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THE

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The Distribution and Physical and Past-Geological Relations of British North American Plants.

By A. T. Drummond.

(*Continuvd from page* 423.)

It is difficult to resist the thought, that many of the plants thus common to Europe and America have had their point of origin and centre of dispersion on the eastern side of this continent. Many interesting geological questions arise in this connection. America has an older look about it than Europe. Eastern Canada has afforded the earliest traces of the dawn of life on the earth. To come down to later times, the floras of the continent in the later Cretaceous and in the Eocene ages afford the first traces of resemblance to the flora of to-day. Some genera then appeared which have representatives at the present time, though, with rare exceptions, specifically different. The American Eocene flora is found to have a resemblance, not so much to the Eocene of Europe, as to the later Miocene

there, as if America were the starting-point of that phase of the vegetation, which, in its later developments, became the flora of to-day on both continents. Again, the first undoubted evidences of the flora of to-day, on any considerable scale, are found in the Leda clays of the Ottawa valley.

The geological structure of Ontario, Quebec and Labrador indicates that much of the areas included within their boundaries has been dry land for vast periods of time prior to the glacial epoch, and that within these areas are the oldest portions of the continent. We can then readily conceive that in tertiary times, this portion of the continent, being even somewhat warmer than now, was the home of vast numbers of the plants of the period. The American species, now represented in Europe, we cannot in Canada trace backward beyond the period of the Leda clays; but it is also clear that none of these identical species have as yet been met with in the tertiary deposits of Europe, nor have any, found in the Leda clays, been as yet observed in the European post-tertiary deposits. Seeing, then, that North-Eastern America, having been for so long a time dry land, has always been an available home for vegetation, that the Upper Cretaceous and the Eocene of America, in the resemblance of their flora to that of northern temperate America of to-day, are older than the European Cretaceous and Eocene, that it is only in later epochs in Europe that the generic identity with North American plants became so very distinctly marked, and that in Europe many of the genera of the Pliocene, identical with those of to-day, have since become extinct, there seems a possible presumption, quite apart from that derivable from their present range, that some of these identical European and American plants may be older in America, and, being chiefly northern temperate in range, may have originated in northern temperate America.

There are other interesting questions in this connection. The rounded or smoothed, often striated, character of the rocks, and the presence of the boulder clay and its accompanying boulders, would, if we admit the action of glaciers in the work, appear to prove the higher altitude of, per-

haps all of the Laurentian area in Canada, as well as of considerable portions of adjoining areas. I think it the most reasonable conclusion that the whole of this part of the country was of a similar character to what, speaking generally, British Columbia is at the present day, but on a greater scale,—mountainous and rugged, with everywhere high peaks and deep valleys, with frequent plateaus, and with lines of summits so continuous and so connected as to form extended ranges of mountains,—and that, with a somewhat colder climate, individual glaciers occurred everywhere on these mountains, and in their descent carried with them débris and boulders to the valleys beneath. Some of these glaciers would, as in the Rocky Mountains now, be of comparatively short length, and their action on the rocks beneath them and on the fragments displaced would be correspondingly light; others would, as in Greenland at the present day, be on an immense scale, extending for very many miles, and be often of great thickness. A universal ice cap over the whole country seems to me an untenable hypothesis, whilst a general mountainous character, with high peaks and ranges, down which glaciers would flow, would explain the phenomena met with at the present day, which are properly ascribed to glacial action. Even at this later day, the whole Laurentian country to the north and south of the St. Lawrence is of this rugged, mountainous character, with indications, as at the Thousand Islands at the outlet of Lake Ontario, that at one time there was a much greater elevation than now. In fact, the whole inner country lying between the estuary of the St. Lawrence and Hudson Bay is described by explorers as being of an extremely mountainous character—broken, rugged and impassable, as if the subject of some exceptional convulsion in former ages.

There are some of the phenomena of glacial action in Canada which go far to show that there also have been, subsequent to, but perhaps before the close of, the glacial epoch, extensive areas of depression, more particularly along and south of the Middle and Lower St. Lawrence and up the Ottawa River, and, perhaps contemporaneously, in the lake

region, and again, towards probably the close of the glacial epoch, and subsequently, over the vast country east of the Rocky Mountains, now occupied by the prairies, and extending as far as the shores of Lake Winnipeg. The eastern side of this lake, as I have shown in a previous number of this journal, probably formed the eastern coast of a vast inland fresh water sea. There are ample evidences on the prairies of more than one elevation and depression and of the existence of vegetation, during the former. Boulders, some of great size, have been transported immense distances, and this can only be explained by the action of icebergs floating, as at the present day, under the influence of winds and currents. We can even now trace the direction of the currents and of the prevailing winds in those far distant times, as well as of the force which gradually raised the land to its present level. In the great prairie country occupying the southern central portion of Canada, the greatest depression was at the base of the Rocky Mountains, whilst the existence of great boulders there of eastern origin, brought undoubtedly by icebergs, the great areas of sand at and south of the sources of the River Qu'Appelle, and the stretches of sand and the gravel ridges southwest and west of Lake Manitoba, all prove that the winds most prevalent, and probably the currents, were in a direction somewhat south of west. The elevation of the land to its present level was greatest at the Rocky Mountains and least towards Lake Winnipeg, and this has resulted in the flow of great rivers like the Saskatchewan and Qu'Appelle being at the present day in a general easterly direction. The great depth of soil over such a vast area as the north-west prairies, indicates either an immensely longer period during which the mountains and valleys to the northward were the subjects of erosion, or that the process of erosion was of a more severe character than in Ontario and Quebec, or that, in the latter provinces, subsequent depression under the ocean and inland seas has resulted in the carrying away of much of the soil. That there were inter-periods when the land was raised to some extent from beneath the sea, and vegetation appeared

upon it, is evidenced by the rings of black vegetable loam which appear in excavations made at Winnipeg for tanks and wells. It does not, however, appear to me necessary to assume that these were milder inter-periods. A northern temperate vegetation was already in Canada, I cannot avoid concluding. There is also some evidence of more than one depression during or after the glacial epoch in the Lower St. Lawrence valley, or of the renewed action of glaciers on the rising of the land there.

The hypothesis of a universal ice cap throughout Canada almost dispels the notion of any phænogamous flora in northern temperate America, or, at least, of any vegetation short of an extremely Arctic type. It assumes the gradual extermination of all northern and middle temperate plants in their native habitats, and the crowding of the species into a very circumscribed area to the southward, presently occupied by the south-temperate vegetation of the continent, of which crowding we have no evidence left, and which is hardly in accordance with existing possibilities. It also assumes the migration of the Arctic flora southward to at least northern temperate countries. Does not, however, the comparatively limited flora of the summits of the White Mountains and other considerable heights in New England and New York, comprising chiefly four or five really arctic and a few sub-arctic and boreal plants, nearly all also found on the coast of the Lower St. Lawrence, of the Gulf of St. Lawrence or of Labrador, show that the true Arctic flora had hardly reached as far south as these points? If, however, as I believe, there were only individual glaciers everywhere over the Laurentian and immediately surrounding country, on the high peaks and mountain ranges of that period, perhaps all of which are at much lower elevations now, it by no means follows that vegetation was entirely driven southward at this time. There could be a cold sufficient to produce glaciers on the mountain sides, and their resultant icebergs where, farther north, these glaciers met the sea, and these icebergs might be found even as far south as the New England States, for the cold Labrador current now existing would, without doubt,

have been diverted inward over the then or subsequently depressed New England area; but this cold, and even the added presence of this current, would not preclude the idea of vegetation. Judging from the analogies of to-day in Switzerland, in the Rocky Mountains, and along the Lower St. Lawrence, it would be quite within the range of likelihood that northern temperate and sub-arctic and arctic plants would be found in Canada at this early time, in those places most suited to them, and just as they at this later day occur even alongside of glaciers. Though glaciers may have been near at hand, it does not behoove us to too readily draw conclusions from them as to the climate and surrounding vegetation. There are glaciers in the Rocky Mountains in British Columbia, but they are not associated there with a general arctic climate, nor has the general flora of the mountains an arctic or even sub-arctic aspect. During the deposition of the Leda clays, which took place before, or on, the close of the glacial epoch—for their relative position seems still uncertain—and contemporaneously with the encroachments of the sea far up the St. Lawrence and Ottawa valleys, the vegetation had a northern temperate aspect. The marine fauna around Montreal, near Ottawa, and elsewhere, had, it is true, a northern, almost boreal, look, implying cold sea currents; and the presence of boulders in these great river valleys would indicate that glaciers flowed through or into them at this or an earlier period, or that icebergs had then found their way as far inland as these points. The presence of cold sea currents, or of even icebergs, was not, however, associated with arctic or even sub-arctic plants. In the Leda clays we have such species as *Drosera rotundifolia*, *Potentilla Norvegica*, *P. tridentata*, *P. Canadensis*, *Arctostaphylos uva-ursi*, *Populus balsamifera*, *Thuja occidentalis*, *Potamogeton perfoliatus* and *P. natans*—all species occurring now in the latitude of Montreal, and all but one in the latitude of Lake Ontario.

It does not, then, seem to me difficult to imagine the vast Laurentian country in Canada,—broken and rugged everywhere as it now is, and rising often to very consider-

able heights—elevated in these distant and somewhat colder times to far greater heights, forming extensive and numerous lofty mountain ranges everywhere, with successions of invividual peaks, on perhaps all of which ranges and peaks were glaciers. Nothing short of this will explain the results of glacier action, whilst, at the same time, it admits of there being a northern temperate flora in the valleys and on the plateaus, just as we now find occurs, though on a smaller scale and under somewhat different circumstances, in central and southern British Columbia and in Switzerland to-day. The sub-arctic and arctic plants would be on the higher summits and on suitable situations farther north, where the temperature was lower and the summers shorter, or on the lake and sea shores in lower latitudes, where the more equable temperature and cold sea levels would lead them.

The phænogamous plants common to Europe and Canada number 419, and adding to these the horsetails and ferns, the number is increased to 450. Prof. Asa Gray gives 320 species as the flowering plants common to the Northern United States and Europe. This number, however, presently includes *Lythrum hyssopifolia*, L., *Salicornia fruticosa*, L., *Narthecium ossifragum*, Huds., nine carices and grasses, and *Asplenium ruta-muraria*, none of which have been detected thus far in Canada. This indicates that there are about 112 species, chiefly arctic and sub-arctic, confined, in their American range, to Canada. An analysis of the Canadian species indicates how the following leading orders are represented :—

	Species.		Species.
Ranunculaceæ	18	Scrophulariaceæ	14
Cruciferæ	24	Naidaceæ	15
Caryophyllaceæ	23	Juncaceæ	16
Rosaceæ	19	Cyperaceæ	56
Compositæ	21	Graminæ	40
Ericaceæ	22	Filices	23

The representation of species is very disproportionate to the importance, at this later day, of the orders. The great order Compositæ, which now embraces about 390 species

in Canada, has only 21 of these represented in Europe, but this is in partial keeping with the fact that in the preceding Tertiary period, this order does not appear to have been even known. Leguminosæ—also an order of apparently no antiquity—which has 147 species in Canada, includes in these only 6 common to Europe. On the other hand, Caryophyllaceæ, out of 72 species in Canada, has 23 identical with European species, Naidaceæ out of 27 has 15, and Juncaceæ ont of 35 has 16 identical.

These species thus identical in Europe and America are, on the evidence we have, the oldest flora still existing in America. They undoubtedly existed in post-pliocene times, but to account for the migration of the species from one continent to another, it is necessary to have connecting links of land, and, at the same time there, a suitable climate for the distribution of northern-temperate as well as arctic and sub-arctic forms. To find a union of these two conditions, it is requisite to go back to pre-glacial times when a climate warmer than now existed in northern-temperate and arctic America and Europe, and when the relations of land and water in northern-temperate Europe, and possibly arctic America, presented more favourable facilities for migration.

General Causes of Distribution.

The general causes, still in force, which have affected distribution are well known. River, lake, and ocean currents, play their part in every section of the globe, not only in dispersing the seeds of aquatic plants, but also those of land species, which constantly come into connection with the water. Particularly would this be the case in Canada where the water communications are on such an extraordinary scale, both on the coast line and in the interior of the country. Birds form a constant source of distribution, going on for ages past. That seeds and fruits are the especial food of birds, that birds not only traverse great distances in search of food, but large numbers of them have semi-annual migrations, that the peculiar habits of birds

lead them into situations where seeds may adhere to their feathers or in mud to their legs and feet, and that great numbers of seeds are for the purpose of diffusion supplied by nature with means of adhesion to objects, whether birds or quadrupeds, with which they are brought into contact—are all of them circumstances, in progress for long centuries past, the one fitting into the other, which have been instrumental in the gradual and wide dispersion of many plants. The popular view of the economic purposes of fruits is that they are provided by nature as food for man and the lower animals. Perhaps an even more immediate purpose in their colour and flavour, is that they may attract birds and quadrupeds—as the colours of flowers do insects—and that the seeds, by being carried great distances in the crops and stomachs of these creatures, should thus have an important means of diffusion.

