of their physical characteristics. Reading through his papers, one gets an idea of the difficulties he faced. One of the first things that strikes the reader is the extremely small size of most of the fossils with which Matthew dealt. Most of them were minute, often no more than a few millimeters in any dimension. The only truly large specimen was that of the giant trilobite *Paradoxides regina* (Figure 3). This fossil, which was found by William Diller Matthew, the son of George Matthew, was over two feet in length, with an estimated total surface area of about 117 square inches (Matthew, 1887). Unfortunately, this specimen, after surviving one fire in the Custom House, was largely destroyed, along with Matthew's scientific library, when his residence was consumed in the Great Saint John Fire of 1877 (Bailey, 1923b; Miller, 1987).

Aside from the difficulty presented by the small size of most of the fossils, there were other problems as well. As Matthew expressed it:

"In the study of the fauna of the St. John group, the writer has found the investigation of these small crustaceans replete with many difficulties, partly on account of the imperfection of the material collected for study, partly from the variability of some of the species, and partly from the difficulties thrown around the subject by descriptions of species based upon material more imperfect than his own. The difficulties incident to the treatment of zoological questions connected with the earliest deposits of the Cambrian age — owing to the alteration to which most of the sediments of this time have been subjected, and to the poverty of the fauna in most districts — are obstacles to the prosecution of these researches; ..." (1887, p. 123-124)

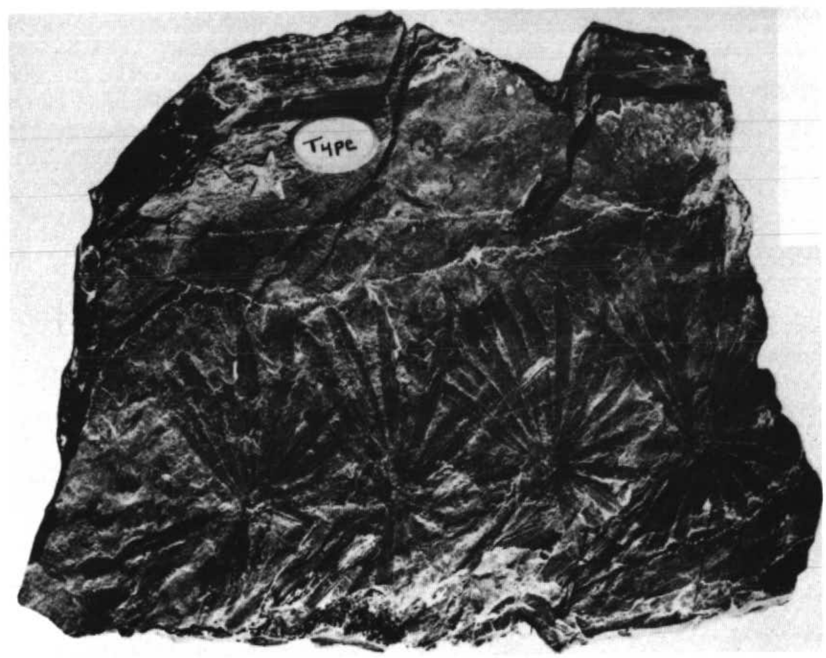

Figure 2 *G.F. Matthew also studied plant fossils, such as this specimen of* Annularia longifolia leavitti *from the Pennsylvanian strata near Saint John, New Brunswick. Courtesy New Brunswick Museum, Photo #NBMSP 0018, Catalogue NBMG 3423.*

