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THE

# CANADIAN NATURALIST

AND

# Quarterly Journal of Science.

## ON THE ORIGIN OF SOME AMERICAN INDIAN TRIBES.

BY JOHN CAMPBELL, M.A.

Member of the Canadian Institute, Society of Biblical Archaeology, Institution Ethnographique, Correspondant de la Société Américaine de France, Délégué du Congrès International des Américanistes, Hon. Loc. Sec. Victoria Institute, &c., Professor in the Presbyterian College, Montreal.

After all the time and talent that have been devoted to the study of aboriginal American languages and antiquities, the materials collected, the societies formed for their investigation, the books written, it is disappointing to find that no one Indian tribe has been satisfactorily connected with any people of the Old World. This phenomenon is capable of explanation in one of three ways; either by the fact that the aborigines of this continent are autochthones; or, that they represent an ancient stock which has entirely disappeared from the older abodes of humanity; or, finally, by the imperfect and unscientific methods that have been employed in all attempts hitherto made to unite the populations of the two hemispheres. The first of these explanations is virtually contained in Agassiz' doctrine of Faunal Centres, no fewer than six of which he found in America. It accords with the traditions of some Indian families, for Dr. Oronhyatekha, a Mohawk, holds that all the Iroquois legends "teach that the red man was created upon this continent." Catlin, the artist and traveller, saw no necessity for showing that the aborigines of North America ever came here from any other part of the world; and Mr. Hubert Bancroft appears to hold similar views in regard to the native races of the Pacific States. The President of the Anthropological Society of Paris lately gave it as his dictum "that the Americans are neither Hindoos, nor Phœnicians, nor Chinese, nor Europeans; they are Americans." The Darwinian theory of the Descent of Man does not necessarily establish relations between the human inhabitants of the New World and those of the Old, yet Mr. W. H. Dall, in his remarks on the origin of the Innuit or Esquimaux, published in the first volume of Contributions to American Ethnology, writing from a Darwinian standpoint, is compelled to admit these relations. He says: "The fact that the home of the highest anthropoid apes is in Africa and also that of some of the least elevated forms of man; that we have none of the higher anthropoid animals, recent or fossil, in America, and none are known anywhere outside of the Asiatic and African regions, tells forcibly against any hypothesis of autochthonic people in America." The second explanation is that of Mr. Clements Markham in regard to northern, and of the Abbé Brasseur de Bourbourg in regard to southern families. The former holds that the Hyperborean Americans are the descendants of Siberian tribes, who within the historical period wholly passed over to this continent; and, according to the latter, the once civilized tribes at least of Central and South America are the remains of the mythic Atlantides, whose continent formerly extended from north-western Africa to the West Indian Islands.

Turning now to the third explanation, that, namely, which charges writers who have failed in their attempts to establish any relations between the populations of the Old World, on the one hand, and of America, on the other, with the use of imperfect and unscientific methods of investigation, it will be found thoroughly in accordance with fact. Careful and full induction is the only true, scientific method to follow in such an investigation; and this induction should regard, first of all, language in its grammatical processes and simpler verbal forms as well as in its relation to tribal, geographical and mythological nomenclature, then physical features, moral and intellectual character, religion, traditions, antiquities or arts, and manners and customs. It is not too much to say that these conditions of successful investigation have not been fulfilled in the case of the vast majority of writers on American origines. Their aim has generally been to prove the truth of a preconceived theory. Such were the attempts of Manasseh Ben Israel, Adair, Lord Kingsborough, and others, to establish the descent of the Indians from the Lost Tribes of Israel, who have lately found, on evidence as valuable, a nobler family of descendants. Such was the Welsh theory, which led Morgan Jones to find the descendants of Madoc's ill fated expedition among the Tuscaroras, and Catlin to detect them in the Mandans. Recently Mr. Lopez. in his Aryan Races of Peru, and Mr. Ellis, in his Peruvia Scythica, have devoted much learning and ingenuity to connect the civilization of the Incas with that of the Indo-European stock. Some of the relations which have been established between the American tribes and certain peoples of Africa, high Asia and the Indo-Chinese area, have been arrived at scientifically it is true, but one naturally asks for the missing link by which the Guanches of the Canary Islands, for instance, may be united with the Aymaras of Peru, or the inhabitants of Pegu, with the Aztecs of Mexico. Such hypotheses, on the one hand, and far fetched derivations, on the other, I seek to avoid in endeavouring to account for some of the American tribes as derived populations.

It is a common error to regard the Indians as members of one great division of the human family. Such a notion finds no support from a study of their languages, religions, customs, or physical and moral characteristics. It is true that most of the American languages are polysynthetic, not all however, but so varied is this polysynthetism that M. Lucien Adam, whose acquaintance with the Ural-Altaic languages specially qualifies him to express an opinion, finds it to consist essentially "in the affixing of subordinate personal pronouns to the noun, the post-position and the verb, a process which equally characterizes the Semitic languages, the Basque, the Vogul, the Mordwin and even the Magyar." To these he might have added many African, Polynesian, and Northern Asiatic tongues. As for that agglutination in connection with which polysynthesis takes place, it prevails more or less among all the branches of Turanian speech, and also in the Tagala and other Malay-Polynesian dialects. Very few American tribes justify by their complexion the name of "red-man," while outside of America may be found red Fulahs, red Kariens, red Koriaks, and many tribes of red Polynesians. In Canada the best known native stocks are the Algonquin and the Wyandot-Iroquois. The external resemblance between these two families arises from similar conditions, necessitating similar appliances and modes of life. Before they were subjected to the influences of civilization, in every other respect than that which a community of condition imposed, they differed *toto coelo* from each other. The Algonquin languages are radically distinct from those of the Iroquois, both in grammatical and in verbal forms. The flatter face, inferior stature, and more delicately formed extremities of the Algonquin are in contrast with the prominent features, the larger proportions and muscular development of the Iroquois. The Iroquois is preeminently a landsman, a warrior and a lover of manly sports, while the Algonquin loves the water, is unaggressive, and spends his spare time in idleness. Taciturnity, with all that it implies, such as the absence of humor, is characteristic of the Algonquin, but not of the Iroquois. The Iroquois was originally a sun-worshipper, but such the Algonquin never was. In fact these two families have nothing in common beyond the mere accidents of condition and certain minor features of life resulting from mutual intercourse. The Algonquin and the Iroquois, who have jointly contributed to the portraiture of the ideal red-man, are the representatives of two families as distinct as any that can be found outside of the Aryan and Semitic areas of the Old World.