Wind is, however, the most important factor in distribution. Many plants have their seeds furnished with appendages to be utilized in connection with the wind, and such plants have a generally wider distribution than those not so furnished. The different species of maple, ash and pine, have what might be termed wings attached to their seeds, and these are undoubtedly thus provided that in falling at maturity, the seeds may be carried by the wind beyond the parent tree. The seeds of most of the Compositæ are supplied with plumes or awns which form an important means by which they are distributed, and thus this, in America, most extensive of the phænogamous orders is, though geologically recent, of wide diffusion. Whilst, however, the ordinary winds have their local effects in scattering seeds, it is to hurricanes and tornadoes, and even ordinary high winds, that we must look for the carrying of them to great distances. It is not difficult to suppose that most seeds can be so carried. Where the fruit is heavy, as in the case of the oaks, hickories, walnut, butternut, chestnut, plum and cherries, the range of the species is relatively circumscribed. The seeds of herbaceous plants generally are, however, light, almost feathery, in weight—a circumstance which like the awns and wings provided as appendages to many of them,

has been without doubt so arranged by nature that they may be readily distributed at maturity by the wind. The power of the wind as a distributor of the lighter class of seeds has been underrated. The rapidity with which new railroad tracks, extending into newly settled as well as old settled country, have become tenanted, not by plants from the neighborhood but by roving introduced plants, is a striking evidence of the action of winds. Most of these introduced plants, so common in cultivated fields, on roadsides and on compost heaps, as well as on railroad tracks, have seeds relatively light in weight, and provided in many instances with appendages to facilitate their dissemination. In districts where the forests have been burned over, the native plants with which the burned area is soon peopled, are generally of two classes—berry-bearing shrubs, the seeds of which have been deposited by passing birds, and plants like the Epilobium, birch trees and willows, whose seeds have wings or awn attachments, which not only prevent them from too quickly reaching the ground when they fall at maturity, but also afford a better opportunity to the wind to carry them to great distances. And these distances are not to be measured necessarily by an acre lot or by even the breadth of a township. Recent investigation has shown that volcanic dust, forced from a volcano during eruption, may float through the atmosphere for many hundreds of miles before descending. What may not be possible with light seeds when a gale of wind prevails! I can conceive it probable that the seeds of even arctic and subarctic, as well as other plants, may have found themselves occasionally by this means carried over great distances to high peaks, mountain ranges and plains, to the southward, where, on finding once more a favorable climate, soil and physical surroundings, they would, each in its appropriate place, germinate and develop. To the same cause would probably be attributable the occurrence, on the White Mountains, of plants like *Geum radiatum* and *Paronychia argyrocoma*, which belong properly to the alpine flora of the mountain ranges far to the southward. The great range of so many sub-arctic and boreal lichens, also found

on most high summits in middle and even southern temperate countries, seem to me, likewise, an evidence of the power of the winds in carrying spores to enormous distances.

Many of the special causes which have operated in Canada to influence distribution have been of a physical character—as, ocean currents, the enormous coast line with its peculiar configuration and its effects on climate, the lie of the mountain ranges, the vast stretches of prairie country quite divested of trees, and over a considerable extent of which the annual rain-fall is limited, the breadth and the cooling effects of the immense bodies of fresh water embraced in the inland lakes, and the general prevalence of fogs on the eastern coasts. All of these causes and others will be referred to in detail in their special places.

Divisions of the Flora.

Taking a general view of the whole flora of the Dominion, we can readily distinguish the following groups:—

Canadian Group.—Embracing numerous species very generally distributed over the whole country from the Atlantic to the Pacific, and northward more or less to the limit of growth of trees. They also occur in the Northern United States, but probably the great mass of the individuals of each species is rather in Canada than the United States.

Forest Group.—Comprising numerous species which range more or less from Nova Scotia and New Brunswick to the Rocky Mountains or towards there, but which appear to affect the forest country and to avoid the prairie, unless in those sections where there are extensive bluffs of trees, or in the river valleys. The species of this group do not cross the Rocky Mountains.

Maritime Group.—Species confined to the immediate sea shore, though several of them are also found along the Great Lakes and in the neighbourhood of saline ground farther inland.

Eastern Coast Group.—Comprising plants confined in

range to the eastern portions of the Province of Quebec and to the Maritime Provinces of Nova Scotia and New Brunswick, though this large area is more conspicuous by the absence of numerous species common in Ontario and Western Quebec than by the presence of a distinctive flora.

ERIE GROUP.—Including the large number of middle temperate plants found in the south-western peninsula of Ontario, and common to that area and to south-western New York, and to Ohio, Pennsylvania and other Middle States of the Union.

ST. LAWRENCE GROUP.—Embracing numerous species distributed generally throughout the St. Lawrence valley and lake region, but not ranging west of the wooded country immediately beyond Lake Superior and Lake of the Woods.

BOREAL GROUP.—Including in this those northern species which occur near the shores of the Lower St. Lawrence, around the coasts of Northern Michigan and Lake Superior and north-westward, and in many cases also found on the coasts and among the mountains, of British Columbia. Though intermingling sometimes with them, they are not sub-arctic plants, but generally occur where the deep waters of the sea or lakes or sufficient altitude supply a moderately low temperature.

ONTARIO GROUP.—Representing a considerable class having its maximum development in Canada in the Province of Ontario, but also occurring in the eastern and middle sections of the Province of Quebec, and southward in the New England and Middle States.

PRAIRIE GROUP.—Embracing species familiar more or less over the whole of the prairies of Manitoba and north-westward, but probably limited in range over the dry prairies of the western and south-western sections of Assiniboia.

WESTERN PRAIRIE GROUP.—Including species belonging to the dry prairies to the west of Manitoba where the rainfall is limited, and extending thence westward almost to the foothills of the Rocky Mountains.

WESTERN CENTRAL GROUP.—Including a very consider-

able number of species which range from Middle and Southern British Columbia to, more or less, the eastern confines of Manitoba.

Rocky Mountain Group.—Embracing the numerous plants, not alpine, which in our present knowledge of their range are confined to the valleys and foothills of the Rocky Mountains.

British Columbia Group. — Comprehending all those species which are distributed somewhat generally over, and are confined to, the Province of British Columbia

Oregonian Group.—Including under this the more southern plants found in British Columbia, and whose range northward, from Oregon and Washington Territory, has been facilitated by the general direction of the valleys in the Rocky, Selkirk, and other mountain ranges there.

Western Coast Group.—Including in this, not the shore plants, but those species which probably the rain-fall and other causes have confined to the neighbourhood of the coast and the adjoining islands, in British Columbia.

Sub-arctic Group.—Comprising species found on the higher hills and mountains in Eastern Canada and British Columbia, on Anticosti and the northerly coasts of the St. Lawrence estuary, on the more exposed points of Lake Superior and northward, and often intermingling far to the northward with the true arctic species.

Arctic Group.—Comprising a few rare representatives in the Alpine districts of New England, and on the Mingan Islands and Island of Anticosti and neighbouring coasts, but as a rule confined to the high northern coasts of Labrador and Hudson Bay, and to Greenland, the shores of Baffin's Bay, the Arctic islands and Lower Mackenzie River country.

(*To be continued.*)

Invaporation.

By W. L. Goodwin, D. Sc.

Professor of Chemistry and Mineralogy, Queen's University, Kingston.

The experiments noticed in the Record of Science, Vol. II., No. 4, Oct. 1885 (p. 259), have been continued since that

date, and new experiments of the same sort are in progress. As explained in the previous notice, the object of these experiments is to determine the relative force with which different soluble substances attract water. The process is the reverse of that of evaporation, water vapour being very slowly condensed from a nearly saturated atmosphere by substances soluble in water. Of course, condensation goes on more and more slowly as the solutions become dilute, so that at length, in some cases, years are required for the completion of the experiment. At present we are working with three salts, viz.: sodium, potassium, and lithium chlorides. In Series A, sodium and potassium chlorides are put in molecular proportions into small glass tubes, which are enclosed in a stoppered bottle along with a third small tube containing a weighed quantity of water. The proportion of water is varied for different experiments. The salts soon deliquesce, the sodium chloride more rapidly than the potassium chloride, and when the proportion of water is small, the water-tube soon becomes dry. It will be interesting to trace the progress of a few experiments of this series. *Experiment I.*, sodium chloride, 1.1672 grm.; potassium chloride, 1.4882 grm.; water, 1.44 grm. After 56 days, the sodium chloride has gained 0.8058 grm. of water, the potassium chloride 0.6292 grm. and the water-tube is dry. After 159 days the sodium chloride is found to have increased its quantity of water at the expense of the potassium chloride, and this process continues, until at the end of 314 days the potassium chloride is very nearly dry. A glance at the following statement will show the progress of invaporation in this case:—

	Days.	Water.	Days.	Water.	Days.	Water.	Days.	Water.	Days.	Water.
Sodium Chloride...1.1672g	56	0.8058	159	1.1978	172	1.2392	314	1.4207	410	1.4183
Potassium " ...1.4882g		0.6292		0.2332		0.1900		0.0072		0.0076

This experiment illustrates the decrease in the rate of invaporation as the solution becomes diluter.

In Experiment 2 of this series the proportion of water is doubled.

Series A., Experiment 2.

	Days.	Water.	Days.	Water.	Days.	Water.	Days.	Water.	Days.	Water.
Sodium Chloride....0.5836	111	0.9516	155	1.1160	276	1.3331	290	1.3386	431	1.2523
Potassium " 0.7441		0.4866		0.3166		0.0991		0.0976		0.0841

The potassium chloride still holds an appreciable quantity of water, and in all probability will continue to do so. Experiment 3, in which the proportion of water is again doubled, is still in progress. After 286 days the water is divided in the ratio of about 5 to 1. In Experiment 4, after 404 days, the salts have divided the water (again doubled) into approximately equal parts. We are watching this and Experiment 5 with considerable interest, and we expect that the latter will show a reversal in the invaporating power of the saline solutions, since very dilute sodium chloride solutions may have less invaporating power than corresponding potassium chloride solutions.

During the past year, we tried a series of invaporation experiments with the so-called "gem" and "crown" jars, hoping that they would be found to be air-tight. Two interesting results followed from experiments with these jars. They were carefully selected from a large stock, but only a small percentage of those selected were found to be air-tight. Of course the slightest leakage alters the conditions of the experiment. In the second place, it was found that the large space enclosed by these jars (large as compared with that enclosed by the narrow tubes and bottles before used) made invaporation exceedingly slow.

Series B is for the purpose of showing the effect of increasing the ratio of sodium chloride. The effect, as seen by comparing Experiment 1 with Experiment 2 of Series A, is twofold, (1) to increase slightly the rate of invaporation by the sodium chloride, and (2) to dry the potassium chloride more completely.

Series C will show the effect of increasing the ratio of potassium chloride. There are indications that potassium chloride cannot be completely dried by means of sodium chloride, but these experiments are still in their first stages.

A single experiment, made for the purpose of bringing to light any causes of variation not taken into account, has proved interesting. Equal quantities of sodium chloride were put into two small tubes and enclosed with water as usual. The invaporation was noted from time to time. Of course, under the same conditions the two quantities of salt, should invaporate at the same rate. As a matter of fact, the two quantities gained water at very nearly the same rate, but, while at first the weight of one tube increased slightly faster, after some time the second tube began to gain weight a little more rapidly than the first. In seeking an explanation of this variation, we noticed that the inside of the first tube was covered with small drops of liquid, while that of the second was dry. This evidently arose from the presence on the former of fine particles of salt exposing a large invaporating surface. Rapid dilution of the salt solution, thus formed, soon destroyed this temporary advantage, and then came into play a second but less marked cause of variation. Tube No. 2 was slightly wider than No. 1, and thus gave a somewhat larger invaporating surface.

"The Plague of Mice" in Nova Scotia and P. E. Island.

By the Rev. George Patterson, D.D., New Glasgow, N. S.

In the early settlement of Nova Scotia and P. E. Island, mention is not infrequently made of mice appearing in such swarms as to become a real plague, entirely destroying the crops of the new settlers over considerable areas. Diereville, a French writer, in a work published in 1699, says: "The Island of St. John (Prince Edward Island) is stated to be visited every seven years by swarms of locusts or field mice alternately—never together. After

they ravage the land, they precipitate themselves into the sea." There is no evidence of any such regularity in the visitation of mice, but later writers speak of it as occurring on Prince Edward Island at longer or shorter intervals, and on the main land it has not been unknown. We have, however, authentic information of two instances of the kind, one in Prince Edward Island, in the year 1775, and the other in Nova Scotia, in the year 1815. I do not know that in either case the facts were noted by any scientific observer. It may therefore be of interest, and also render some service to science, to gather up what information we can obtain regarding these rather remarkable events, from men of ordinary intelligence, who were witnesses of them.

The former of these visitations is now only a matter of tradition, but some years ago I conversed with aged persons, who in early years had passed through the troubles of that period, and from them gleaned the following particulars. In the year 1774 a number of families emigrated from Dumfries or its neighbourhood, in the south of Scotland, and commenced a settlement at Georgetown, or Three Rivers, as it was called. They raised some crop that season, and, if my memory serves me right, in autumn they found the mice a little troublesome. But at all events, the next season they swarmed in such numbers as to become a real plague. They consumed all the crop, even the potatoes in the ground. They boldly entered the dwellings of the settlers, and, when they could get no other food, they even gnawed the leather in the binding of books.