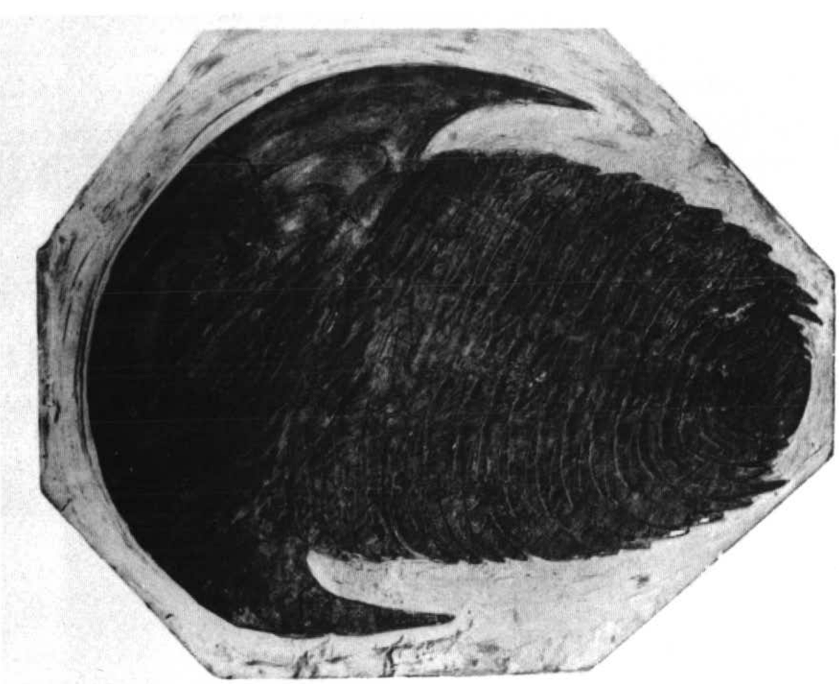

Figure 3 Paradoxides regina *Matthew, Cambrian, New Brunswick. Courtesy New Brunswick Museum, Photo #NBMSP 0014, Catalogue NBMG 4004.*

Matthew attempted to overcome these difficulties by various methods, including searching for the best specimens he could find, seeking out original types when he was able to do so in order not to have to rely on poor descriptions, and making detailed descriptions of what he himself found. In his descriptions he attempted to incorporate not only all the observable physical differences between specimens, but also the embryonic development of the various species, as far as it could be reconstructed. He also attached great significance to the geological strata in which the fossils were discovered. I shall examine these elements in turn.

In the trilobites Matthew relied on a number of physical characteristics, the most important being: the position of the eyelobes; the presence or absence of various sutures in the shields; the size and shape of the glabella and pygidium; the presence and size of spines, if discernible; and the number of segments in the thorax. When classifying the brachiopods he had many fewer structures upon which to rely. Notwithstanding this, he examined the fossils for external features such as the shapes of the ventral and dorsal valves and their sculpture. However, Matthew was well aware of the limits imposed by this system, and welcomed works which described the internal features of fossil genera. His appreciation of the value of such work is clear, as he wrote:

> "Depending, as paleontologists have had to in the first case, on external features as the means of determining the genus, a number of species have been referred to *Obolus*, which, when better known, were found to exhibit important points of difference, these were chiefly in the moulding of the interior surface of the valves." (1902, p. 93)

Matthew thought that he could discover the "natural classification" of the various organisms through attention to these features. By "natural classification" he seems to have meant a classification which correctly represented their evolutionary development. In his efforts to discover this, Matthew placed great emphasis on the embryonic development of the organisms as illuminating evolutionary steps which he believed to be due to a "law of development". Such a law he saw to have been active in producing changes in organisms over time; he believed that the changes could be traced in the lives of the organisms, as exhibited in the fossil specimens attributed to various stages of development from larval to adult.

Matthew was aware that this method of seeking "in the embryonic characters of the young for the links by which a natural classification" (1887, p. 124) of the Cambrian forms might be obtained was subject to the hazard of uncertainty in tracing the life history of species through various fossils, which might be quite unlike in form. Matthew (1898a) observed that it was very often the case that larval forms of trilobite species are so similar that at early stages they cannot be easily distinguished with certainty, though as they become older distinctive characteristics make their appearance. He viewed this from an evolutionist's standpoint as exhibiting the phylogenetic heritage of the organisms. Consequently, he classified the different genera according to their adult forms as being more or less primitive or advanced in the different classificatory characteristics, by reference to how much the adult form had changed from the larval form. Therefore, when he came to classify the Cambrian trilobites of the St. John Group, he ranged them into four basic groups: "(1) Those which are devoid of eyes and have a thorax of a few segments only; (2) Those which lack eyes, but have a long thorax; (3) The smaller species possessing eyes; (4) The Paradoxides" (1887, p. 160). His method of determining which features were more primitive or more advanced was quite simple — he relied on the fossil record as exhibited in the geological strata, assuming that forms which appeared in the early strata were more primitive. Such a method put a great deal of weight on the determination of the correct chronology of the stratigraphy.