In seeking the origin of the Iroquois and Algonquin families, language must be our chief guide, and first in language stand grammatical forms. There are three important differences in structure which separate Algonquin from Iroquois grammar. The former frequently makes use of prepositions like the Aryan and Semitic languages; the latter invariably employs postpositions, like the Turanian tongues. Thus in Cree, one of the most widely distributed Algonquin dialects, *tchik-iskutek* means "near the fire," *tchik* being the preposition "near"; but in Iroquois the same expression is translated by *ontchicht-akta*, in which *akta*, "near," is a postposition. The place of the temporal index in the order of the verb is a second distinguishing feature of the two grammatical systems. In the Iroquois the mark of time is final, although it is sometimes implemented by a prefix to the initial personal-pronoun; thus in *ke-nonwe-s* I love, *ke-nonwe-skwe* I loved, *wake-nonwe-hon* I have loved, and *enke-nonwe-ne* I shall love, *s*, *skwe*, *hon* and *ne* are the indices of present, imperfect, perfect and future time, *nonwe* being the verbal root and *ke* the pronoun. But in Algonquin the temporal index is, in the more important tenses at least, prefixed to the verbal root; so that in *nin gi-sakiha* I have loved, and *nin-ga sakiha* I shall love, *gi* and *ga* are the indices of the perfect and future respectively, *sakiha* the verbal root, and *nin* the personal pronoun. A third peculiarity of Algonquin grammar is that the accusative or direct regimen follows the verb. It is true that the same order appears frequently in Iroquois, but the principle of the latter group of languages, as exemplified in the case of pronominal accusatives, is, like that of neighbouring and allied American tongues, to place the verb after its regimen. As regards phonology, the difference between the Algonquin dialects and those of the Iroquois is well marked. The soft vocalic forms of the Ojibbeway, the Nipissing, the Cree, the Delaware, present a remarkable contrast to the more manly but harsh and guttural utterances of all the members of the Iroquois family. The first clause of the 35th verse, chap. 5. St. Matthew's Gospel, reads in Ojibbeway: "Kagoohween kiya ewh ahkeh; mesah ween ewh ootahkookahjegun"; but in Mohawk it is: "Nokhare ne oghwhentsyate, ne wahoene raouhha naah ne thoraghsidageaseraghkouh." It is true that within the Aryan area similar contrasts appear, as in a comparison of the Italian with the German, but in such cases the influence of climate is recognized, a factor which cannot enter into any comparison of the Algonquin with the Iroquois. Moreover the Algonquin dialects are in this respect, large as is the area they cover, completely isolated; for all the surrounding languages, as well as those which interrupt their continuity, bear a closer resemblance phonetically to the Iroquois than to them. Such are the Tinneh or Athabascan tongues that border upon the Algonquins in the north-west, the Dacotah or Sioux west of the Mississippi, and the Choctaw-Cherokee which originally formed their southern boundary. This isolation of language extends beyond the region of phonology into that of grammatical construction, for the three distinguishing peculiarities of Algonquin grammar, the use of prepositions, the preposition of the temporal index in the verb, and the postposition of the accusative, are neither Tinneh, Dacotah nor Choctaw. That these peculiarities are found west of the Rocky Mountains I know, but the extent of my knowledge does not at present justify me in dealing with the languages in which they appear.

In Central America there is an important family of languages known as the Maya-Quiché. It embraces the Maya of Yucatan, the Quiché and Poconchi of Guatimala, and the Huastec and Totonac of Vera Cruz. Of the Maya, Dr. Daniel Wilson, in his address before the American Association for the Advancement of Science, says, "It strikingly contrasts in its soft vocalic forms with the languages of the nations immediately to the north of its native area." Here then is the same phenomenon that is presented by the Algonquin languages. I do not propose to make the Mayas Algonquin, or the Algonquins Maya Quiché, but simply to indicate their common relation to a parent stock. All the Maya-Quiché dialects use prepositions, and prepositions exclusively, while the surrounding languages, Aztec, Mixtec, Pima, Tarahumara, &c., employ postpositions. The Quiché verb again is the precise analogue of the Algonquin, the only difference being that the pronoun, instead of occupying an initial position, intervenes between the temporal index and the root. Thus in *ca-nu-logoh* I love, *xi-nu-logoh* I have loved, and *ch-in-logoh* I shall love, *ca*, *xi* and *ch* are the indices of present, past, and future time, *xi* and *ch* being the equivalents of the Algonquin *gi* and *ga*, or better still of the Cree *ki* and *ka*. In Maya also the accusative seems to follow the governing verb as in Algonquin. There is, however, in these languages an important syntactical peculiarity which does not appear in Algonquin so far as is known to me; it is the postposition of the genitive. Thus in Maya, *upoc Pedro* "the hat of Peter" reverses the order of the Iroquois, Dacotah and Choctaw, which is that of the English "Peter's hat." The Algonquin dialects follow the latter order, and it may fairly be asked whether this be not a result of surrounding influences rather than one of the original forms of Algonquin speech. Apart from this, however, there are, in the use of prepositions, the preposition of the temporal index and the postposition of the accusative, together with phonetic coincidence, links sufficient to ally the Algonquin with the Maya-Quiché languages.

The next great family of languages which employs prepositions is found in La Plata and Paraguay on the Gran Chaco, and is known as the Mbaya-Abipone, including the Mocobi, Toba, Lengua and other dialects. Here again we meet with "soft vocalic forms," contrasting more or less with the manlier utterances of the Peruvian and Chileno tribes, who almost invariably employ postpositions. The verb again is essentially the same as that of the Quiché, the pronoun intervening between the temporal index and the root; thus in *ne-ya-enagui* I came, *de-ya-enagui* I shall come. *ne* is the index of past and *de* of future time. But in the neighbouring Peruvian and Chileno languages the tempo ral index follows the verbal root as in Iroquois, Dacotah, &c. Of the positions of the accusative and genitive in this family I am not able to speak. It is worthy of note, however, that in Mbaya the adjective follows the noun it qualifies, while in the Maya Quiché and Algonquin languages it precedes, as in the majority of American tongues. The identity in form of the Mbaya and the Quiché verb, a form in itself so peculiar and differing so widely from those of nearly all other American languages, is the main link uniting the earlier fortunes of the Mbaya-Abipone family with the Maya-Quiché and the Algonquin.