The consequence of this was that the settlers were brought to the verge of starvation, and would undoubtedly have perished but for a French settlement at some distance, from which they received supplies of potatoes. What that settlement was, and its distance from the place where they were located, I have not ascertained. But from their being able to save their crop of potatoes at least, I would be disposed to conclude that the mice had not reached them, and that the plague was, therefore, of very limited area. But the French settlement was much older, and had much more land under cultivation, and thus might have saved a part of

its crop. Still I believe that the mice could not have been as destructive as where the new-comers were.*

Of the visitation in Eastern Nova Scotia, in 1815, commonly known there as "the year of the mice," we possess fuller information. The Rev. Hugh Graham, then ministering in Stewiacke, thus writes to a friend in Scotland, under date 21st July of that year:

"This last winter was the coldest that ever I saw. The spring was also very cold and late. Appearances are now promising, only the field mice have become so numerous as to threaten the destruction of a great part of the crop. We have not had such a visitation for more than forty years past. They began to multiply last year, and did some damage."

The next year he writes under date, August 1:

"The plague of the mice is so far removed, that there is scarcely a mouse to be seen in house or field, or the woods, where they swarmed. But we feel the effects of it still. The grass, as well as grain, being greatly cut off, the farmers had to sell off a great part of their stock at low prices before winter, to bring their stock to their provender. But the winter was severe, and the spring uncommonly cold and late, which occasioned a great mortality in the remainder of their stock. And now breadstuffs have to be brought from afar, and at a high price, and many are very straitened as to the means."

This is the only contemporary record I possess of this visitation, but some years ago I made enquiries on the subject of persons who remembered it, and recently I have conversed with persons still alive, who though advanced in years are still, from recollection, able to give an intelligent account of it. These all agree in their testimony as to the main facts, and if they vary slightly in the details, these variations represent the differences which existed at different places. From these sources I am able to furnish the following particulars:—

1. *The area of their ravages.*—This included, we may say,

* It is probable that it was from some visitation of this kind that they gave the name Souris to a harbour about twenty-five miles to the North-East.

the whole of the Counties of Antigonish, Pictou and Colchester, and part of the County of Cumberland along the north shore to the north-west, and some small portions of the Counties of Guysborough and Halifax to the south; being a district about 80 miles in length by about an average of 50 miles in breadth, with a superficies of about 4,000 square miles. We may observe that, at this time, settlers had generally occupied the land both along the shores of the Straits of Northumberland on the north, and of the Basin of Minas on the south, and also along the principal rivers for some distance into the interior. But the central portions were still covered with the primeval forest, consisting largely of deciduous trees; and even where settlement was most advanced, the major part of the land was still under wood.

2. *The species.*—All the persons with whom I have conversed in the County of Pictou, agree that the creature by which the mischief was done was what is commonly known as the large, burrowing or short-tailed, field mouse, sometimes called the meadow mouse (*Arvicola riparia*, Ord),[1] but others say that other species were more abundant than usual. Mr. Roderick McKay, of St. Mary's, says that having set a large pot, partially filled with water, and a trap by which they fell into it, he found it in the morning filled with all the species known in the country. Mr. Samuel Waugh, of Tatamagouche, then over 19 years of age, says that there the jumping mouse was also numerous, and manifested its powers of destruction, but not nearly to the same extent as the other. And Dr. J. N. McDonald, who writes the history of Antigonish County, describes it as the meadow mouse, which he calls *Arvicola agrestis*, a term now applied by naturalists to a different species, but he adds

[1] In a paper by Sir Wm. Dawson on the "Species of Arvicola and Meriones, found in Nova Scotia," (Ed. Phil. Journal, 1856), he indicates the species as *A. Pennsylvanica*, Ord. This is, however, regarded by Baird as probably a synomym for *A. riparia*. A smaller Arvicole, *A.* (*Hypudaeus*) *Tupperi*, has been found by Downs, in Nova Scotia, and there are two varieties of *Jaculus Hudsonius*, the jumping mouse. One of them is the variety *Acadicus* of Dawson.—Ed.

that about a month after its appearance a smaller kind appeared, and that then a deadly feud arose in which many of the larger kind were killed. But none of my informants in the County of Pictou know anything of this second species.

3. *Rise and progress of the plague.*—Mr. Graham says that they began to multiply the previous year and did some damage. But in the County of Pictou, my informants generally state that they did not appear in such numbers as to excite notice. Probably in particular places, such as Stewiacke, which was situated, it might be said, in the midst of a large hardwood forest, they appeared sooner than in others.

But toward the end of winter they began to arrest attention. Those engaged in making maple sugar were troubled by their fouling their troughs for gathering sap. At this time, Dr. McDonald says that they were so numerous that a fall of two or three inches of snow was literally packed down by their feet in a short time; and before planting was over, the woods and fields alike swarmed with them. Generally their appearance in the clearings was sudden. One day they might not appear in a field, and the next they might be found in dozens or in hundreds. The seed grain, sown early, generally escaped them, but the later sown and the seed potatoes suffered from them. A story is told of a man who had made a clearing in the woods and carried out a quantity of oats to sow upon it. But on commencing his work they came in such swarms around him, eating the grain as he sowed it, that after continuing a while he threw the whole to them and returned home in disgust.

4. *Their numbers and ravages.*—By midsummer they swarmed everywhere. Every observer speaks of them as being in prodigious numbers—"in millions" was the common expression. In mowing, a cut of the scythe would not be made without killing some. They were bold too, and actually fierce. If pursued, when hard pressed, they would stand at bay, sitting upon their haunches, setting their teeth and squealing viciously. The males fought like little terriers. On passing a field one might hear them squealing

in these contests, and when killed, their skins might be found torn as the result. Boys sometimes caught them and for their amusement set them fighting. They seemed almost amphibious, readily taking to the water, and swimming small streams. An intelligent man on the East River of Pictou, told me that one of the places where they were most abundant was an island in the river, though whether this great increase was the result of migration or of the rapid multiplication of those formerly on the island I am unable to say. Cats, dogs, martens and foxes, gorged themselves upon them to repletion, but with little apparent diminution of their numbers. An old man, then a boy, told me that, where he lived, a cat had kittens in an outhouse, and used to hunt for them at night. In the morning he used to amuse himself counting the number of mice she would bring in, and on one occasion found it over 60. It was noticed that the wild animals became very plenty, but rather I should say were attracted from the woods by the abundance of prey. One man told me that he has seen as many as a dozen foxes on an intervale at one time. On the other hand, the Hon. Samuel Creelman, of Stewiacke, mentioned to me that in that settlement the domestic cats assumed a ferile condition and multiplied so that the next year they became a nuisance. They were so wild that they were a terror to children, and were hunted and killed in great numbers.

The hay crop was much damaged. The mice cut so much of it that lay withered, that the scythe catching upon it, would sometimes slide over the rest without cutting. But it was when the grain began to ripen that their destructiveness became especially manifest. They then attacked it in such numbers that all means were unavailing to arrest their progress. They have been known to cut down an acre in three days, so that whole fields were destroyed in a a short time. The jumping mice would spring at the ear and thus bring it to the ground, but the others were in the habit, as the country people expressed it, of junking it. They would nip a stalk off a little above the ground, and if instead of falling over, the end sank to the ground, leaving

it still upright, they would bite it off further up, until it fell over, or the end came within their reach, when they would either devour the grain or draw it to their nests, which were commonly under the roots of stumps. Over acres on acres they left not a stalk standing nor a grain of wheat, to reward the labours of the farmer. Trenches were dug, and when it could be done, filled with water, but they formed only a slight barrier to their ravages. When the grain was consumed, they so far burrowed in the ground as to attack the potatoes.

The result was that while in the older settlements, where the clearings were large, people by great effort managed to save a small part of their crops, in the back settlements and in clearings near the woods, all their crops were destroyed, with the exception of the hay, and that was much damaged.

5. *Their departure.*—As described to me by residents in the county of Pictou, they passed away as rapidly as they came. In the autumn, as the weather became colder, they became languid, scarcely able to crawl. One could trample them under his feet, and finally they died in hundreds, so that they could be gathered in heaps, and their putrefying carcases might be found in some places in such numbers as to taint the air. The reason assigned for this by the common people, was that they had eaten all the grain or other suitable food, and that they had nothing to subsist on but raw potatoes, which proved unwholesome.

But Dr. McDonald says that, after haying, millions of fleas (?) could be seen upon them, and that, to rid themselves of these tormentors, they rushed to the nearest river or pond and were drowned in great numbers. None of my informants in the county of Pictou speak of anything of this kind, but it is commonly reported that at Cape George they took to the salt water and died, their bodies forming a ridge like seaweed along the edge of the sea, and codfish being caught off the coast with carcases in their maws.

At the northwestern part of the district, another cause was assigned as a means of their destruction. Mr. Samuel Waugh, of Tatamagouche, mentioned to me that winter set in early, with one of the most remarkable sleet-storms ever

known here. The ground was covered with a sheet of ice perhaps an inch thick. It came so early that the wild geese had not left the north shore. At that time they were in the habit of stopping there for some time. They then passed over to the shores of the Bay of Fundy, where they lingered a few days, and then passed to the southern shore, where they again stopped before taking their final flight to the South. But, on this occasion, they were caught in the sleet-storm when crossing the Cobequid mountains. Their wings became so encrusted with ice that they were unable to fly. Their cries were heard, and settlers, attracted to the place, killed numbers of them. This, or the difficulty of obtaining food in consequence, was regarded by my informant as the cause of the disappearance of the mice. The next year, it was supposed from the same cause, the wild animals were found to be very scarce. However, I have not found any person in the eastern or southern portion of the district who recollects this storm, and perhaps it did not extend that far.

At all events, over nearly the whole district, which one season was ravaged by these creatures, in the next, as Mr. Graham says, scarce a mouse was to be seen. The only exception to this of which I have heard was at the East River of St. Mary's. Here Mr. Roderick McKay informs me that, for several years, they were numerous enough to be troublesome. But it may be observed that there were not more than five families there, who only arrived in the year of the mice, and made the first breach in the magnificent hardwood forests of that region.

6. *Causes.*—I do not intend fully to discuss the causes of these phenomena. All I design is to supply such information as I have obtained which may throw some light on the subject. The Rev. Thomas Trotter, late of Antigonish, arrived in the province in 1818. He had heard the contents of Mr. Graham's letter before leaving Scotland, and was interested in the question. When he arrived, the whole matter was fresh in the minds of the people, and by enquiring into the facts he thought he had arrived at an explanation of the case. He gave me his views, but it is

so long ago that I can only recollect their general purport. According to his report, one season before, I think that of 1813, and perhaps also the one previous, had been extremely favourable for the production of mast. Wild fruit and nuts were in unusual abundance. Then winter set in with a fall of snow which covered the ground before it was frozen, and it remained so covered the whole season. In the woods, therefore, in such circumstances and with such abundance of food, the mice would multiply rapidly. During the summer of 1814, as Mr. Graham says, they began to show themselves, but still they had sufficient food in the forest. That summer, however, did not prove so favourable, so that, with their increased numbers and decreased supply of food, by the spring of 1815 the woods no longer afforded them the means of subsistence, and they were driven to seek it in the clearings. The same cause, namely, "a failure of their ordinary food in the woods," is assigned by Sir W. Dawson, in the paper referred to above. I cannot but think, however, that if, on the one hand, there were such circumstances favourable to their multiplication, on the other, the destruction which had been going on among the fur-bearing animals for some time, must have been removing one of the natural barriers to their increase, and thus helped to produce the result.

At all events, when we consider the fecundity of these creatures, that they produce from five to eight young at a birth, and this at intervals of from one to two months, so that it has been calculated that a single pair might in one year produce 20,000, we need not be surprised that under circumstances favourable for their increase, and with the removal, in any measure, of the checks which Nature has set up against it, they should on occasions appear in such overwhelming numbers.

Other bearings of the subject I must leave to skilled naturalists to consider.

The Rearing of Bears and the Worship of Yoshitsune by the Ainos of Japan.