As he examined the younger forms of the different species of trilobites, Matthew thought that he could discern a certain amount of plasticity in the larval forms, along with a "readiness to vary". In a paper delivered to the Royal Society of Canada he spoke of the "importance of the early changes of form in the trilobites".

> "If the development of species, as exhibited in the embryonic and larval forms of *Agraulos, Liostracus* and *Solenopleura* ... be examined, the very great importance of these early stages, as showing the plastic condition of the organism in the initial metamorphoses is readily seen."

This "plastic condition of the organism" was only seen to obtain in the early embryonic stages of development, though:

> "One has only to note in the series of embryonic and larval forms how different the embryos are from the adult; and yet to observe also how soon the generic and even the specific types become visible in the larval headshield, to be satisfied that the main potentiality of development is in the embryo and the embryonic stages of the organism." (1887, p. 164)

Matthew viewed the different groups of organisms as having possessed differing capacities to vary, the capacity being roughly proportionate to how advanced the group was. For example, compare his estimation of the first two of his four principal groups of the trilobites mentioned above. His first group, those without eyes and possessing short thoraxes exhibited "the most perfect retention of embryonic features ... The younger tests in this group, so far as they have been observed, show little difference from the adult form", with a long glabella and a thorax containing only two joints. The second group, while still lacking eyes, was seen to have had such important differences of structure compared to Group 1, as in the many joints of the thorax and in the presence of the facial suture, "that we cannot fail to see a great advance in structural development ... This capacity for variation indicates the acquisition of powers of metamorphosis greater than those possessed by the simpler forms of the first group." (1887, p. 160, 163)

Such a tendency to vary according to a law of development was also discerned in the brachiopods. In its simplest form, this was expressed in a slow increase in size. He noted that, owing to the small size of the early brachiopods, this "peculiarity" in the forms of the successive faunas might be "easily overlooked", but that "in reality the change of bulk is quite noticeable, and in some cases is nearly as great as that observed in the species counted in the descent of the Horse from *Hyracotherium* to the modern Horse." (1902, p. 99)

Matthew arranged the species of the genera of the brachiopods in the order in which they were known to have appeared in the Cambrian period, and decided that careful measurement revealed "a decided, though not very great increase in bulk, is observable". He admitted that there was a possible source of bias in the comparisons due to the fact that more abundant collections have been made from the higher Etcheminian terrane than from the Coldbrook terrane (two of the lower divisions of the Cambrian system in Acadia); since the size of the largest valves is recorded in the description of the species, this would give the species from the higher terrane an advantage regarding comparative size. However, Matthew felt that "even after eliminating this possible occasion of a greater than the natural difference, there remains enough variation to prove an increase in size of the Brachiopods as time went on." (1902, p. 100-101)

Because he was looking for such rules of development, it is not surprising that Matthew also found "synthetic forms" and "closed forms". Synthetic forms were those which seemed to link two species or genera. Matthew (*e.g.*, 1898a, b) gave examples of synthetic forms especially in the brachiopods, sometimes discovering them in the embryonic development of species and sometimes in the adult forms. Synthetic forms could be interpreted as the precursors of later genera and species, quite in line with the law of development. Closed forms presented more of a problem, for they seemed to represent the termination of development, or even its regression; they were apparent exceptions to the law of development, where development seemed to have run into a dead-end.

As an evolutionist Matthew believed that synthetic forms actually represented precursor forms of later species. It has already been noted that he observed that the embryonic forms of different species of trilobites can sometimes be all but impossible to distinguish. In 1898, he wrote of two such species which he had recently discovered in the rocks of the Kennebecasis valley.