Turning now from America, where can the philologist discover a language or group of languages that will satisfy the grammatical conditions of the prepositional American family in comparison? Such language or languages must be soft, abounding in vowel sounds, must employ prepositions, must set the temporal index before the verbal root, and, if we take the Quiché and Mbaya as typical, must also make it precede the pronoun before the root, must postpone the accusative to the verb, and probably the genitive to its governing word and the adjective to its noun. These conditions are numerous enough to satisfy the most exacting critic. I do not profess an exhaustive acquaintance with the grammatical systems of the Old World; but, after a survey of the most important of these, I find one that does fulfil all the conditions, and only one. It is that of the Malay-Polynesian languages, which cover the vast area from Malacca to New Zealand, and from Madagascar to the Sandwich and Easter Islands. Every one who has ever heard of these languages knows that they carry the palm for soft, liquid sounds over all other tongues. They use prepositions, and prepositions exclusively. Their verb is identical in structure with that of the Quiché and Mbaya. Take, for instance the verb "to make" in the language of the Tonga or Friendly Islands, which is *gnahi*, and compare it with the corresponding Mbaya verb *yoeni:* the Tongan *ne-oo-gnahi*, I made, and *te-oo-gnahi*, I shall make, are not simply analogous to, but identical with the Mbaya *ne-ya-yoeni*, *de-ya-yoeni*. In the case of the accusative, *na-ia-gnahi he togi*, "he made axes," is a Tongan sentence exhibiting its position after the verb in the Malay-Polynesian languages, thus furnishing a fourth point of agreement between these languages and the prepositional American forms of speech. The nominative was found to precede the genitive in the Maya-Quiché, and this is its position in the Tongan, as in *tama he mataboole*, "the child of the chief." Finally, in Mbaya the adjective follows the noun, and the Tongan *he tangata lille*, "a man good," shows that it is thus in accordance with Malay-Polynesian order. There are many other resemblances uniting the two groups thus compared, such as the absence of gender and the substitution for it of a distinction between nouns as animate and inanimate, the employment of the first person plural in an exclusive and in an inclusive form, the formation of derivative nouns, etc.; but, as these are common also to many Turanian languages of Asia, they do not aid in determining the origin of Algonquin, Maya and Mbaya speech.

The grammatical forms of the three American families under consideration being proved Malay-Polynesian, and no other languages in any kind of geographical relation to America, whether in Eastern Asia, Western Europe and Africa or the Papuan and Australian areas, presenting anything analogous to them, it is to be expected that they should be confirmed by equally close lexical resemblances. The limits of this paper will not permit me to set them forth at length, and I must, therefore, refer the reader to my vocabularies published in the *Canadian Journal*. But a few may serve to indicate how close these resemblances are:

| | Algonquin. | Malay-Polynesian. |
|---|---|---|
| man........ | linnon *Delaware*, ilenni *Shawno*, renoes *Sankikani*, nemarough *Virginia*, | lanan *Java*, ulun *Malagasy*, remau *Amblaw*, maroka *Gilolo*, |
| | ethini, eyinew *Cree*, | tano *Tahiti*, ohana *Tonga*, |
| | menapema *Miami*. | mondemapin *Gani*. |
| woman...... | weewan *Ojibbeway*, wewimow *Cree*, | wewina *Teor*, wahine *Sandwich*, |
| child........ | enese *Narraganset*, | anak *Malay*, &c. |
| | pappoos *Piankashaw*, | bibigi *Tonga*, |
| boy......... | necovis *Micmac*, | ngofa *Tidore*, |
| | bawtoos " | budak, *Malay*. |
| | unquece " | anak " |
| girl......... | epidek " | opideka *Galela*, |
| head........ | wile *Delaware*, | ulu, *Malay* &c. |
| | wilan *Shawno*, | ilon *Malagasy*, |
| | uppa *Algonquin* | oupoko *New Zealand*. |
| mouth...... | madoon *Penobscot*, mitoon *Cree*, | moudoo *Tonga*, motoo *Mariannes*, |
| | namadthun *Bethuck*. | numatea *Amblaw*. |
| tongue...... | weelauloo *Penobscot*, | ileeloo *Tonga*, |
| | otaineni, *Ojibbeway*, | tumoma *Matabello*, |
| tooth........ | nibit *Algonquin*, wipit *Delaware*, | niffi *Malagasy*, afod *Gani*. |
| nose........ | yoch *Ojibbeway*, | iuka *Morella*, |

| | ALGONQUIN. | MALAY-POLYNESIAN. |
|---|---|---|
| eye | miskichi *Cree.* | masson *Malagasy.* |
| ear | touwango *Penobscot.* | tayinga *Tagala.* |
| hand | niligee *Shawno.* | ngalan " |
| | nuchk *Delaware.* | ingoa *New Zealand.* |
| foot | kussie *Shawno.* | kakee *Malay.* |
| black | kusketa *Cree.* | kokotu *Tidore.* |
| | mokkum *Algonquin.* | moitomo *Bolanghitam.* |
| white | wabi *Algonquin*, wompi *Natick.* | babut *Ahtiago.* umpoti *Cajeli.* |
| red | mik *Cree.* | mia Sula |
| bad | mattik *Nanticoke.* | maduki *Bouton.* |
| good | wulillisiwi *Delaware.* | weel *Pelew.* |
| | meyoo, mithoo *Cree.* | mai *Lariki*, maitai *Tagala.* |
| butterfly | kwakwapisew " | kupukupu *Malay.* |
| axe | togkunk *Algonquin.* | togi *Tonga.* |
| canoe | wuskiwoose, oot *Cree.* | wog *Gani*, oti *Tidore.* |
| bow | uchape, *Cree.* | jobi-jobi *Tidore*, djub *Sula.* |
| bread, food | mechim " | macunnan *Malay.* |
| grass | muskooso " | moochio *Tonga.* |
| sky, heaven | heyring *Shawno.* | harani *Sandwich.* |
| nut | pukan *Cree.* | pooe *Pelew*, beequee *Malay.* |
| sleep | nebat *Micmac.* | moopat *Pelew.* |