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Griffis,[1] in speaking of the Aino worship says:—"They worship the spirit of Yoshitsune, a Japanese hero, who is supposed to have lived among them in the twelfth century, and who taught them some of the arts of Japanese civilization." This is a statement which one frequently meets with in the modern literature of Japan; and it is also so often met with by those who are resident in northern Japan, that there appears to be some reason for its general currency, more substantial than that of mere fable. Yoshitsune, the son of Yoshituno, was born in 1159, and while yet a babe in his mother's arms, was saved by her from the vengeance of his father's assassins. He lived to become the ideal of chivalrous and knightly valor, to all future generations of those who aspired to military fame. Becoming the leading general in the army of his elder brother Yoritomo, his success in gaining a victory over the Tiara so aroused the jealousy of the latter, that his execution was ordered upon very slender grounds. According to some accounts, after escaping toward the north of Honshiu, Yoshitsune found further escape impossible, and committed hara-kiri. Another account relates that he escaped to the Island of Yeso, where he ruled undisturbed for a time. While yet another version, derived from the Chinese, identifies him with Genghis Khan. It is in connection with the second version that the account of his reign over the Ainos, and the worship of his spirit by them is associated. That there is, therefore, considerable uncertainty as to the actual manner and time of his death is evident, and to those who have studied the Ainos, his worship by them has also always been surrounded by very grave doubts. It is, therefore, a pleasure to produce testimony obtained by Mr. John Batchelor, which seems to throw a great deal of light upon the true nature of the relations existing between this

[1] Mikados Empire, 34.

renowned warrior and the savages of the north. His account, which he has sent to us, is as follows:—

THE WORSHIP OF YOSHITSUNE BY THE AINU.

"It appears to be a generally received opinion among those persons, whether Japanese or foreign, who have written or made any special inquiries respecting the subject, that the Ainu people are in the habit of worshiping the image or spirit of Kurōhonguwan Minamoto no Yoshitsune, who, it will be remembered, was driven to Yezo by his elder brother in the twelfth century of our era. And, indeed, when we call to mind that there is a little shrine upon a cliff at the village of Piratori, containing an idol representing that great personage; that some Ainu residing at and immediately round Piratori itself actually tell inquirers that some few of their number do at times, though not often, worship at the said shrine; and when we note the fact that most, if not all, of the Ainu men recognize the name Yoshitsune, then we see that this generally received and constantly asserted opinion has, apparently, a good degree of foundation in fact. The writer of these lines formerly shared, in common with many others, the generally received views on this subject, but after long residence with the people themselves, having spent many months in the village of Piratori (at, so to speak, the very doors of the shrine in question), he has been obliged to change his opinion, or at least very considerably to modify it, in regard to this as well as many other subjects connected with the Ainu. The following remarks contain a few facts bearing upon this question, and the writer's reasons for believing that the Ainu do not, in the commonly received meaning of the term, actually *worship* either the spirit or image of Kurōhonguwan Minamoto no Yoshitsune.

"In the first place, it must be clearly understood that when persons say the *Ainu* worship Yoshitsune, they mean that people, not as a nation, but merely a few individuals resident in the Saru district. Again, the facts are still more narrowed when we make strict inquiries; for it is not even pretended that all the Saru Ainu worship him, but only

those of Piratori. Now, there are two Piratoris, viz.: Piratori the upper and Piratori the lower. These two villages were once united, but are now situated from about a quarter to half a mile apart. The shrine of Yoshitsune (and there is but one shrine in Yezo) is at the upper Piratori, and the inhabitants of the lower village will tell an inquirer that it is the people of the upper Piratori who worship the person in question. Now, the upper village contains only about thirty-two huts, and we find that not even ten persons out of these families really worship Yoshitsune. It is clear then that the Ainu, considered as a race or nation, do not at the present day deify that hero.

"Then again, it should be noted that the present shrine is decidedly of Japanese make and pattern: in all respects it is like the general wayside shrines one may see anywhere in Japan. It was built about ten years ago by a Japanese carpenter resident at a place called Sarabuto (Ainu, *San-o-butu*). Previous to this there was also a Japanese-made shrine on the same spot, but a much smaller one. The idol in the shrine is both small and ugly; it is a representation not so much of a god as of a warrior, for it is dressed in armour and is furnished with a pair of fierce-looking, staring eyes and a horribly broad grin; it is just such an idol as one might expect in this case, seeing that Yoshitsune was a warrior. Besides this, the Ainu have treated the image to an *inao* or two. There is nothing more, and the shrine is too small for a person to enter.

"Now, it is a fact not generally known, I believe, that, according to Ainu ideas and usages, it is absolutely necessary to turn to the east in worshipping God (the goddess of fire excepted). Hence, the custom of building all huts with the principal end facing the east. The chief window is placed in the east end of the hut, so that the head of a family may look towards the east when at prayer. It is considered the height of impoliteness and disrespect to look into a hut through the east window. But the shrine of Yoshitsune is placed in such a position that the worshippers would have to sit or stand with their backs to the east. In every other matter (and why not in this also),

assuming such a position in prayer would be a great disrespect to the object worshipped:

"The image of Yoshitsune is looked upon from the east, hence, speaking from analogy, it would appear that it is not the Ainu worshipping Yoshitsune, but either Yoshitsune worshipping the Ainu, or the Ainu insulting Yoshitsune; such a conclusion may appear to be somewhat far fetched, but is, when compared with other things, at any rate a logical one. The writer does not intend to say that the Ainu, in the present case (for with them religion is a serious thing), place such a construction upon the form of the shrine, though they dearly like to play upon a person sometimes. All he wishes to remark is, that the position of the shrine of Yoshitsune does not come up to the acknowledged requirements of the Ainu ideas of Deity worship.

"Again, it is said by the people that they would not worship an idol, because it would be directly against the expressed command of *Aioina Kamui*, their reputed ancestor. The Ainu are, in many things, a very conservative people, and in the matter of religion, particularly so. Note the following incident. In the days of the Tokugawa *regime*—so runs the tale—the Ainu were ordered by the Government, or rather by the authorities of Matsumai, to cut their hair Japanese fashion. The result was a great meeting of the Yezo chiefs, which ended in sending off a deputation to beg that the order be countermanded, or at least suffered to lapse. For, say the Ainu, we could not go contrary to the customs of our ancestors without it bringing down upon us the wrath of the gods. And, though a few Ainu, particularly those at Mori, did cut their hair as ordered, the people as a whole were let off. If then a mere change in the fashion of cutting the hair should be such a weighty matter, what would the institution of idol-worship involve?

"But notwithstanding all this, there is still not only the fact of the shrine being at Piratori to be accounted for, but also the fact that some Ainu do tell us tnat Yoshitsune is worshipped by a few of their number, though very seldom. What is the explanation?

"An Ainu himself shall answer the first question. "You know," says he, "we have for a long time been subject to the Japanese Tono Sama and Yakunin; and it has been to our interest that we should try to please them as much as possible so as not to bring down trouble upon ourselves. As we know that Yoshitsune did come among our ancestors, it was thought that nothing would please the officials more than for them to think that we really worship Yoshitsune, who was himself a Japanese. And so it came to pass that the shrine was asked for and obtained." This statement was made to the writer quite spontaneously and confidentially, along with many other matters. Taken by itself, this statement might not be worth much, but viewed with other things of the sort, it speaks volumes. The spirit here unwittingly shown is happily fast dying out, for the Ainu begin to see that there is now but one law for both peoples, and that there is justice obtainable even by them. Nevertheless, the spirit above exemplified has been a real factor in the life and actions of the Ainu people.

"The whole secret of the second question turns upon the meaning of the word worship. The word used by the Ainu is *ongami*, and the meaning is "to bow to," "to salute." The Ainu are delightfully sharp in some things, and this is one of them. An Ainu told me one day, with a most benign grin, reaching almost from ear to ear, that he did *ongami* (salute) Yoshitsune's shrine or idol; but as for (*otta inonno-itak*) praying to that person, neither he nor any one that he knew, did so; and, as regards (*nomi*) the ceremony of offering *inao* or libations of wine to him, both he and many others were always ready to do so providing someone else would find the *saké!* Here, then, is the point; the Ainu do not worship Yoshitsune in the sense of paying him divine honour any more than the people of England worship Lord Beaconsfield; but some Ainu *do* worship him in the sense of honouring him, in the same sense as Lord Beaconsfield is honoured by the members of the Primrose League, only not in anything like the same degree. Some London cabman would be just as pleased to worship Mr. Gladstone by drinking his health, and in the same sense,

too, as an Ainu would be to hold libations in honour of Yoshitsune; for after all, the said libations are neither more nor less than a drinking of *saké*. The real god worshipped is the person's own stomach.

"Such then are my reasons for dissenting from the generally received opinion on this subject. On the contrary, I believe that Yoshitsune is merely honoured by the people. And this opinion rests, not upon the argument of question and answer, but upon that together with actual observation and spontaneously given information. It is, indeed, a wonder that the Ainu do not worship him as a god, and perhaps a few of the rising generation may yet do so; but I hope not."

A second point of interest, is that relative to the bringing up of young bears by the Ainos. The bear, which in Yezo closely resembles the cinnamon bear of the Western States, if, indeed, he is not the same species, is an object of worship among these people. Whenever the young can be captured, they are kept in close cages until of a certain age, when they are sacrificed with great ceremony. Every Aino village and hamlet has at least one such cage, and a traveller usually finds them occupied. The belief is common among the Japanese—and it has also been freely accepted by foreigners—that when these bears are captured very young they are nursed by the Aino women as they would their own babes. That such could be the case, has always appeared to the writer as highly improbable, but in view of the difficulty of gaining trustworthy testimony, it has heretofore been impossible to satisfactorily deny the truth of the common assertion. Mr. Batchelor has now brought forward the result of evidence obtained during his residence among these people, and his statements are sufficiently conclusive to justify us in asserting our original opinion. His communication to me is as follows:—

The Rearing of Bears by the Ainu.

"It appears to be thought by many people, both English and American, that the Ainu women are in the habit of bringing up bear cubs at the breast as they do their own children. This opinion has received so much credence be-

cause many persons who have written anything about the Ainu race have, for some reason or other, either passed the subject over without even venturing a word of denial or explanation, thereby appearing, at all events, to give assent thereto, or else they have stated it to be fact. But no one —that I am aware of—has ever told us that he has actually seen an Ainu woman nursing a bear's cub, and I for one, shall be very much surprised if ever I hear it has been seen by any foreigner. It is not intended to deny absolutely that bear's cubs have, in a sense, been brought up at the breast, or that they may again be so brought up. But allow me to remark, in behalf of the Ainu, that during five year's sojourn amongst, and almost daily intercourse with them—living with them in their own huts—I have never once witnessed anything of the sort, nor can I find a single Ainu man or woman who has seen the cubs of bears reared by women in the same way as a mother rears her child. The facts appear to have been somewhat over-stated, and it is hoped the following remarks may be received in extenuation of the charge.

"My experience of the rearing of young bears is as follows :—Bears' cubs are very seldom taken so young that they cannot lap water, and when a dish of millet and fish, boiled into a soft pap, is placed before a cub it soon learns to feed itself. They never care to starve for more than a day or two. With those, therefore, that can lap (which is by far the greater proportion), no difficulty is experienced. The only inconvenience with them arises from the great noise they make in crying for their mother. This nuisance is soon cured, for the owner of the cub takes it to his bosom and allows it to sleep with him for a few nights, thus dispelling its fears and loneliness.

"When a cub is taken so young that it cannot even lap its food, it is fed from the hand and mouth, not from the human breast. Sometimes small portions of fish or a little millet (often both mixed) are chewed by a person and thrust little by little into the animal's mouth, and it is thus made to swallow. At other times millet is made into a kind of batter or very thin paste, a mouthful of which is taken

by a man or woman, and the cub allowed to suck it from the lips, which it will readily do. In fact, it is at first fed in much the same way as boys at home feed young birds. The next step is to teach the animal to lap from the hand, which is also soon accomplished; then it learns to take its food from a wooden tray. This is the general way of rearing bear cubs, and any one who knows how fond bears are of licking things, will readily understand how easy it would be to teach a cub to lap. A very young cub could almost subsist by licking only.

"However, a woman may occasionally be found who is strong-minded enough to take a very young cub—a cub whose eyes are not yet open—to her breast, once a day, for a day or two, but at the same time she feeds it from the hand and mouth in the manner above stated. Such women are very scarce indeed, as is also the occasion for them, for cubs are seldom taken young enough to admit of being so nursed. I have seen the cubs of bears brought up by hand, but have never seen one nursed as a woman nurses a child.

"No doubt the Ainu are low enough in the scale of humanity and have some barbarous manners and customs, but their barbarity has been exaggerated, as their stupidity has been taken too much for granted. Thus, in the case under discussion, it seems to me that the bringing up of bears' cubs at the human breast, should not be called an Ainu custom merely because a few strong-minded women can be found, after a great deal of search, who will take and nurse a cub for a day or two. They are exceptions to the rule."

The results of the communications as above, are of considerable interest as showing that the Ainos are, at least in some respects, of a much higher order than might be inferred from the writings of many, and we cannot but feel that these facts are of considerable ethnological interest, concerning as they do, a most interesting remnant of a people of whom we are just beginning to gain a correct knowledge.

On the Physiology of the Heart of the Snake.

By T. Wesley Mills, M.A., M.D.

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This paper will furnish an account of a study of the heart of the Snake, as a continuation of a series of papers already published on the cardiac physiology of the cold-blooded animals, including thus far also the Water Tortoise, the Sea Turtle, the Fish, the Alligator, and Menobranchus.