> "Another interesting discovery made in this valley was finding of a type of trilobite in company with *Agnostus pisiformis, L.*, which showed peculiar phases of development. In this type were contained two species which, so far as the headshields are concerned, could not in the earliest stages be distinguished from each other. They first become separable by their pygidia, which in one species takes the form of *Olenus* and in the other that of *Anomocare*. It would thus appear that from the one phylum or stem-form two genera were developed ...
>
> "The distinction of the headshields of the two species comes at a later period of growth. Both, in the younger stages (though not in the youngest) have an apical spine on the margin of the headshield; this spine continues on the headshield of the Olenoid form, whose growth is arrested, so that it never reaches the size of the Anomocaroid form. In this latter the apical spine is gradually absorbed, so that in the final moult there is no trace of a spine in the front of the shield.
>
> "We see from this example how not only two species, but two genera, may be developed from the same root-stock. That is, the differentiation may produce two separate genera direct from the phylum, without the indirect method of producing separate species before the generic characters appear." (1898b, p. 40, 42)

Matthew related the differentiation of these species as due to "an accelerated development, in which some stages in the growth of the pygidium were passed over" in the smaller form. He was well aware of the implication of such findings for existing classification schemes if it were true that "not only were two species produced directly from a common stock, but these species should be assigned to different genera". He wrote that:

> "If an Olenoid type can thus originate independently from a phylum which does not show any direct relation with the genus *Olenus*, we may surmise that the *Olenus* itself had an independent origin, and that the Leptoplastids, formerly associated with *Olenus* are another group of later and independent origin. Indeed, it seems as though many of the family groups of trilobites now in use will have to be reconstituted from data of the larval forms, as Hyatt and others have reconstructed the old groupings of the Ammonites." (1898a, p. 146)

All the same, such statements also indicate that Matthew did not really grasp the significance of Darwinian evolutionism for classification, since it implies that there is no real difference between the neighboring hierarchical ranks as genera and species or species and varieties during their emergence. Instead, every new "genus" would originate simply as a new variation, so that there would be no reason to be surprised at the production of "two separate genera direct from the phylum, without the indirect method of producing separate species before the generic characters appear", as Matthew (1898b, p. 42) put it.

Use of Stratigraphy

Since we have seen that, on the one hand, Matthew believed in the evolution and transmutation of species and thought it important in constructing the "natural classification" to try to take the likely evolution into account, but on the other hand he classified individual species primarily on the basis of their physical characteristics, the question arises of how he drew species boundaries, if he felt that species themselves were subject to change. This is still a problem for modern-day paleontologists and the basic methods remain much the same, though they are greatly enhanced by modern techniques (Clarkson, 1986). It was at this point that Matthew made use of stratigraphy, for to a large degree his taxonomy depended on the strata in which various forms were found.

It had been known since the end of the eighteenth century that different geological strata exhibit fossils of different animals, and one of the most common methods of determining the relative ages of the strata was to compare the ratios of species of marine fossils exhibited to those still extant. Strata exhibiting higher ratios of still extant species were assumed to be more recent. One can easily see that such a method depends on the ability to determine species boundaries and assumes a high degree of fixity of species, at least as far as their external forms are concerned. Fossils which are very similar but not quite identical present problems to such a methodology. Should they be regarded as mere varieties or as separate species? Does it make a difference if they are found only in the same strata, only in different strata, or in both? Such questions are relevant to species boundaries.

Matthew had to deal with these concerns, and in a few cases he made explicit statements strongly relating species and stratigraphy. As a geologist, Matthew followed the customary method of dating the strata on the basis of the fossils found, and one of his central concerns was to show the relationship of the geology of Acadia to that of Europe. As a paleontologist, he was particularly pleased, then, to have at his disposal near Saint John what he felt was "a formation containing the fullest representation of the oldest Cambrian fauna yet discovered in America" (Matthew, 1885, p. 97). In his investigation of this formation, Matthew continued the work begun by Charles F. Hartt, and consciously related his efforts to those of Joachim Barande in Bohemia and J.W. Salter and Henry Hicks in Great Britain; he was delighted whenever he could report the discovery of links to their regions (*e.g.*, Matthew, 1898c). To do this, he attempted to define the various species and by means of them determine the progression of the strata.