| | MAYA-QUICHE. | MALAY-POLYNESIAN. |
|---|---|---|
| axe | baat *Maya.* | peda *Sula, &c.* |
| bad | ilil *Maya*, tziri *Poconchi.* | ollo *Java*, atoro *Galela.* |
| belly | pam *Quiche.* | pompon *Salayer.* |
| break | pax " | fachi *Tonga.* |
| body | cucut *Maya.* | gete " |
| fight | tock " | tow " |
| bow | pimp " | panna *Malay.* |
| come | pet *Quiche.* | paituco *Baju.* |
| ear | leexiceu *Maya* | likan *Ahtiago.* |
| good | alhua *Huastec.* | alla *Baju.* |
| head | hol *Maya.* | ulu *Malay.* |
| heaven | taxah *Poconchi.* | tahua *Marquesas.* |
| leaf | lu *Maya.* | lo *Tonga.* |
| man | illauh *Huastec.* | lelah *Baju.* |
| neck | kay *Maya.* | gia *Tonga.* |
| night | agab *Quiche.* | gubio *Bolanghitam.* |
| rain | chuluhaa *Maya.* | kull *Pelew.* |
| skin | keuel " | koli *Sula.* |
| small | mehen " | mahe " |
| star | ghumil *Poconchi.* | umali *Camarian.* |
| flower | lol *Maya.* | lolum *Sanguir.* |
| ant | zinic " | singa *Teor.* |
| frog | xtutz " | codac *Malay.* |
| fly | xlem *Quiche.* | kolang *Mysol.* |
| bone | bak " | buko *Sanguir.* |
| saliva | tub *Maya*, chub *Quiche.* | tefoo *Mysol.* kivi *Gilolo.* |
| shoe | xahab *Quiche*, chanal *Maya.* | guiapoo, quenella *Malay.* |
| pot | cucul *Maya.* | quell *Pelew.* |

| | MBAYA-ABIPONE. | MALAY-POLYNESIAN. |
|---|---|---|
| boy | yonigi *Mbaya.* | anak *Malay &c.* |
| child | niganigi " | inianak *Ceram.* |
| earth | alobo *Mocobi.* | lupa *Tagala.* |
| | iigodi *Mbaya.* | tougoutoo *Tonga.* |
| eye | natocle *Abipone* | matacolo *Ceram.* |
| face | natobi *Mbaya.* | naogabio *Gilolo.* |

| Mbaya-Abipone. | Malay-Polynesian. |
|---|---|
| fish .........noay *Mocobi.* | nau *Gilolo.* |
| moon.........epenai *Mbaya.* | bouan *Tagala.* |
| tongue .......noqueligi " | nangaladi *Gilolo.* |
| white.........yalaga *Mocobi.* | kuloh *Celebes.* |
| name .........oonagadi *Mbaya.* | hingoa *Tonga.* |
| come..........enagui *Mbaya*, anac *Toba.* | inokere *Tidore*, maika *Celebes.* |
| food..........geeenique " | genanga *Tonga.* |
| make .........yoeni " | gnabi " |
| bad ..........beagi " | behei *Amblaue.* |
| day ..........nagata *Mocobi.* | hangat *Bouru.* |

The poverty of my collection of Mbaya-Abipone words places the comparison of these dialects at a great disadvantage as contrasted with the Algonquin and the Maya-Quiché, although I must confess that even in my short vocabulary many terms appear which have no corresponding forms among the Malay-Polynesian languages known to me, but exhibit decided evidence of a Turanian origin. Similar words occur in the Maya-Quiché and Algonquin vocabularies, and may be the result of admixture on the part of the peoples employing them with other American tribes of Turanian derivation. Thus *naccuta*, the Mocobi word for "hair," is Aymara Peruvian, Kadiak and Asiatic Tchuktchi, languages whose grammatical structure is totally different from that of the Mbaya-Abipone.

Language naturally leads to mythology and religion in such an enquiry as this. According to Sir John Lubbock and Mr. Tylor, the Polynesians do not worship the heavenly bodies. I do not know whether this is the case with the Mbaya-Abipone family or not, but solar worship had at least no prominence among the Maya-Quichés, and was unknown among the Algonquins before the adoption of the Delawares into the Iroquois confederacy. On the other hand, the Dacotahs, Iroquois, Choctaws, Natchez, Mexicans, Peruvians, Muyscas and Chilenos were sun-worshippers. The heaven of the latter peoples was supposed to be continental, happy hunting grounds in some distant region, or it was celestial above the clouds: but the Algonquin heaven was, like that of the Polynesians, an island in the ocean. The Abbé Maurault, in his Histoire des Abénaquis, says: "Ce Grand-Esprit résidait sur une ile du grand lac (l'Océan Atlantique)." In this we find an evidence of insular derivation. The same appears in the story of the creation of the world. Maui of New Zealand, with whom Mr. Tylor compares the Algonquin Manitou or Monedo, fished up the earth with a hook from the universal ocean, as did Tangaloa of the Friendly Islands. The Quiché Tohil, Tzakoll or