The snakes used belonged to the genus *Tropidonatus*, and the experiments were made during the midwinter of 1886 and 1887. The animals had been without food since their capture in the autumn, but were not apparently in any degree hibernating, the temperature of the apartment in which they were kept being not far from 17° C. They were left in a tank, with fresh water running constantly from a tap, but they were free either to remain in the water or to betake themselves to the dry shelves of the tank, on which they were, in fact, mostly seen coiled up together. The method of study has been that pursued throughout, viz., direct observation, and as a stimulus, the interrupted current supplied by a Du Bois' inductorium, fed by one good-sized Daniell's cell.

The Vagus Nerve.

Comparison of the vagi througout these experiments has established the following conclusions for this animal:—

1. In no case was either vagus without effect on the rhythm of the heart. In every case actual slowing, and with a sufficiently strong current, arrest followed stimulation.

2. In the majority of instances the *right* vagus was more efficient than the left.

3. In a very few cases, both nerves seemed to be almost, if not quite equally, influential over the heart's rate.

In this comparison, then, it appears that the vagi of the snake resemble functionally those of the other cold-blooded animals examined by me.

After Effects of Vagus Stimulations.—These were of the following kinds:—

1. Increased rate of beat, more marked the slower the heart at the time of stimulation,

2. In all cases increased force (working power) of the heart. This was, sometimes, the only effect noticeable.

3. When irregularity of rhythm of either the whole or some part of the heart existed prior to stimulation, this was abolished for a longer or shorter period.

In these respects the heart of the Snake follows those of the other animals referred to above; but none of the effects have been so marked as in the case of the Chelonians, though more certain than in the Fish, so far as my own observations on that animal go.

Mode of arrest and of re-commencement of the cardiac beat.—When the current is too feeble to arrest the whole heart; or when the whole heart is not amenable to its action, as is the case when its nutrition is much impaired, the *auricles* are the first or only parts to stop pulsating. The *sinus venosus* is always the first part to commence to pulsate after vagus arrest, and for several beats the auricles proper may be quiescent, the wave of contraction passing over what I have called the "Sinus extension,"[1] to the ventricle which may respond for some seconds prior to the auricles.

I am inclined to believe that the auricles are not a little dependent for the maintenance of their rhythm on the intracardiac blood pressure, and that this may enter as one factor into the explanation of this phenomenon. At all events, the same takes place in all the poikilothermers I have examined. McWilliam[2] pointed it out for the Eel, and as I indicated in my paper on the Sea Turtle,[3] Gaskell[4] is in error when he states that in the Tortoise an excitation wave cannot travel from the sinus to the ventricle and cause a

[1] "The Rhythm and Innervation of the heart of the Sea Turtle," *Journal of Anat. and Phys.*, vol. xxi.

[2] *Journal of Physiology*, Vol. vi., Nos. 4 and 5.

[3] Op. cit. p. 7.

[4] *Journal of Physiology*, Vol. iii., Nos. 5 and 6.

ventricular contraction independently of a wave of contraction over both parts of the auricle.

Inexcitability of the Sinus and Auricles under vagus stimulation.—One of most interesting results of the recent cardiac studies has been the unexpected demonstration that certain parts of the heart in some animals (the particular region being variable) are, to a greater or less degree, *inexcitable* to direct stimulation when the heart is arrested by vagus influence.

[1]McWilliam has stated that the excitability of the auricle is temporarily abolished in the Eel's heart under vagus stimulation. But this investigator is entirely in error when he affirms that such is not observed in the heart of the Snake, though he is correct as regards the other animals he instances. I have established, by experiments, that *during vagus stimulation, the sinus and auricles of the heart of the Snake do not respond to direct stimulation.* If the heart be at its best, and the stimulating currrent sufficiently strong, the excitability may be wholly abolished; but with a weaker current, or a less vigorous heart, the effect may be only partial. I have shown that in [2]*Menobranchus* the ventricle is the part of the heart most readily and most profoundly affected by stimulation of the vagus, and that during such stimulation the ventricle is inexcitable. According to [3]McWilliam, in the *Newt*, the sinus, auricles and ventricle, are all inexcitable to direct stimulation during strong inhibition.

These peculiarities and differences show how dangerous it is to *assume* the applicability of the same physiological generalization to animals, even closely related morphologically. This conviction on my part had much to do with my beginning these studies and continuing them on animals of related groups; for I felt satisfied that a systematic comparison would establish differences unsuspected by those accustomed to extend conclusions derived from experiments

[1] Op. cit., p. 226.

[2] *Journal of Physiology*, Vol. vii.

[3] Proceedings of the British Physiological Society, *Journal of Physiology*, Vol. vi.

on one species or even genus of animals to others. A survey of my own series of investigations alone, will amply demonstrate the desirability of such a course as I have pursued, and still more so if taken in connection with the work of other investigators, like McWilliam, who have followed the comparative method to any degree. A great deal of laborious work, without brilliant results, must be done, but I wish to state most emphatically my conviction that it is the only way by which a broad, solid and safe physiology can be produced.

Cardiac Riflexes.

I have noticed in the Snake, as often in the Chelonians, that while the medulla oblongata is intact, the heart may be very irregular, but that upon the destruction of that part, the rhythm at once changes, becoming always regular and often more frequent. The explanation is probably to be sought in the various influences reaching the medulla and passing down the vagi in an animal in an abnormal condition from the circumstances of the experiment. But when stimuli are applied to various parts, as the skin, the viscera, etc., the results are found to be very variable. Apparently there are great individual differences, and not a little depends on the vigour of the animal at the moment of experiment. Much of what has been established for the Chelonians[1] might be repeated for the Snake.

It only remains to note a few peculiar results of special interest in this connection. It sometimes happens in the Frog, and rarely in the Chelonians that the first effect of vagus stimulation is not slowing but acceleration of the beat of the heart. This I have not witnessed in the Snake; but, on one occasion, when reflex inhibition was unusually marked, on placing the electrodes over the lung, the rate was accelerated for 3-4 beats. In my paper on the Fish,[2] it has been pointed out that a certain strength of current

[1] *Journal of Phys.*, Vol. VI.; *Journal of Anat. and Phys.*, Vol. XXI.

[2] Op. Cit. p. 89.

may give rise only to acceleration, or acceleration followed by slowing, when a stronger current caused only decided and prompt inhibition. In such cases the result has followed stimulation of various parts of the body. Similar observations have been made in the Sea-Turtle[1] on stimulation of the surface of the liver. In these cases the nerve mechanism requisite for reflex inhibition was intact, and the brain beyond the medulla destroyed. As the above has been an almost constant result of excitation by the interrupted current of the anus, and above all of the tail in the Fish, it is not possible to explain it in this animal by escape of current on either the main stems or terminal branches of the vagi. Nor do I think this explanation holds for either the Sea-Turtle, or the Snake. The subject has been discussed in my paper on the Alligator.[2] Our knowledge does not seem to be sufficient at present to clear up these cases fully; in the meantime, I add the results in the Snake to those already recorded for other cold-blooded animals.

In the explanation of Marshall Hall's remarkable result on crushing the stomach of the Eel, when cardiac inhibition followed, notwithstanding that the brain and spinal cord had been wholly destroyed, McWilliam[3] holds that Hall's explanation of exhibitory action through the sympathetic system is not valid, and that the result is to be explained by vibratory stimulation of the vagi, owing to the concussion of the blow of the hammer used in crushing the stomach.

With a view of testing the above hypothesis as regards the Snake, in a case in which reflex inhibition was specially well marked, I destroyed the whole brain and then attempted to get cardiac arrest by blows upon the animal with a large forceps, and heavy blows on the table on which the subject of the experiment rested, but with entirely *negative* results. That in the sensitive heart of the fish McWilliam's explanation might, in certain cases, be valid, it is possible to understand; but that they explain either Marshall Hall's experi-

[1] Op. Cit. p. 7.

[2] *Journal of Anat. and Phys.*, Vol. xx., p. 555 *et seq.*

[3] Op. cit., p. 238.

ment or my own results as detailed in my paper on the Alligator and the Fish, I am unable to believe.

When Hall stated his belief that the sympathetic was a channel for influences that may lessen the heart's action, he reached, I believe, a new truth. In my paper on the Terrapin,[1] I called attention, for the first time, to certain peculiar and hitherto unobserved phenomena, that I then felt must lead out to something of importance. If Gaskell's[2] conclusions turn out correct as to the physiological character of certain different kinds of nerve fibres, then my previous statement that "the vagus is a sympathetic with inhibitory fibres; the sympathetic a vagus without these fibres, if indeed it be wholly without them (a point I have suggested previously as not yet to be considered settled);"[3] may be considered the first announcement in distinct form, in a published paper, of a doctrine likely to be soon established on a firm anatomical and physiological basis. But yet it must be admitted that the genius of Marshall Hall was the first to penetrate the darkness. At the time of writing the above, I was unaware of his suggestion as to the influence of the sympathetic over the heart. If it be true that certain fibres running in the sympathetic system have the effect of first increasing metabolic action, thus leading to exalted functional activity followed by exhaustion, then certain results of stimulation pointed out by me in my papers on the Terrapin and the Fish, become clearer, though not, perhaps, fully explained, *e.g.* acceleration followed by slowing on stimulation of various parts of the body, even with the whole brain destroyed.

Faradization of the Heart.—The results of this method of stimulation may be stated somewhat briefly, as, in the main, they correspond with what I have found in the other animals experimented upon. The results vary much with the strength of the current used, but especially with the functional condition of the heart at the time. When

[1] *Journal of Physiology*, vol. vi., p. 271, 283, etc.
[2] *Journal of Physiology*, vol. vii., No. 1.
[3] Op. Cit., p. 383.

at its best, the heart may be arrested on placing the electrodes over either auricle or sinus. When somewhat exhausted, the auricles alone, or but one of them, may be arrested on placing the electrodes over one auricle. Arrest of the sinus is, of course, always followed by stoppage of the rest of the heart.

The behaviour of the ventricle, when thus directly stimulated, differs from that pointed out in the heart of the Sea Turtle.[1] In the Snake, stimulation of the ventricle is never followed by that "intervermiform" action so common in the Sea Turtle, and less frequently seen in the Terrapin. One of the first effects, if the current be not too strong, may be accelerated action; and I believe the ventricle is only arrested by the escape of current on the rest of the heart, so that the ventricular pause is really due to the arrest of the sinus. As after arrest from vagus stimulation, the sinus and ventricle often beat for some time before the auricles begin. The usual paralysis and light colored points, to which allusion has been made in my other papers, are evident in the Snake, though not so marked as in the Chelonians. I see no reason to change the opinion expressed in former papers as to the meaning of these phenomena, nor to doubt that arrest of the heart on direct faridisation is owing to stimulation of the fine terminals of the vagi nerves within the heart's substance.

Independent Cardiac Rhythm.—As in former instances, *ligatures* have been used to separate one part of the heart from another. They are unquestionably much more reliable than clamps or other apparatus. When the ventricle is ligatured off from the rest of the heart, in no case does an independant rhythm arise in it. Notwithstanding the increase in pressure, the parts above continue to beat well, even more vigorously than before. A ligature between the sinus and auricles drawn tightly enough to prevent any wave of contraction passing down over it completely arrests all parts below; and I have in no case seen an independant rhythm arise in these regions of the heart. In short, my experiments have given negative re-

[1] Op. cit.

sults as to a really independant rhythm, and confirm views already expressed in the other papers in which my work has been recorded.

SUMMARY.

1. The investigations recorded in this paper were made in midwinter, on fasting but not hibernating animals.

2. Comparison of the vagi showed that in every instance both nerves were efficient; but usually the right was the most so; in some cases the difference, if actual, was minimal.

3. Stimulation of the vagi, leads to after increased force and frequency of beat, or of the former only, and according to the law[1] of inverse proportion previously announced by the writer.

4. The mode of arrest of the heart is identical with that noted in the Chelonians, Fish, &c.; the same applies to the mode of re-commencement.

5. During vagus arrest the *sinus* and *auricles* are inexcitable.

6. There are certain peculiar cardiac effects not explicable by reference to the vagi nerves alone, but which put the sympathetic system of nerves in a new light.

7. Direct stimulation of the heart confirms results previously noted by the writer for other cold-blooded animals. Arrest is, in all the animals of this class yet examined, owing to stimulation of the terminals of the vagi within the heart's substance.

8. As regards independent cardiac rhythm, the results have been negative.

9. The heart of the Snake, upon the whole, seems to lie physiologically between that of the Frog and that of the Chelonians.

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McGILL UNIVERSITY,
Montreal, June, 1887.

[1] *Journal of Phys.*, Vol vi., p. 281 *et seq.*

The Freshwater Sponges of Newfoundland.

By A. H. MacKay, B.A., B.Sc.

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In August of 1885, I spent a few weeks in Newfoundland, investigating its natural history and paying particular attention to the fauna and flora of its freshwater lakes. My work was confined nearly entirely to the Avalon peninsula and adjacent portions of the island. I dipped into the ponds and lakes more easily accessible from the line of railway extending from St. John's towards the interior, such, for instance, as Virginia, Quidi Vidi, lakes near Harbor Grace Junction, Lady, Bannerman, Rocky, Carbonear and other lakes and lakelets upon the rocky highlands and near the sea level as far west as Heart's Content, on Trinity Bay. This region is generally considered as of Huronian age, and sponge collections were often limited to those growing in the shallow margins of the lakes on the under sides of splinters of hard, slaty quartzites, or more massive more or less water-worn rock fragments. The dredge, although successful in the collection of other material in some of the lakes, brought no satisfactory specimens of sponges from any considerable depth.