An 1890 paper gives a good idea of both the complexity of the problem that he at times faced and the method that he utilized to resolve the question. Writing for the Royal Society of Canada, Matthew referred back to his first papers he had presented on the fauna of the St. John Group and how he had then given "an outline of the succession of members, as far as it was then known". He wrote that the outline had been based on the work of the Geological Survey of Canada done in previous years, and that he had "stated that the measures of Divisions 4 and 5 mentioned therein resembled parts of Divisions 2 and 3". Matthew continued:

> "Neither the officers of the Survey, nor the present writer, at that time, had the data sufficient to determine whether these resembling sets of strata were of independent and successive origin, or were the same measures repeated by faults or folds of the strata." (1890a, p. 123)

As long as he had been engaged in the study of the *Paradoxides* fauna, it had not been of "pressing importance" to determine the succession of the higher members of the Group. "But from time to time," he went on, "as the study of the fauna progressed, fossils were found in these higher beds, and it became desireable [sic] to ascertain what the natural succession of the folded and crumpled strata of the St. John Basin actually had been." To that purpose, he devoted some time in 1888 "to the determination of this point, so important to a proper understanding of the relations of the later faunas of this group", since as far as he was aware "no one heretofore has attempted to unravel the intricate plications of the St. John Group itself." (1890a, p. 123-124)

His method was basically one of comparison, searching for fossils which could be related to those in other geological formations which did not exhibit such a complicated structure. He noted that the fossils which were used as indicators of higher horizons in the St. John Group than that of the *Paradoxides*, "were not all found in the St. John Basin at first, though most of them were at a later period". They were first discovered in "isolated points in other basins, and their relation to the zone with *Paradoxides* did not always at first appear". Once they had been found however, "it became necessary to study out the succession of beds more carefully, else the paleontology and apparent succession would have been in conflict." (1890a, p. 124)

The task was not eased by the fact that the rocks of Division 2 in the St. John Group contained very little in the way of organic remains, and thus gave "very little aid in determining the succession". But Matthew wrote that

> "by observing the mechanical marking on the beds, and those due to various animals, as well as the lithological aspect of this part of the group, many difficulties arising from the conflict of the apparent succession with what proved to be the actual succession, were cleared away. In the shallow-water flags, which form the greater part of this division of the group, frequent ripple-marked layers, and the trails and burrows of worms and tracks of animals, show the difference between the strata which are merely inclined, and those which are overturned." (1890a, p. 124)

As a result of the information that he obtained through the study of such markings, along with that gained by comparing the fossils found in various strata with their succession in other areas, Matthew determined that "only Division 1, 2 and 3 are valid members of the series, marking successive periods of time. The higher numbers 4 and 5 are repetitions of one or other of the above (parts of Divisions 2 and 3)." (1890a, p. 124)

An Attempt at a Synthesis

It is time to attempt something of a synthesis of Matthew's views on evolution and speciation. Matthew considered himself to be an evolutionist and, indeed, once wrote "I was an evolutionist before I saw any of Darwin's books." (Parks, 1922, p. 19)

If this was indeed true, it is also true that in his evolutionism he was not a Darwinian, but more of a believer in a "law of development" in accord with the school of orthogenesis. I have noted the strong relationship which Matthew drew between species and geological strata; it was clearly related to his ideas on evolution and the development of species in geological time. I have also noted how Matthew viewed the fossil record as exhibiting evidence of a "law of development" in species. These lead me to speculate that Matthew probably thought in either neo-Lamarckian or weak orthogenetic terms. Peter Bowler (1983) has written that it was very common in the latter half of the nineteenth century for paleontologists in North America to embrace such forms of evolutionism rather than Darwinian natural selection as an explanation of the fossil record, especially when concerned with linear embryological development.

I have shown above that Matthew believed in a law of development which operated basically in terms of enlarging existing structures, though on occasion species could develop into a "closed form" and exhibit regression. On the whole, Matthew seems to have viewed development as a positive thing for the organism. I have seen no mention of non-adaptive evolution in Matthew's papers, which leads me to say that his would have been a weak orthogenesis, since Bowler lists the emphasis on the possibility of non-adaptive evolution as one of the distinguishing marks of orthogenesis. Perhaps it would be best to label Matthew's views as, in Bowler's terminology, "orthogenetic Lamarckism" (Bowler, 1983).