Tockill, who is undoubtedly the Malay-Polynesian Tangaloa or Tagala, according to the Popol Vuh or sacred book of the Quichés, called the earth into being in a similar waste of waters. The Ojibbeways and Delawares tell an identical story of Manitou; while other Algonquin tribes make the rat his agent in the work of creation. The notion of the Ojibbeways of Lake Superior that they inhabited an island, and their habit of alluding to the American continent as such, seemed surprising to Kohl, the traveller, who imagined it to be the result of knowledge acquired by exploration, instead of a necessary result of their system of cosmology. In their un-Darwinian account of the origin of man the Malay-Polynesians, Algonquins and Maya-Quichés agree. The Tagalas of the Philippines believed that "mankind sprang out of a large cane with two joints, and the man came out of one joint and the woman out of the other." In Samoa the tradition is that the first land brought forth wild vines, and from the worms which developed when they rotted men and women were produced. According to the Delawares, Manitou, having brought up the first land from the ocean, made man and woman out of a tree; and, in one of the Ojibbeway legends in Kitchi-Gami, the first man appears among the reeds which Manitou had planted upon the shore. Compare these with the Quiché legend, in which "man was made of a tree called *tzite*, woman, of the marrow of a reed called *sibac*," and there appears an agreement in tradition to which I know of no parallel. I have already stated that the Quiché or Maya-Quiché Tockill is the Polynesian Tangaloa and the eponym of the Tagalas in the Philippines This is confirmed not only by the identity of the Tagalan and Quiché accounts of the creation of man, but also by the appearance of the Quiché deity Bitol in the Tagalan Bathala, just as the Algonquin Waubuno reappears in the Polynesian Ofanu. The Algonquins, Quichés and Abipones agree with some Polynesian peoples in identifying the soul with the shadow; and Mr. Tylor, in his Primitive Culture, draws special attention to "the conception of the spirit voice as being a low murmur, chirp or whistle, as it were the ghost of a voice," a conception common to the Polynesians and the Algonquins.

Space or rather the lack of it precludes my saying anything of the physical and moral characteristics of the Algonquins, Maya-Quichés and Mbaya-Abipones, as compared with those of the Malay-Polynesians. I may simply refer the reader who has any acquaintance with these features in the American tribes under consideration to the sketch of Malay physique and morale in Mr. Wallace's Malay Archipelago, which I have transcribed in part in my article on the Affiliation of the Algonquin Languages in the *Canadian Journal*. It will, I think, be found equally applicable to the American tribes of Malay-Polynesian origin. Before quitting the field of language, however, I should mention that the name of the Delawares, the most prominent originally of all the Algonquin tribes, finds not indeed its counterpart, but its parallel, in the Malay area. It consists in the prefix of the word meaning man, Lenni, to the tribal designation Lenape. So we find Malay tribes calling themselves Oran-Benua, Oran Malaya. Let the Javanese Lanan take the place of the Malay Oran, and the analogy is complete. Of manners and customs I may select one, the Couvade, which Butler, in his Hudibras, thus describes:

> "Chineses go to bed
> And lie-in in their ladies' stead."

This singular practice prevails among the Abipones, but is so far from being peculiar to them that it is found also in Bearn, Congo and Southern India. It is not, therefore, a distinctive mark of ethnic relationship; but, on the other hand, it can interpose no obstacle to the origin which has been claimed for the Abipones, inasmuch as the practice obtains largely among the Malay inhabitants of Borneo, from whose stock the Abipones may have received it. If it be asked whence the Maya-Quichés derived their architectural knowledge and skill, the massive stone structures of the Malay Archipelago, of the Marquesas, Navigators, Easter and Sandwich Islands may give reply. But if the question be, Whence came the snowshoes, the skin dress, the quill ornamentation, the birch canoe, the calumet and the scalping art of the Algonquins? the answer must be, From Northern Asia, where all of these now are or once were found. The Malay immigrant became perforce a borrower and a learner by his change of condition from the circumstances of a tropical and insular to those of a cold or temperate continental home. Athabascans, Dacotahs and Iroquois from Northern Asia taught him their arts of peace and war; but his character, his physical features, his language and his religion he had no need to change, even where he had the power, for they can flourish as well amid the snows of Labrador as under the burning skies of the Indian ocean.

How came the Malay to America? Dr. Pickering, in his Races of Man, replies: "In attempting from any part of Polynesia to reach America a canoe would naturally and almost necessarily be conveyed to the northern extreme of California; and this is the precise limit where the second physical race of men makes its appearance." The same writer holds that "if any actual remnant of the Malay race exists in the eastern part of North America, it is probably to be looked for among the Chippewas (Ojibbeways) and the Cherokees." The judgment of Dr. Pickering, founded upon observation of physical features, led him to sound conclusions in the case of the Algonquin Ojibbeway, but was misleading in the extreme in that of the Cherokee, who is a Turanian of the Turanians. Where the Maya-Quichés and the Mbaya Abipones landed it is hard to say, but in the case of the former there seems to be historical evidence from their own traditions that they once dwelt considerably to the north of their Central American home. The Quiché mythology appears to link the Malays of Yucatan and Guatimala with the Tagalas of the Philippine Islands; and if, as Dr. Pickering says, "San Francisco is commonly regarded in Mexico as being on the route to Manilla," we should find the landing place of the Maya-Quiché colonists at some point on the Northern Californian Coast. There or farther north, probably in Oregon, where Dr. Pickering finds his "second physical race of men," we should place the beginnings of Algonquin life in America. What part of the Malay-Polynesian area they originally came from I am not yet in a position to say; but that it was a Malay rather than a Polynesian region seems evident from the forms *lenni*, *linnon*, *ilenni*, *alnue*, *renocs*, *etc*, denoting man, forms which refer the ethnologist to Java, Borneo and the Moluccas group. In the Illinoans, a tribe of Borneo, we may even find the parent stock of the Algonquin Illinois, who have given their name to one of the United States. From Borneo or some neighbouring region where Papuan influences have more or less corrupted the verbal forms of Malay speech, the Mbaya Abipones must have started for the Western, but to them Eastern World.