Of the sponges, *Spongilla fragilis*, Leidy, was the most abundant and universal. Then came *S. lacustris* var. *Dawsoni* and *Meyenia fluviatilis*, Carter. Next, the newly discovered Nova Scotian sponges, *Spongilla Mackayi*, Carter, and *Heteromeyenia Pictovensis*, Potts. And lastly, *Tubella Pennsylvanica*, Potts, and the following species:

Encrusting stones in lakelets near Heart's Content, on August 14th, this sponge was found. On a superficial examination, from the presence of small birotules in the flesh, nearly identical with those of *Meyenia Everetti*, Potts, it was thought to be the latter species. But the character of the statoblast or reproductive gennule, showed it to be quite distinct from any other sponge described. The specimens were referred simultaneously to H. J. Carter, Esq., F.R.S., of England, and E. Potts, Esq., of Philadelphia, who

substantially agreed on every point. On account of its birotulate dermal spicules which caused it to approach *M. Everetti*, and a tendency of the spinous processes in some of the statoblast spicules to group themselves towards each end of the shaft and assume a partial birotulate aspect, as shown in the accompanying illustration given by Mr. Potts, he was disposed to classify it as a Meyenia. The non-birotulate character of the great majority of these spicules, and the tangential position of their shafts with respect to the spherical chitinous envelope of the statoblast, seemed, however, more technically to bring it under *Spongilla* as defined by Carter.

But, in *Meyenia acuminata*, Potts, we have an example of birotulate spicules placed tangentially. We have in *Spongilla Terræ Novæ*, at all events, a form closely connecting *Spongilla* and *Meyenia*. It is to be regretted that owing to the distance and generally inaccessible nature of the rocky-ridged, lake-cradling highlands around Trinity Bay, that I have found it impossible to obtain more mature specimens of this species as yet, such as would be collected in October, for instance. Mr. Potts has published a description, which is substantially that given below, in the Proc. Acad. Nat. Sci., Phila., 1886, pages 227-230.

SPONGILLA TERRÆ NOVÆ, *Potts and Carter.*

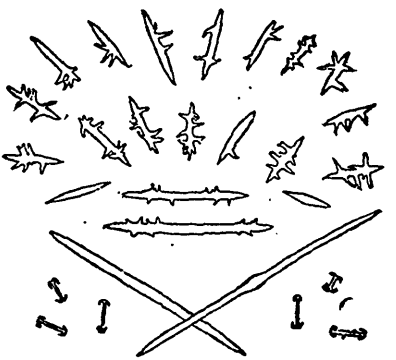
"Sponge encrusting; sarcode of the young growth a dense mass of minute spherical cells, embedding slender, thread-like, curving lines of fasciculated skeletal spicules, developing later into a very loose, open tissue, with few connecting spicules, charged with statoblasts.

"Statoblasts, spherical, rather large, averaging about 0.036 inches in diameter; chitinous coat thin, "crust" apparently wanting; aperture, single, circular, about .0015 inches in diameter.

"Skeletal spicules relatively few, generally smooth, slender and gradually pointed, forming, by overlapping each other linearly, threadlike fascicles; dimensions about 0.0067 by 0.0002.

"Dermal or flesh spicules very abundant, minute birotulates of unequal size, averaging about 0.0007 inches in length; shafts slender. cylindrical, occasionally spined; outer surface of rotules dome shaped; rays prolonged, terminations acute; irregularities frequent. Mixed with occasional linear, spined spicules.

"Spicules upon statoblast fundamentally smooth, robust fusiform, pointed, averaging 0.0015 inches in length, generally having as many as from one to twelve spines unsymmetrically scattered along the shaft, which is placed tangential to the chitinous coat of the statoblast."



SPICULATION OF SPONGILLA TERRÆ NOVÆ, *Potts.*

NOTES ON FOSSIL WOODS FROM THE WESTERN TERRITORIES OF CANADA.

BY SIR WILLIAM DAWSON, F.R.S., &c.

(*Abstract of a Paper read before the Royal Society of Canada.*)

Silicified wood occurs in the country west of Manitoba, in the true Cretaceous beds, in the Laramie and in the Miocene of the Cypress Hills. Consequently the numerous specimens in our collections, picked up loose on the plains, are of little palæontological value, as their sources are uncertain. The author had, however, obtained some specimens found in situ on the Boundary Commission Survey, which were described in the Report of Dr. G. M. Dawson,

1875. More recently, Schroeter had described specimens from the Laramie of the Mackenzie River. A considerable number of specimens from ascertained horizons had now been collected by the parties of the Geological Survey, especially by Dr. G. M. Dawson, Mr. J. B. Tyrrell, and Mr. T. C. Weston; and slices, prepared by the latter, had been submitted to the writer.

The present note on these might be considered as supplementary to previous papers on the fossil plants of the Western Cretaceous. The paper then proceeds to notice the specimens in detail, and to refer them to their probable genera, but as they may have belonged to species already named and described by their leaves and fruits, he thought it unnecessary to give specific names to the new forms, but merely referred to the modern types represented by the several specimens. In this way it appeared that a number of genera of conifers, more especially *Sequoia*, *Taxus*, *Salisburya* and *Thuja*, were present, as well as woods allied to Birch, Poplar, Hickory, Elm, and other familiar forms. Appended to the paper were notices of additional species of plants recently collected in the Belly River and Laramie formations, and concluding remarks on the general bearing of the subject, of which the following is a summary:

While studying the specimens described in this paper, I received the volume of the Palæontographical Society for 1885, containing the conclusion of Mr. Starkie Gardner's description of the Eocene Coniferæ of England. The work which he has been able to do in disentangling the nomenclature of these plants and fixing their geological age, is of the greatest value, and shows how liable the palæobotanist is to fall into error in determining species from imperfect specimens. Our American species no doubt require some revision in this respect.

I have also, while writing out the above notes for publication, received the paper of the same author on the Eocene beds of Ardtun in Mull, and am fully confirmed thereby in the opinion derived from the papers of the Duke of Argyll and the late Prof. E. Forbes,[1] that the Mull beds very

[1] Journal Geol. Socy. of London, Vol. VII.

closely correspond in age with our Laramie. The *Filicites hebridica* of Forbes is our *Onoclea sensibilis*. The species of *Ginkgo*, *Taxus*, *Sequoia* and *Glyptostrobus* correspond, and we have now probably found a *Podocarpus* as noted above. The *Platanites Hebridicus* is very near to our great *Platanus nobilis*. *Corylus Macquarrii* is common to both formations; as well as *Populus arctica* and *P. Richardsoni*, while many of the other exogens are generically the same and very closely allied. These Ardtun beds are regarded by Mr. Gardner as Lower Eocene or a little older than the Gelinden series of Saporta, and nearly of the same age with the so-called Miocene of Atane-kerdluk in Greenland. I have ever since 1875, maintained the Lower Eocene age of our Laramie and of the Fort Union Group of the Northwestern United States, and the identity of their flora with that of McKenzie River and Greenland; and it is very satisfactory to find that Mr. Gardner has arrived at similar conclusions with respect to the Eocene of Great Britain.

An important consequence arising from this is, that the period of warm climate which enabled a temperate flora to exist in Greenland was that of the later Cretaceous and early Eocene, rather than, as usually stated, the Miocene. It is also a question admitting of discussion, whether the Eocene flora of latitudes so different as those of Greenland, Mackenzie River, N. W. Canada and the Western States, were strictly contemporaneous, or successive within a long geological period in which climatal changes were gradually proceeding. The latter statement must apply at least to the beginning and close of the period; but the plants themselves have something to say in favour of contemporaneity. The flora of the Laramie is not a tropical but a temperate flora, showing no doubt that a much more equable climate prevailed in the more northern parts of America than at present. But this equability of climate implies the possibility of a great geographical range on the part of plants. Thus it is quite possible, and indeed highly probable, that in the Laramie age, a somewhat uniform flora extended from the Arctic seas through the great central plateau of America far to the south, and in like manner along the western coast

of Europe. It is also to be observed that, as Gardner points out, there are some differences indicating a diversity of climate between Greenland and England, and even between Scotland and Ireland and the south of England, and we have similar differences, though not strongly marked, between the Laramie of Northern Canada and that of the United States. When all our beds of this age from the Arctic sea to the 49th parallel have been ransacked for plants, and when the palæobotanists of the United States shall have succeeded in unravelling the confusion which now exists between their Laramie and the Middle Tertiary, the geologist of the future will be able to restore with much certainty the distribution of the vast forests which in the early Eocene covered the now bare plains of interior America. Further, since the break which in Western Europe separates the Flora of the Cretaceous from that of the Eocene does not exist in America, it will then be possible to trace the succession of plants all the way from the Mesozoic Flora of the Queen Charlotte Islands and the Kootanie series described in previous papers in these transactions, up to the close of the Eocene, and to determine for America at least, the manner and conditions under which the Angiospermous flora of the later Cretaceous, succeeded to the Pines and Cycads which characterized the beginning of the Cretaceous period.

Squirrels: their Habits and Intelligence, with Special Reference to Feigning.

By T. Wesley Mills, M.A., M.D., Professor of Physiology, McGill University. With an Appendix by Robt. Bell, M.D., LL.D.

(Abstract of a Paper read before the Royal Society of Canada.)

The writer believes that the comparative method should be applied to the psychology of animals, and in his paper compares the intelligence of the various species of squirrels with each other and with that of other rodents. He thinks the evidence derived from his own studies and the accounts of others, warrants the conclusion that the intelli-

gence of the Flying Squirrel (*Pteromys volucella*) and the Chipmunk (*Tamias striatus*) are about on a par; that the Red Squirrel or Chickaree (*Sciurus hudsonius*) ranks nearly if not quite first; and that, as compared with other rodents, squirrel intelligence is of a high order. The superiority of the Red Squirrel is to be attributed, in part, to his contact with man; and is evidenced by his wide geographical distribution (as pointed out in Dr. Bell's appendix), showing capacity to cope with many and varied conditions. The influence of hibernation on the psychic life of any animal is an interesting enquiry; but, as regards the squirrels, much more information of an accurate kind, in regard to the manner in which these creatures pass the winter, is greatly needed. All species seem to lay up more or less of food for a time of scarcity. It is certainly known that the Chipmunk does hibernate for a portion of the winter, at least; but the real state of the case, as regards the other species of squirrels, has not yet been fully learned. Among the peculiar habits of the squirrels, of special physiological interest, is that of sneezing into the fore-paws when going through the operation of dressing their fur (toilet); it seems to be voluntary, and functionally comparable to clearing the throat in human beings.

Concurrent testimony from widely different quarters has established that, among many different genera of rodents, there is musical capacity both of execution ("song") and, as we must infer, of appreciation. This faculty has been most observed in mice. The writer's investigation into the vocal expression of the Chickaree has revealed a wide range of capacity in expressing the emotions and other psychic states, naturally most marked in excitement. A study of two Chipmunks kept in confinement has enabled the writer to correct some partially erroneous statements of other writers, and has convinced him that while the intelligence of this species, relatively to that of the Red Squirrel, is low, it shows the power to adapt to its surroundings in an intelligent manner beyond what has been supposed.

A large part of the paper is devoted to a critical exami-

nation of the various theories relating to *feigning death and injury* by animals, especially in the light of the writer's own study of the Red Squirrel. He believes that the expression, "feigning death," is in a large proportion of cases misleading, the behaviour exhibited being explicable by the influence of fear or other powerful emotion; in other cases, cataleptic influences are to be taken into account. Animals, when in danger, naturally remain quiet from instinct, or a vague perception that it is the best way to escape notice. It is erroneous to assume that an animal forms any abstract idea of death in such cases.

The writer relates in minute detail the histories of cases of feigning injury, etc., in the Chickaree, observed by himself, which show that, in addition to the above noted instinctive behaviour, deliberate feigning was practised in a way which led to successful escape from confinement. The circumstances indicated that there was the clear perception of relations new to the animals, and adaptation of means to accomplish ends. The Chipmunk does not seem to be capable of this, though it appears to become cataleptic from fear sometimes. Experiments with traps had also shown the great superiority of the intelligence of the Chickaree over that of the Chipmunk.

Dr. Bell's appendix deals especially with the Chickaree: its geographical distribution; feeding habits, and intelligence connected therewith; its courage and adaptability to its varying surroundings, etc.

The Meeting of the American Association for the Advancement of Science.

This, the thirty-sixth meeting, with Professor Langley as President, was held in the buildings of Columbia College, New York, in August. In numbers and in papers presented, the meeting was at least up to the average. While credit is due to the authorities and teachers of Columbia College, to certain local scientific clubs, to the local committee and some others, for their efforts on behalf of the

Association, New York as a city did not meet expectations. The social element was not as strong as at former meetings, and certainly was not in keeping with what the people of New York are capable of, when they are inclined to be hospitable. Canada was, as regards numbers, somewhat feebly represented; in papers presented, rather strong. Below we give abstracts of most of the Vice-Presidents' addresses, and of that of the retiring President. The next meeting is to be held in Cleveland, under the presidency of J. W. Powell, of Washington.