Matthew was not very explicit about how he thought evolution occurred, but he did believe that climate had a great influence, and that at certain periods there could be very rapid development of different forms, though he considered most of the "plasticity" to belong to the embryonic forms. Matthew (*e.g.*, 1882, 1887) seems to have regarded climate as having a limiting or enabling function. Though he wrote a couple of papers in which he presented some thoughts on the "causes which may have effected the development" of the Cambrian faunas and the regional variations seen in various parts of the world, he tended to offer these thoughts more as "suggestions than positive opinions, as to the causes which have produced changes in the Cambrian faunas, or have led to their annihilation" (Matthew, 1891, p. 255). He speculated on the possible effects of the relative distributions of land and sea and the effects of this on the temperature. He noted also the differences in distribution of the various trilobite species when progressing southward from the higher latitudes in both Europe and North America and wondered if "this deficiency of the later species of *Paradoxides* in the more southerly district [could] be due to conditions of temperature of the sea, and would a sufficiently high temperature exclude the genus entirely?" (1891, p. 264)

The emphasis was on environmental conditions and whether or not they provided the possibility of development into the various species. In this also, Matthew would have been in agreement with the American neo-Larmarckian school or with the orthogenetic school, both of which gave some role to environment in stimulating changes in species (Bowler, 1983). Moreover, it is conceivable that there might have been some influence on Matthew by Alpheus Hyatt or Edward Drinker Cope, founders of orthogenetic Lamarckism, and Cope's disciple Henry Fairfield Osborn, an exponent of first orthogenetic-Lamarckism and then pure orthogenesis. Aside from reading Cope's and Hyatt's works, there is evidence that Matthew at one period maintained at least an occasional correspondence with Hyatt. In a paper discussing pteropods found in the St. John Group, Matthew referred to a letter received from Alphaeus Hyatt: "Prof. Hyatt," wrote Matthew, "had very kindly offered to advise me in reference to difficult points connected with the fossils of the St. John Group." (Matthew, 1885, p. 102)

Further, there could have been a connection in thought through his son William Diller Matthew, who studied in the geology department at Columbia University in the early part of the 1890s while Cope held the chair in paleontology there, and thereafter at Osborn's invitation, worked under Osborn as a scientific assistant in the department of vertebrate paleontology in the American Museum of Natural History (Gregory, 1950). There might have been a connection in this indirect fashion, but nothing I have seen in the published papers indicates one. Even if there was such an influence, it probably only served to strengthen Matthew's pre-existing ideas, since we have seen that he wrote of a law of development even in the early 1880s, long before the younger Matthew went to Columbia.

The Eozoon Controversy and a Precambrian Fossil

Before concluding, I would like to mention Matthew's involvement in the controversy surrounding the alleged but later discredited Precambrian "fossil" *Eozoon Canadense*. The specimen was discovered in the Laurentian rocks of the Ottawa valley by a worker for the Geological Survey of Canada, and first described in 1863 by Sir William Logan, director of the Survey (Merrill, 1924). On May 18, 1864, Sir William Dawson proclaimed the *Eozoon Canadense* would be "one of the brightest gems in the scientific crown of the Geological Survey of Canada". In an initial flurry of excitement, *Eozoon* was hailed by no less an authority than Sir Charles Lyell as "one of the greatest geological discoveries of his time" (Zaslow, 1975, p. 86-87). To Dawson's embarrassment, even Darwin took up the cause of *Eozoon*, using it as paleontological evidence for evolution in the fourth edition of *Origin of Species*, writing: "after reading Dr. Carpenter's description of this remarkable fossil, it is impossible to feel any doubt regarding its organic nature". It did become possible to doubt the fossil's organic nature, however, as subsequent editions of *Origin of Species* testify. In the fifth edition Darwin wrote: "it is scarcely possible to doubt", and in the sixth edition: "the existence of the Eozoon in the Laurentian formation of Canada is generally admitted" (Peckham, 1959, p. 515), as *Eozoon* came under increasing attack from mineralogists after 1865. In the face of almost unanimous opposition from the geological community of both the Europe and North America after 1888, Dawson continued to support the organic origin of the ill-fated *Eozoon* right up to his death in 1899. In the late nineteenth century, however, Dawson found in Matthew one of his few believers, one reason being that Matthew himself possessed a Precambrian fossil that he related to *Eozoon*.