I have already stated that my knowledge of the tribes west of the Rocky Mountains hardly justifies me in taking them into account in this discussion of origins. There is one Oregon family, however, that helps to carry the line of Malay migration across the continent, to which I must briefly refer. With the Sahaptin or Nez Percé grammar I have not yet had opportunity to become acquainted. Dr. Latham regards them as the first of the Oregon tribes in point of culture, as in physical appearance more like the Indians east of the Rocky Mountains than any others in the same area, and mentions the fact that the more eastern of them have crossed that great natural barrier into the buffalo region, where they have become hunters, like the neighbouring Algonquin Blackfeet and Shiennes. In the Sahaptins I believe that the link is found uniting the Algonquin stock with the Pacific. Language, however, must be the first test. The number 2 in Cayuse, a Sahaptin dialect, is *leplin*, a form so peculiar that in a collection of over six hundred vocabularies the only one with which I can compare it is *leplu* of the Gani, a language of the Moluccas group. The same formation in *lep* appears in the number 7, *noilip* in Cayuse, but *lepfit* in Gani. Otherwise the Sahaptin numerals have miscellaneous Malay-Polynesian affinities: the Sahaptin *mitat*, 3, connecting with the Paumotuan *neti*, and *veti* of the Isle of Pines, which latter language gives *tahue* as the equivalent of the Cayuse *tawit*, 5, and *noibeti* as that of the Cayuse *noimat*, 8, while the Cayuse *piping*, 4, and *noina*, 6, are represented by the Javanese *puppat* and *nannam*. In the ordinary vocabulary most of the Sahaptin words are found to relate to those of the dialects of Celebes, Ceram, Gilolo, Bouru and other islands of the Moluccas. Thus, dog, which is *mantal* in Williamet, and *naapang* in Cayuse, is *muntoa* in Celebes, and *nawang* in Ceram; man, which is *iniaw* in Wailatpu, and *keewas* in Sahaptin, is *anow* in Gilolo, and *gebba* in Bouru. Knife is *tekek* Sahaptin, and *shekt* Cayuse, and in Ceram the same forms appear as *tuka* and *seite*, while *wals*, another Sahaptin term for the same object, meets us in *oyless* of the Pelew Islands, which would naturally be one of the earlier stages in the north-eastward progress of the Malay emi-grants. Mouth is *mandi* in Williamet, *him* in Sahaptin, and *shumkakoh* in Cayuse, forms which appear in the Tongan *moudoo*, the Teor *huin* and the Javanese *saukum*. The Williamet *hammeih*, house, is the Bouru *huma*; the Klikitat *wassas*, canoe, the Gilolo *wog*; the Williamet *umpium*, day, the Celebes *unureno*, and *umhok*, flesh, in the same dialect, the Celebes *untok*; while *tshal*, another Williamet word denoting tho colour red, is the Gilolo *desoella*. Peculiarly Malay-Algonquin forms are the Sahaptin *bipi*, white, and the Williamet and Cayuse *puti* and *tenif*, tooth, which find their analogues in the Ceram *babut*, the Gilolo *afod* and the Ceram *nifan*. Two peculiar cases of inversion are presented in the Williamet *tshita-pinna*, girl, and *man-tsal*, river, which in Wahai, one of the Ceram dialects, are *pina-hieti* and *tolo-maina*. For further examples I refer to my comparative vocabularies in the *Canadian Journal*, as those stated are amply sufficient to establish a connection of the Sahaptin family with the Malays of the Moluccas group.

In this sketch I have not been able to fulfil all the conditions of perfect induction, but sufficient evidence, has, I think, been adduced to make a case worthy of fuller investigation. The theory of a Polynesian migration to America is one that has been long and generally held, but held in so loose and indefinite a way that it has been barren of ethnological results. The Rev. Richard Garnett has indicated the presence of Polynesian grammatical forms in South America, but has vitiated his comparison by dragging into it the Dravidian languages, which are Turanian as distinguished from Malay. Now, if an American language can be proved Malay in origin, it is thereby cut off from all Turanian connection, for the two systems are radically different. Dr. Edkins, of Pekin, recognizes the distinction, and finds that "on the American continent Turanian and Polynesian linguistic principles meet in the various Indian languages; but so far is he from allowing the relationship of Turanian with Polynesian that he maintains "a Polynesian immigration from the Ocean and a Turanian immigration by the Aleutian Islands and by Iceland and Greenland, which united to form the population of the American continent." Still, like Mr. Garnett, we find Dr. Edkins looking for his Polynesians in South America and in Mexico. If the Mexicans and Peruvians be Polynesians, they are Polynesians with Turanian grammar, vocabulary, religion and arts, and consequently ethnology as a science, on American ground at least, is impossible. Again Dr. Edkins seems to say that in individual American languages Turanian and Polynesian principles meet. This is true to a certain extent, as we have seen in the case of the languages of Malay-Polynesian origin, which, intruding into a Turanian region, could not fail to be partially, but very partially, influenced by their surroundings. Some Polynesian principles may perhaps be detected in the Iroquois dialects, though of this I am not sure. I can find none in the Tinneh, the Dacotah, the Choctaw or the Peruvian; they are essentially and exclusively Turanian.

Let it be granted that the Malay origin of Algonquins, Maya-Quichés and Mbaya Abipones, with Sahaptins, is established, and a foundation is laid for a history of aboriginal America. Javanese, Moluccan and Tagala traditions may be found to check and supplement those of the Algonquins, Sahaptins and Maya-Quichés; and, as the geologist settles the relative ages of his formations by their fossil remains, so words may enable us, in similar grammatical strata, to fix the various periods of Malay migration to this continent. I reserve the consideration of the Turanian American languages for another occasion.

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## ON SOME POINTS IN LITHOLOGY.*

By Prof. James D. Dana.

(*From the American Journal of Science*)

4. *Containing Quartz or not.*—Since quartz is the most universal of the materials of rocks, its presence is least entitled to be made a basis for distinctions among them. In sedimentary deposits, the original of many of the crystalline kinds, it is a very common ingredient owing to their mode of origin, and its more or less abundance is a matter of no great geological importance. Sufficient reasons exist, therefore, for the course pursued by recent writers on lithology in making the presence or not of quartz even in crystalline rocks a basis only for a subdivision under *a kind* of rock. Thus there is under dioryte, *quartz-dioryte*; under trachyte, *quartz-trachyte*; under felsyte, *quartz-felsyte*; and so in other cases.

Syenyte is defined by such authors as consisting chiefly of orthoclase and hornblende. Now a rock made prominently of these minerals often contains also quartz; and the name for the quartz-bearing kind, which a system of lithology using the above-cited terms would seem to require, would be *quartz-syenyte*. To call it "hornblende-granite," as is often done, is at variance with the system which uses the word *quartz* as an affix in other cases.