The Prehistoric Chronology of America.

In Section H (Anthropology) the address given by Vice-President Brinton was "A Review of the Data for the Study of the Prehistoric Chronology of America." He said:

The prehistoric period of America dates back from the discovery of the several parts of the continent; and the problem is to reconstruct the history of the various nations who inhabited both Americas in this period. A review of the means at our command to accomplish this divides them into six classes:

1. *Legendary*—This includes the legends or traditions of the native tribes. These often bear a strong resemblance to Semitic or other oriental myths; but the similarity is a coincidence only, and those writers have been led astray who count it for more. The annals of the Mexicans, of the Mayas of Yucatan and the Quichuas of Peru, carry us scarcely five hundred years before the voyage of Columbus, although the contrary is often stated. The more savage tribes practically remembered nothing more remote than a couple of centuries.

2. *Monumental*—The most famous monuments are the stone buildings of Mexico, Yucatan and Peru. By many, these are assigned an antiquity of thousands of years, but a calm weighing of the testimony, places them all well within our era, and most of them within a few centuries of the discovery. The celebrated remains of Tiahuanuco in Peru are no exception. Much more ancient are some of the

artificial shell heaps along the coast. They contain bones and shells of extinct species in intimate connection with stone implements and pottery. They furnish data to prove that the land was inhabited several thousand years ago.

3. *Industrial*—The industrial activity of man in America may be traced by the remains of his weapons, ornaments and tools, made of stone, bone and shell. In most of the deposits examined, specimens of polished stone and pottery testify to a reasonably developed skill, but in the Trenton gravels and a few other localities, genuine palæolithic remains have been found, putting man in America at a date coeval with the close of the glacial epoch, if not earlier. The vast antiquity of the American race is further proved by the extensive dissemination of maize and tobacco, tropical plants of southern Mexico, which were cultivated in remote ages from the latitude of Canada to that of Patagonia.

4. *Linguistic*—It is believed that there are about 200 radically different languages in North and South America. Such a confusion of tongues could only have arisen in hundreds of centuries. The study of these languages, and of the gradual growth of their dialects, supply valuable data for the ancient history of the Continent.

5. *Physical*—The American race is as distinctively a race by itself as is the African or white race. Although varying in many points, it has a marked fixedness of ethnic anatomy and always has had. The oldest American crania collected from the most ancient quaternary deposits are thoroughly American in type.

6. *Geologic*—As the discovery of implements in glacial deposits located man on this continent, at least at the close of the glacial epoch, this carries his residence here to about 35,000 years ago. But there is no likelihood that he came into being on this continent. He could not have developed from any of the known fossil mammalia which dwelt here. More probably some colonies first migrated along the pre-glacial land-bridge which once connected Northern America with Western Europe. Later others came from Asia. At that time the physical geography of the Northern hemis-

phere was widely different from the present. These various data have been as yet but imperfectly studied; when they shall have received the attention they merit, we may confidently calculate on a large increase in our knowledge of the course of events in ancient America.

Vegetable Parasites and Evolution.

Vice President Farlow in Section F (Biology) discussed "Vegetable Parasites and Evolution." The following is an abstract of his address:

Botanists, as a rule, have contented themselves with considering the ancestral relations of the higher plants only. Hardly any attempt to elaborate a scheme of development for the whole vegetable kingdom has as yet been made. This is due in part to the fact that the older plants, unlike the older animals, have left almost no fossil records. It is my purpose here not to attempt a complete botanical tree of life, but to inquire into the origin of a single group of plants, vegetable parasites.

A parasitic plant is one obliged to obtain its organized material from other plants or from animals. Parasites subsisting on dead matter are called saprophytes; those on living substances true parasites. The mould on bread is a saprophyte; the potato-rot fungus a true parasite. Excepting plants like the Indian pine and dodder, most parasites belong to the fungi.

The parasite is usually destructive to the host on which it lives. As the mould grows, the bread disappears; the decay of the potato is due to the ravages of the potato-rot fungus. There are several cases, however, in which, for the host and parasite, mutual advantages are claimed. Notably among these are lichens, in which the green cells and fungous filaments are said to be reciprocally beneficial. To this mutual relation has been given the name symbiosis. The benefit derived by the fungus is real; that for the alga is at best hypothetical, for the green cells seem to grow more luxuriantly when free from the fungus. A second case of so-called symbiosis is the root-fungus, Mycorhiza,

and the trees on which it grows. Frank, who discovered the Mycorhiza, supposed that the fungus conducted all the food from the soil into the roots. However this may be, Hartig has shown that the fungus is not necessary to the tree, and Kamienski claims that it is really harmful. Both lichens and Mycorhiza must be considered cases of true parasitism, not symbiosis. The animals and unicellular algæ offer more promising examples of symbiosis, for here the products of assimilation are such as to render a condition of mutual benefit readily conceivable. Botanists seem to have over-estimated the number of cases of symbiosis, and to have included among them cases of true parasitism.

It is highly probable that vegetable parasites originated at an early epoch from non-parasitic forms. There is reason for believing that the earliest forms of undoubted plants were unicellular and not unlike Protococcus. These once having established themselves, there is no reason why they should not have been quickly followed by colorless, unicellular parasites like Chytridium. The transition from Protococcus to Chytridium is easily effected. As soon as a Protococcus has developed the power of attaching itself to other protococci, lost its chlorophyl and developed means of penetrating the wall of its host, it has assumed all the essentials of a Chytridium. Whether the filamentous and higher parasites have been derived from such chytridiaceous forms is not easy to say. It is, however, perfectly possible.

It seems not unreasonable to suppose that true parasites may have originated at a very remote period from non-parasitic plants; but may not saprophytes first have developed from non-parasitic plants, and then parisites from saprophytes? The question is one not to be answered from actual knowledge; the probabilities seem to favor the view of parasites originating directly from forms like Protococcus.

Still another possibility must be noted. Could not both parasitic and non-parasitic plants have originated simultaneously from a protoplasmic ancestor, neither animal nor plant? The Myxomycetes favor this view. But the Myxomycetes may be animals and, if plants, they have remained in a low condition and have no off-shoot representing higher

forms. It is safe to say, however, that the more highly developed parasites have not had their origin in the Myxomycetes; and there is very little to lead us to believe that parasitic and non-parasitic plants were simultaneously developed from primitive protoplasmic structures.

When one regards fungi as a single class of plants, and attempts to trace a clear connection between the highest and lowest members, he finds numerous gaps which cannot well be filled. A general parallel, however, exists between chlorphyll-bearing algæ and fungi, and one is forced to ask whether the order of development has not been from the lowest to the highest algæ, and whether the different groups of fungi have not arisen from different groups of algæ at different periods in the process of evolution. This view seems more in accordence with existing facts than any other.

Economy in the Management of the Soil.

The members of Section I (Economic Science and Statistics) listened to an address by Vice-President H. H. Alvord on "Economy in the Management of the Soil." The following is a partial synopsis:—

The American Association was largely modelled after the British. The section of economic science and statistics was organized at its thirty-first annual meeting. The largest and most important meeting of the section was in Philadelphia, in 1884. The work of the section in the past has included the subjects of education, foods, food-fishes, forestry, and others not classified. The tendency has been toward the many economic problems connected with the material wants of man. Tracing the visible wealth of the country to its source, we find that it has all, with insignificant exceptions, been produced from the soil. Generation after generation has recklessly drawn on the stored fertility of the land, with no systematic effort at restitution. The value of all the possessions of our people, land excepted, does not equal the sum total of three years production of our industries.

For food, clothing, shelter and fuel, we depend almost

entirely, directly or indirectly, on the soil. The rapidly increasing demands of our own country are met, and more than met, so far as mere quantity is concerned, for a great surplus is annually sent abroad. For twenty years, agricultural products have constituted three-quarters of all the exports from the United States. And it is manifest that this superabundance of soil products will continue despite all possible increase in population, at least well into the next century. But the wisdom and economy of our present systems of production and disposition is a very different matter. A steady draft continues upon the important elements of fertility, with no adequate system of restitution or recuperation for the soil. Every crop removed from the land diminishes its store of plant food, and this reduces its productive power. The three most important elements of plant food are nitrogen, potash and phosphoric acid. The effect upon the soil depends upon the disposition of the products embodying these. Fortunately very large parts remain upon or are returned to the land; on the other hand, there are vast losses from waste, besides the portions sent to foreign lands.

With our rapidly increasing population and a constantly increasing fertility of soil, we have presented to us questions of the gravest importance. By the wasteful processes prevailing, we are expending our very substance and daily adding to a burden under which generations to come will stagger. These considerations should increase our regard for the business of farming and our interest in it. We should all rejoice at the revival of agricultural studies and the increasing number of able men who are making them their life's work. Let me cordially invite continued contributions to the proceedings of this section—upon foods, fabrics, forestry, industrial education and other topics closely related to our material welfare.

International Congress of Geologists.

Vice-President Gilbert, in Section E (Geology and Geography) dealt with "The Work of the International Congress of Geologists." His address may be condensed as follows:—

Eleven years ago the Association met at Buffalo. It was the year of the Centennial Exhibition, and we were honored by the presence of a number of European geologists. This naturally opened the subject of the international relations of geology; and the proposition to institute a congress of geogolists of the world took form in the appointment by the Association of an International Committee. The project thus initiated found favor elsewhere, and there resulted an international organization which up to the present time has held three meetings. It convened first at Paris in 1878, then at Bologna in 1881, and at Berlin in 1885. Its next meeting will be held in London next year, and an endeavor will be made to secure for the United States the honor of the fifth meeting. The original committee of the association has been continued with some change of membership, and has sent representatives to each session of the congress.

The work of the congress, as originally conceived and as subsequently undertaken, has for its scope geologic nomenclature and classification, and the conventions of geologic maps. The particular classifications attempted are the establishment of the major divisions used in historic and stratigraphic geology and the subdivision of volcanic rocks. In nomenclature three things are undertaken; first, the determination of the names of historic and stratigraphic divisions; secondly, the formulation of rules for nomenclature in palæontology and mineralogy; and thirdly, the establishment and definition of the taxonomic terms of chronology (period, epoch, etc.,) and of stratigraphy (system, series, etc.) The map conventions most discussed are colors, but all signs for the graphic indications of geologic data are considered. The congress has also undertaken the preparation of a large map of Europe, to be printed in forty-nine sheets. The work accomplished is as follows: Agreement has been reached as to the rank and equivalence of the taxonomic terms employed in chronology and stratigraphy; a set of rules for palæontologic nomenclature has been adopted and many sheets of the map of Europe have been prepared for the engraver. A partial classification of stratified rocks has been agreed to and also a partial

scheme of map colors; but the reports of proceedings indicate that action in these matters is tentative, not final.

The terms and the order adopted by the congress are as follows: Of stratigraphic divisions, that with the highest rank is group, then system, series and stage. The corresponding chronological divisions are era, period, epoch and age. There are propositions before the congress to distinguish the names of individual groups, systems, series and stages by means of terminations. Thus it is proposed by one committee that every name of a group shall end in "ary"—Tertiary, Primary, Archeary; that names of systems shall end in "ic"—Cretacic, Carbonic, Siluric; that names of series shall end in "ian"—Eifelian, Laramian, Trentonian; and that names of stages shall terminate in "in." Another committee has suggested that "ic" be used for stages instead of systems. The adoption of such a plan would enable a writer to indicate the taxonomic rank of a terrane without adding a word for that purpose. Palæontological nomenclature was another point considered by the congress. From one point of view, palæontology is a part of geology, but from another, it is a part of biology. In so far as it names genera and species, it is purely biologic, and it would seem proper that the students of fossils unite with the students of living animals and living plants in the adoption of rules of nomenclature.

No action has yet been taken as to the nomenclature of mineralogy, and action has also been deferred on the classification of eruptive rocks. It is to be hoped that it will be deferred *sine die*. The congress is attacking its two most important undertakings, the classification of terranes and unification of map colors through the geological map of Europe which it is preparing. It is the opinion of many that the smallest unit of such classification should be the stratigraphic system. What is this? The congress implies a definition in saying that a system includes more than a series and less than a group, and that the Jurassic is a system, but this gives only a meagre conception and we need a full one. As the problem of classification demands a true conception of a system, and as there is reason to

believe that a false conception is abroad, it is proper that in seeking the true one we begin with the elements.

The surface of the land is constantly degraded by erosion, and the material removed is spread on the floor of the ocean forming a deposit. This process has gone on from the dawn of geologic history, but the positions and boundaries of land and ocean have not remained the same. Crust movements have caused the submergence of land and the emergence of ocean bottom, and these movements have been local and irregular, districts here and there going up while other districts have gone down. The emergence of ocean bottoms exposes the deposits previously made and subjects them to erosions. In transportation from its region of erosion to its place of deposition, detritus is assorted, and so it results that deposits that are simultaneous are not everywhere the same. Many of these variations in deposits are correlate with depth of water and distance from shore, and it results that elevation and subsidence in regions of continuous deposition, produce changes in the nature of the local deposit. If now we direct attention to some limited area, and study its geology, we find that under the operation of these general processes it has acquired a stratigraphical constitution of a complex nature. Its successive terranes are varied in texture. Breaks in the continuity of deposition are marked by unconformities. The fossils at different horizons are different, and when they are examined in order from the lowest to the highest, the rate of change is found to vary, being in places nearly imperceptible and elsewhere abrupt.