Matthew first took a public position in favor of the organic origin of *Eozoon* on the occasion of his address as outgoing president of the Natural History Society of New Brunswick. He knew that the majority of the "leading geologists of America" were among those who doubted "the animal nature of the object which goes under that name [of *Eozoon*]". Nevertheless, he noted that most of the doubters had probably "not had the opportunity of studying *Eozoon* in the field", and continued

> "The mode of occurrence of *Eozoon* in the rocks in which it is found is such that one accustomed to observe the older organisms, resembling the reef-corals, can hardly come to any other conclusion than that *Eozoon* is of organic origin, and this irrespective of the evidence supplied by the microscope.
>
> "The outward appearance of *Eozoon* is so much like that of some stromatoporoid corals that if the object were found in Ordovician or Silurian rocks its organic nature would scarcely be questioned." (Matthew, 1890b, p. 29-30)

Matthew did admit, however, that it was remarkable that *Eozoon* should be the "only organism of its kind known", and that it was even more remarkable that "it stands almost alone as a solitary animal structure in the great system of rocks to which it pertains", *i.e.*, the Precambrian; consequently "anything therefore which goes to prove the existence of living beings prior to the Cambrian age is of considerable importance to the geologist, and to the naturalist as well". It gave him great pleasure, then to call the attention of his fellows "to the existence in your neighbourhood of remains of organic forms of an antiquity far antedating the Cambrian age", citing three examples of fossils found in the area surrounding Saint John — two different sponges and a coral-like structure he first called *Eozoon Acadiense*, (1890b, p. 30) but later renamed *Archaeozoon Acadiense* (1907).

Whether Matthew ever abandoned belief in the organic origin of *Eozoon* is unclear, but he never abandoned his belief in *Archaeozoon*, though he had to fight for its acceptance. As late as 1918, in a co-authored paper, Matthew and Loring Woart Bailey wrote of the question: "What are the evidences of Life in connection with the Precambrian rocks of New Brunswick?"

> "The answer is still negative with the possible exception of the upper or limestone group about St. John ... the only form, however, excepting sponges, as yet met with to which an organic origin has been definitely assigned is one to which Matthew has given the generic name of *Archaeozoon* ... but there are those who prefer to regard that origin to be mechanical and concretionary ..." (Bailey and Matthew, 1918, p. 112-113)

It would seem that even Bailey, Matthew's friend of so many years, harboured doubts about *Archaeozoon*.

Bailey was not the only one close to Matthew who remained doubtful. His own son, William Diller Matthew, by then a well-known and respected paleontologist in the United States, also harboured reservations regarding the alleged fossils. He wrote of the finds in Precambrian rocks:

> "In some of these Precambrian limestones have been found more less doubtful remains of organisms, *Archeozoon, Eozoon*, and others supposed to be related to calcareous sponges or algae. But they may be merely peculiar types of concretionary formations, and it has been held that these Precambrian limestones were of chemical, not of organic origin." (W.D. Matthew, 1980, p. 119-120)

In the end their doubts proved unnecessary; *Archaeozoon* was a good fossil of a stromatolite and one of the earliest valid discoveries of a Precambrian fossil anywhere.

Conclusion

To assess the methodology of a scientist is not easy. In the case of George F. Matthew the task is not made any easier by the fact that he rarely engaged in theoretical speculation. Nonetheless, his few statements about methodology reflect, first of all, an awareness of the difficulties that attend the work of paleontology and, secondly, his willingness to adopt any new method which might hold promise in his aim to relate the fossil fauna he found to that of Europe and to uncover their "natural classification". Thus, he relied greatly on fossil embryology and larval forms, as well as on what could be determined of internal structure. He was particularly limited by the extremely small size of many of the specimens with which he dealt, and it can be imagined that he might often have felt tempted to say: "Well, I think it looks like that!" However, the evidence is that Matthew was rather more cautious, since he was not averse to classifying a number specimens, especially of larval forms, as "doubtful", nor was he slow to reconsider and rename his specimens if he found better examples.