This term "hornblende-granite," is at variance also with the fundamental idea and nature of granite. Granite is eminently

* Continued from page 48.

a potash-bearing rock. The feldspar is a potash-bearing species; and the mica, whether muscovite or biotite, yields on analysis little less potash than the feldspar, the amount being eight to twelve per cent. These two micas are both present in most granite, gneiss and mica schist: and they are so near akin that they sometimes occur combined in a single crystal—the presence of a little iron in the original material having apparently determined the formation of the latter where it occurs. On the contrary the hornblende of such rocks contains usually less than one per cent of alkalies, and rarely in any kinds over five per cent. Looking to chemical and mineralogical constitution—the true criterion as to identity among rocks—the strongly drawn line is between the mica-bearing series and the hornblende-bearing series. Granite belongs to a *mica* and *potash-feldspar series*; and syenyte, whether quartzless or quartz-bearing, to a *hornblende* and *potash-feldspar series*.

Moreover, the original syenyte, from Syene, Egypt (to which the name "syenites" was applied by Pliny and other ancient writers) is a quartz-bearing "syenites." The larger part of the syenyte of all Archæan regions is quartz bearing. The quartzless kind is seldom met with in Eastern North America, or, as far as explored, in the Rocky Mountain region. There are hornblende granites; but these are granites which contain hornblende *in addition to* the mica and other ingredients.

Transitions are common between granite, hornblende-granite and quartz-bearing syenyte; but they are so also between these and quartzless syenyte, between syenyte-gneiss and ordinary gneiss, between hornblende schist and mica schist, and between these and other rocks. They are throughout lithology a source of difficulty in characterizing kinds of rocks, as already stated. But they do not set aside the fact that the division between the mica and potash-feldspar series and the hornblende and potash-feldspar series is the most reasonable on mineralogical and chemical grounds.

5. *Containing "Plagioclase."*—The fact that the composition of the triclinic feldspars between the extreme species albite, a sodium-aluminum tersilicate, and anorthite, a calcium-aluminum bisilicate, may be explained by supposing them combinations of these species through isomorphous substitutions of the tersilicate and bisilicate (the amount of sodium present determining the amount of tersilicate in the combination, and the

amount of calcium that of bisilicate) was immediately followed by the assumption that these two silicates combined *indefinitely*, and, therefore that all the triclinic feldspars were essentially one species, and for this reputed species the name *plagioclase* has been used. Some ground for the assumption was found in the analyses of the feldspars; but how much was uncertain, because, in several cases, *mechanical* mixtures of one species with another had been ascertained to exist in crystals. Now that Des Cloizeaux has proved, by optical investigations, that several of the species of triclinic feldspars are really species, that is, that the combinations of the two silicates, the tersilicate and bisilcate, are based on *definite* ratios, as in combinations in other departments of chemistry, and that there are not indefinite blendings, the term "plagioclase" has become merely a synonym for "triclinic feldspar."

The consequences to lithology of this introduction of the term "plagioclase" were unfortunately great. It was made a sufficient definition of a rock to say that it consisted of "*plagioclase* and hornblende," "*plagioclase* and augite," and so on; and this is now common in recent memoirs on rocks. It was a convenient idea; for an examination with the microscope is made in a hundredth part of the time required for a chemical analysis.

Now this word "plagioclase" covers compounds varying in the silica afforded by analyses from 43 to 69 per cent.; and in the alkali from all lime (20 per cent.) to all soda (12 per cent.) Anorthite, the lime feldspar, is not oligoclase, even if to the two a common name be applied; they still differ 20 per cent. in silica (which is one-fifth the mass), and also in the alkali present. Expressions like "consisting of plagioclase and hornblende," as in the definition of dioryte, have consequently an immensely wide signification; for the word dioryte is made to cover oligioclase-dioryte, labradorite-dioryte, and anorthite-dioryte.

This confounding of things thus unlike may be called simplifying the science of lithology; but it is a confounding of important distinctions in the view of those who are interested in a definite knowledge of rocks, and in the important geological questions connected with their constitution. Some lithologists recognize the bearings of such questions, and use the qualified terms for the kinds of dioryte above cited. But the most recent turn is in the other direction. Rosenbusch's learned work, the latest, says that the rocks of the "family" of diabase consist of "plagioclase and augite," and that the feldspars, oligoclase, labradorite and anorthite have been observed in them. Dioryte is defined as a "family" of older rocks consisting essentially of "plagioclase and hornblende." Had the different *kinds* of rocks embraced in these families been separately stated and described, the account might have been satisfactory. But, under both diabase and dioryte, the term "plagioclase" is used as if sufficiently defined in itself, and under dioryte it is given with its aggregate signification alone, no mention being made of the particular feldspar the dioryte of different localities contains.*

If a dioryte happens to be porphyritic, it is at once put into the grand division of dioryte porphyry, when the only distinction may be that the feldspar is in defined crystals, the chemical and mineralogical constitution being identical. But if the feldspar of one dioryte contains twenty per cent. of silica more than another and no soda at all, it is still all dioryte.

In geology, it is essential to a thorough study of the questions it has before it that the kinds of feldspars should not be massed under a common name, and that in every case the investigation should be considered unfinished until not merely the amount of silica in the rock is accurately ascertained, but also the particular species of feldspar is correctly and fully determined, however great the labor required to reach a conclusion. The use of the term *plagioclase* in such a case is an acknowledgment of incomplete work, and should be so treated.

But the objection to the use of the term "plagioclase" is still stronger than has been stated. It now includes not only the soda-lime feldspars from anorthite to albite inclusive, but also part of *potash* feldspar. The establishment on an unquestionable basis, of Breithaupt's *microcline* by Des Cloizeaux, and his further observations that this triclinic potash-feldspar is a very common mineral, much of what was supposed to be orthoclase belonging to it, has extended the range of "plagioclase," until it is now almost an equivalent of the general term feldspar, so that "*plagioclase* and hornblende" has, as to chemical constitution, the same signification now with *feldspar* and hornblende.

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* It should be here acknowledged that Rosenbusch's very valuable work bears the title "Mikroskopische Physiographie der massigen Gesteine," so that it does not claim to cover the subject of the chemical or mineralogical constitution of rocks.