It is by means of such features as these—that is, by lithologic changes, by unconformities and by life changes—that the stratigraphic column is classified into groups, systems, series and stages. A system is a great terrane separated from terranes above and below by great unconformities or great life-breaks, or both. Smaller unconformities, smaller life changes and lithologic changes are used for the demarcation of series and stages; and, on the other hand, exceptionally great unconformities and life-breaks are used to mark groups. As the same criteria determine groups, sys-

tems and series, differing only in degree, the precise definition of the term system is impossible, and in many cases the grading of a terrane as a group, a system or a series, is largely a matter of convenience.

The Chemistry of Nitrogen.

Vice-President Albert B. Prescott discussed "The Chemistry of Nitrogen as disclosed in the Constitution of the Alkaloids," before Section C (Chemistry). He said in substance:

The character of nitrogen is a challenge to chemical skill. Mocking us by its abundance in its free state, the compounds of this element are so sparingly obtained that they set the rate of value in supplies for the nourishment of life. The agent chosen and trusted for projectile force in arts of war and of peace, yet the manufacture of its most simple and stable compound has been a vain attempt, and it is one urged anew by the chemical industries. Moreover, nitrogen holds the structure of the aniline dyes, and governs the constitution of the vegetable alkaloids. In research, the nearest approaches to the molecule, as a chemical centre, have been reached through organic chemistry. Carbon was the first and hydrogen has been the second element to give to organic chemistry a definition. At present, carbon is looked upon as the member for fixed position, and hydrogen as the member for exchange, in organic families. Nitrogen comes next in turn to receive attention. The study of the carbonaceous compound of nitrogen promises to do for organic chemistry what the latter has done for general science.

The speaker then outlined the history and present state of the structural chemistry of the vegetable alkaloids, as follows:

(1) *Nitrogenous bases as derivatives of ammonia.* It was maintained by Berzelius that the vegetable alkaloids were unions of entire ammonia with organic radicals. In the third decade, Liebig held that these bases were compounds of amidogen, the larger part of ammonia. In 1849,

the French chemist, Wurtz, produced the first chemical derivatives of ammonia by substitution. The names of "methylamine" and "ethylamine" then given by our own Dr. Sterry Hunt, are the names that remain. A. W. Hofmann, who still directs the Berlin laboratory, when in England in 1850, devised means of introducing groups of one or two, or all three atoms of hydrogen, held by the one atom of nitrogen in ammonia. This reaction of Hofmann is of constant and still increasing usefulness, both in research and manufacture. Simply as derivatives of ammonia, however, the structure of vegetable alkaloids has not been revealed.

(2) *Nitrogenous bases represented by aniline.* These artificial alkaloids, extensively produced as color-stuffs from coal-tar materials, are of the so-called "aromatic" constitution, first brought to light by Kekule in 1865. The vegetable alkaloids, when broken up, yield "aromatic" products, but they have not been found actually to possess the "aromatic structure" in any form of it known earlier than 1870.

(3) *The pyridine type in the vegetable alkaloids.* The constitution of the pyridine and quinoline series was ascertained by Koerner and by Baeyer in 1870. These bodies can be obtained from bone-oil and from coal-tar. They are of a remarkable chemical structure. Like aniline, they have the closed chain of six positions, but, unlike aniline, they have one of these positions held by nitrogen. The introduction of the atom of nitrogen into the closed ring so affects the qualities of the molecule that stable addition products are formed. About 1879 it began to appear that the vegetable alkaloids in general are of the pyridine type, of "aromatic" composition.

In this type the structure of ammonia is not violated; and the theories of Liebeg, Wurtz and Hofmann, are not superseded. Within the last three or four years, the veil has been drawn from the structure of the chief alkaloids of plants. Even before that, the alkaloids of black pepper, tobacco and hemlock, of very simple composition, were studied with success. The alkaloids of the belladonna

root, the cinchona bark and the cocoa leaf are now subject to an increasing measure of constructive operation in the laboratory. Morphine is convertible into codeine, and the efforts to convert strychnine into brucine and cinchonine into quinine ought to succeed.

The necessary studies of position in the pyridine molecule are being entered upon. Some good medicinal alkaloids are being made by art. It may come that the identical alkaloids of nature will be made by art. Not by chance efforts, however, nor by premature short-cuts, but, if at all, through the well earned progress of the world's chemistry will these results be gained. And it speaks enough for the rate of this progress to say that one of the very first of the forward steps here recounted was taken by a man still living as a contributor. Due honor for what his hands have done, and all gratitude for what his eyes have seen.

A Decade of Evolution.

In the evening there was a general session in the library to listen to the address of Professor Morse, the retiring president of the association. It was undeniably warm, but every seat had been taken, and there were not a few who listened standing. The speaker began by explaining how he had become engaged in the effort to collate the work that had been done by Americans toward the illustration of evolution during the last ten years. He first quoted the testimony among American naturalists to the derivative theory of those who had written about birds, and said that every principle claimed by Darwin had been illustrated by these little things. Dr. Brewer's work upon nesting was touched upon. O. P. Hay was quoted as authority for the fact that red-headed woodpeckers had taken to hoarding—storing up acorns which had worms in them, and fattening, so to speak, their future food.

Then the speaker glanced from birds to insects, and claimed that there was remarkable individuality in them. Some were superior, some inferior. E. G. Peckham established clearly that wasps could distinguish between colors,

and had some amount of memory. Another observer was quoted as authority for an instance of remarkable reasoning power on the part of a hornet that had captured a locust under difficult circumstances, but managed by much ingenuity to fly off with its victim in triumph. The general deduction was that insects were not to be considered as automata acting by instinct, but reasoning creatures, having likes and dislikes of their own, and solving problems presented to them by the exercise of intellectual faculties. He continued his defence of Darwinianism by stating that the missing links which the public so hungrily demanded were being found on every hand. He quoted the emphatic words of Professor Cope, uttered in 1874, and said that in his magnificent collection at Philadelphia he had many fossils that were clearly intermediate. Some brachiopods were singularly favorable specimens, and some gasteropods were nearly equally so. Dr. Putnam and Dr. Scudder were brought forward as authorities for the interesting fact that insects in the palæozoic age were intermediate in some features, particularly their eyes.

Since 1876 Professor Marsh and Professor Cope had published remarkable works upon extinct vertebrate life. Professor Marsh demonstrated that the brains of early mammals were remarkably small, in spite of the huge size of their owners. It was not strange that they were succeeded after the next geologic age by smaller animals with larger brains. The *Dinocerta* were typical creatures for their bulk and smallness of brain, and it might well be that they were too sluggish and too stupid to protect their offspring. Professor John Fiske had advanced a similar argument with regard to the disappearance *in toto* of the early races of mankind. Another observer had pointed out the reptilian character of the *Monotremata* of Australia, obviously a class of intermediate creatures that had survived from a prior geologic age, and were anachronisms to-day.

The speaker's next statement was concerning a creature with a rudimentary third eye; and he observed that no sooner had Dr. Thomas Dwight, in his attack upon Darwinism, limited possible vision in vertebrates to two eyes,

than this trioptic creature was discovered. His next argument was drawn from the modifications of the legs of crustaceans, particularly decapods. Then he considered the work of a writer who had examined into the ill-effects, physically, which had resulted from man's deserting the posture of his quadrumanous ancestors and assuming an erect attitude. This author advanced a curious series of facts concerning the valves of men's veins, and specially concerning those veins where there are no valves. In an erect posture this absence of valves is detrimental, but in the ancestral attitude of the mammal man, it was a matter of no moment.

Professor Morse's address was so replete with statements of fact, that the above must be considered a very imperfect and fragmentary account of it; indeed he was obliged to omit in the reading much of what he had written.

Reviews and Book Notices.

The Geology of England and Wales; second edition, by Horace B. Woodward, F.G.S.

This is a new edition of a well-known book, which has for many years been in the hands of all working English geologists, and of those abroad interested in making comparisons with the geology of England.

The present edition is considerably enlarged, and gives a fair account of the recent changes in the nomenclature and classification of English rocks, and which are especially important in the older formations. England is not rich in Eozoic or "Archæan" rocks, and from the occurrence of these in limited areas and with indifferent exposures, there is still some controversy about their nature and arrangement. It is perhaps to be regretted that Dr. Hicks, who has endeavoured to establish these older formations, had not at once boldly called his "Dimetian" Laurentian, and his "Pebidian" Huronian. There can be little doubt that they are the equivalents of the rocks so named by Logan, and to have accepted the names already given might have

tended to avoid controversy. The author accepts Sedgwick's term Cambrian for the next rocks in succession, calling the Longmynd and Menebian Lower Cambrian, and leaving us to choose whether we shall call the Lingula Flags and Tremadoc Series Middle or Upper Cambrian. So, in like manner, he seems to leave us to choose as to whether the Ordovician Series of Lapworth shall be called Upper Cambrian or Lower Silurian, or neither. This is no doubt intended to conciliate opposing geological factions; but it tends to obscure the grand general fact that the rocks from the Longmynd to the Lower Tremadoc, inclusive, hold what Barrande has called the *Primordial* fauna, while the rocks from the Arisaig to the top of the Caradoc, hold the *Second* Palæozoic fauna. This is the real distinction. Both the Cambrian and the Ordovician vary greatly in mineral character, even within the limits of England, but they differ in their fossil contents just as the latter does from the the proper Silurian above it.

This leads to the remark that the book is almost entirely stratigraphical, and gives little information as to fossils. There are, it is true, lists of names, but nothing more, and in this respect the work forms a remarkable contrast to Murchison's Siluria or to Phillip's geology of Oxford. It is in accordance with this neglect of fossils that a little further on we find the term "New Red Sandstone" retained for the Permian and Trias, and the former associated with the Mesozoic. It is no doubt sometimes difficult locally to separate them, but the natural arrangement is undoubtedly to place the Permian in the Upper Palæozoic, and the Trias in the Lower Mesozoic.

As is natural in the Geology of England, a large proportion of the book is devoted to the Mesozoic and older Tertiary, and a very clear and connected account of these beds is given. The Pleistocene and its glacial period come in for somewhat extended consideration, and the various complexities which they present in England, are freely discussed. He appears to admit the following changes:—

1. A period of elevated land and cold immediately after the Pliocene (earliest boulder clay).
2. A period of submergence (shells—sands of Moel Tryphaen, &c).
3. A second period of elevation with glaciers and variable climatal conditions, followed by partial submergence.
4. Modern conditions with early elevation and subsequent slight depression of land.

With the exception of a probably exaggerated value attached to No. 1 of the above table, it is not very remote from the general sequence which we obtain on the wider area of North America.

The book is accompanied by a good geological map, and is altogether a most valuable book of reference to the working geologist, whether in England or abroad.

A NATURALIST'S RAMBLES ABOUT HOME; second edition. revised, pp. 485, New York, D. Appleton & Co., 1887

UPLAND AND MEADOW, pp. 389, New York, Harper & Brothers, 1886.

WASTE-LAND WANDERINGS, pp. 312, New York, Harper & Brothers, 1887.

All of the above works are from the pen of Dr. Chas. C. Abbott, of Trenton, N. J., and are so related in style and matter that they may be reviewed together.

They recount Dr. Abbott's experiences in an ordinary tract of country, no bettter provided by nature than thousands of others; and yet, it seems to have furnished to him themes that at once absorbed his own interest, and in the relation of which he charms the reader, who has any love for nature, into a kind of spell. And what is the real secret of the fascination of these works? We think it lies in the fact that the author has taken Nature's children to his own bosom; he has loved them, and they have, therefore, not refused to give up their secrets, and in such case they never will. What Dr. Abbott has learned, others can learn if they will pursue the same methods. It may be that all may not be able to pour out, in such an artless yet charming manner, what they wish to convey.

No better books than these can be put into the hands of young people. The person that cannot see anything to love in natural objects in his own surroundings after perusing such books, is hopeless. They give what ordinary works, on natural history, fail to do: the methods, step by step, by which the author's knowledge was reached, so that the reader feels stimulated to pursue the same plan; and thus the indirect value of such works becomes far greater than the direct.

We would emphasize another matter. Dr. Abbott's *experiments*, though apparently simple, and in reality simple, are just the kind that in our opinion, are most reliable. He arranged to see animals act under conditions perfectly natural. Such constitutes the very essence of trustworthy experiment. Inferences, under such circumstances, are absolutely reliable, which is more than can often be said of methods more complicated.

The moral effect of such reading is of the best. It makes one feel that there is more in the world to admire than man and his works; and that man is himself but a part of a harmonious whole, though it is his fortune to be at the top.

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