In assessing the importance of George Matthew, a number of points should be kept in mind. Matthew learned his geology and paleontology as an amateur; he was not a university man, nor was Saint John, New Brunswick then, a university town. Consequently, he did not originally benefit from the sort of technical education which might have been of assistance. Nevertheless, he was recognized by University men and professionals the world over as having contributed an immense amount of new information to science. Matthew was the recipient of several honorary degrees; the University of New Brunswick awarded him an M.A. in 1878 and an LL.D. in 1897, while Laval awarded him an honorary D.Sc. in 1894. He was awarded the Murchison Medal in 1917 by the Geological Society of London (Anon., 1917) His works were translated into numerous languages, including Russian, Swedish, French, Flemish, and Japanese (Bailey, 1923a) He became a Fellow of the Royal Geographical Society of London in 1865 and was elected a member of the Geographical Society of Belgium in 1914, besides being a Charter member of the Royal Society of Canada. Further, Bailey notes that Matthew received such distinguished guests as the "noted geologist/paleontologists Joachim Barrande (mentioned above) and Charles Barrois of France, who when journeying to the United States, detoured to S[ain]t John to see Matthew and view his work" (Bailey, 1923a, p. viii).

In contrast, his son William Diller Matthew was of the modern era and studied at Columbia under Henry Fairfield Osborn. When George Gaylord Simpson wrote a glowing memorial of the younger Matthew, who died in 1929, only a few years after his father, Simpson merely referred to George Matthew in a footnote, saying that he had been "an amateur of some ability". William King Gregory was more generous. In a Preface for a posthumous second edition of the younger Matthew's *Climate and Evolution*, Gregory wrote that "George F. Matthew, was an amateur geologist in the best sense, since he was a recognized authority on the geology, fossil plants, and early amphibian footprints of New Brunswick" (1950, p. vii). However, perhaps it is best to leave it in Simpson's words — if we remove the obvious bias against amateurs — for the professional judgement of Matthew's contemporaries was also, though meant entirely in praise, that he was indeed an amateur of some ability.

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Letter to the Editor

Dear Sir:

Subject: NSERC Peer Review — A Delicate Balance or Increasing Entropy?

In Volume 14 of *Geoscience Canada*, Professor Michael Church reflects on the mythology of various modes of research publication in the Earth Sciences, as well as how these publications are "valued" by the NSERC peer review system. Clearly it would be foolish to do other than agree with Professor Church that all peer review systems *must* concentrate on quality, not retreat into quantitative measures of how much was produced, nor assign "value" to the publication route to the exclusion of publication content.

How does the NSERC peer review system in earth sciences (ES) function? One of NSERC's responsibilities is to monitor the quality of peer review, being sensitive to the special characteristics of a discipline and the broad spectrum of research areas that must be reviewed by a single committee. Overall, the current ES Grant Selection Committee [ESGSC] peer review is judged to be extraordinarily strong and effective. Committee discussion centres around the significance of previous contributions, the overall level of research activity and the potential for future advances. The following passage extracted from this year's report of the [ES]GSC is witness to the committee's recognition that quality must be the central concern in an evaluation.

> "The committee continued to evaluate publication records in a thoughtful fashion, not relying simply on numbers of publications in refereed journals, but instead assessing the quality of the publications and their contribution to science. The Committee continues to demand regular and consistent dissemination of results, but is realistic and careful in allowing for the varying demands and rates of publication among the various subdisciplines as well as differences in the preferred publication venues (journals, monographs, conference proceedings, maps, etc.). The perception that exists within some parts of the earth science community that this is not the case represents both a failure in communication by the ESGSC and the innocence of some members of the community."

Can the peer review system be strengthened further? The answer must be "yes"; however in Earth Sciences such changes would be refinements, not major changes in philosophy or approach. The strength of this peer review process is that it is dynamic, aware of community needs and aspirations and continually searching for more insight, yet prepared to make the difficult subjective judgements demanded by NSERC. The committee works within tight budgetary constraints given by NSERC. This, coupled with the high quality of Canadian researchers competing for the limited funds makes for a tough decision making process. While such a process cannot please all, it is a pity that the public debate does not include a few more of the many supporters. Enough bricks; pass the flowers!

Yours sincerely,

Janet E. Halliwell
Director (Research Grants)
Natural Sciences and Engineering Research Council of Canada