6. *Rocks consisting of a triclinic feldspar and mica.*—The term dioryte, formerly defined as a rock consisting of oligoclase or albite and hornblende, has been introduced into the the name of a series of rocks containing no hornblende, but mica instead. Thus: "*mica dioryte*" is defined as a "plagioclase-mica rock" in which mica is substituted partly or wholly for hornblende, and it is called mica-dioryte whichever of the triclinic feldspars be present, even if anorthite. This change in the use of the name dioryte so as to include a rock containing no hornblende, makes "plagioclase" the essential constituent, and places mica and hornblende in a subordinate position, as the heads only of subdivisions.

The remarks made respecting syenyte apply equally here; and also those respecting "plagioclase." A mica-dioryte is like granite, eminently an alkali-yielding rock, the mica (biotite) affording usually ten per cent of potash; and as granites often contain oligoclase as well as orthoclase, the amount of potash and soda in a "mica-dioryte" and a granite may not be very widely different. Dioryte, on the contrary, is prominently a hornblende rock.

*Looking to the mineralogical and chemical constitution of the* rocks, we are naturally led to recognize alongside of a mica and *potash-feldspar* series, which is headed by granite, also a mica and *soda-lime feldspar* series, and to include in the latter the so-called mica-diorytes.

7. *Hornblendic or Augitic.*—Hornblendic and augitic rocks stand apart as a general thing in all systems of lithology. Yet the minerals are essentially identical in chemical composition, and related in crystallization, though different in their occurring crystalline forms and in the angle of the cleavage prism. The identity in composition is so close that chemical analysis is not able to distinguish them. Hence the related eruptive rocks of the hornblendic and augitic series (or those containing the same species of feldspar in like proportions) must have originated in material of essentially the same chemical composition. The relation between the two minerals is thus far closer than between the triclinic species of feldspars.

Nevertheless, too much importance is not given them when each is made distinctive of an independent series of rocks; for the very wide extent to which augitic rocks retain unvaryingly their augitic characters—such rocks constituting full two-thirds of the earth's eruptive masses—shows that the special conditions producing augite, instead of hornblende, whatever they are, have often acted on a vast scale in the earth's history. And so, also, the very wide distribution of hornblendic rocks, especially among the metamorphic kinds, is evidence of a like comprehensive influence of the conditions needed to make hornblende in place of augite. The geological importance of the distinction is reason enough for recognizing it in lithological systems.

8. *Massive or Schistose.*—Massive structure is often made *prima facie* evidence of igneous origin. Granite, with hardly a questioning thought, has usually been placed solely among eruptive rocks. The igneous origin of dioryte even now is hardly left open to investigation by some litholigsts. Serpentine has been in the same category, though at present there are advocates of its metamorphic origin. And so other massive rocks are too likely to be set down as eruptive without a fair investigation. No two rocks are put farther apart in some lithological systems than granite and gneiss; and yet, none are more closely related in constitution and all essential characteristics.

The following are reasons for disregarding this distinction of massive or schistose in classifying rocks, and for allowing a massive structure little weight in deciding the question as to eruptive or metamorphic origin.

(1.) *Massive rocks may be both metamorphic and eruptive.*—Granite, syenyte, with dioryte and other hornblendic rocks, are examples of massive rocks that are of both modes of origin. Many localities where kinds of these rocks occur metamorphic have been described. I will mention two or three from the many I have observed in New England. (*a*) Ten miles east of New Haven, Connecticut, in a railroad cut at Stoney Creek, a bed of granite, having a small northward dip, changes gradually to gneiss, and then to gneiss with some very micaceous mica schist, so that within thirty yards from east to west these three rocks are found constituting the same bed; and the granite is a part of the general gneissic formation of the region. (*b*) The labradorite-dioryte two miles west of New Haven graduates rather abruptly above and below, and also laterally, from a massive rock into a slaty chloritic mica schist, and does this so often and variously, that there is no reason for questioning its metamorphic origin. (c) A hornblende (or actinolite) rock, just north-east of Bernardston, of a massive kind, occurs among thin schistose beds of mica schist and hornblendic schist and is part of a series of metamorphic strata. From a hand specimen either of these rocks would be pronounced eruptive; but observation in the field proves that they are not so.

(2) *Certain kinds of mineral constituents are almost sure to make a massive metamorphic rock when the process of metamorphism is one attended with much heat.*—Hornblende and augite are minerals of this kind. Both are rather fusible, and crystallize readily, so that heat easily obliterates all traces of bedding. This principle alone will account for the fact that the rocks north-east of Bernardston, alluded to above, are massive wherever hornblende is the chief ingredient. It explains also the existence of the massive labradorite dioryte among the schists west of New Haven. Feldspar also, when alone, or accompanied by quartz without any associated mica (as in felsyte, quartz-felsyte, granulyte), is almost sure, under the circumstances mentioned, to make a rock, with the bedding obliterated, in other words, a massive rock; and only with a low degree of heat in the metamorphism, would any original bedding be retained. And even if hornblende is present, there is the same tendency to massive forms. Serpentine is another species that makes almost necessarily a massive rock, whatever the method of origin, because the mineral has nothing in its structure that favors any other condition.

(3.) *Pressure may be a source of schistosity or foliation, and it may also obliterate bedding.*—On the first of these points illustration is not necessary. As to the second, there are many examples in the crystalline limestone region of Western New England, both in Vermont, Massachusetts and Connecticut. At West Rutland, Vermont, as first observed by Prof. Edward Hitchcock, many limestone beds have been cemented by the pressure which gave them their high dip into a bed of great thickness, so that masses as large as a moderate-sized house could be cut out if needed. The component beds are easily distinguished in the southernmost of the three quarries. Moreover, in the middle of the same valley the metamorphism of the limestone stratum was not complete enough to obliterate the fossils—shells, corals and crinoids being distinguishable; so that there could have been no fusion to produce the coalescence. As this welding of beds is so perfect in the limestone, it is reasonable to believe that a similar cause may have acted in the case of feldspathic, hornblendic and augitic rocks, without even the aid of incipient fusion.

(4.) *The sedimentary beds which have been converted into crystalline rocks were often originally massive.*—This is the condition of most conglomerates, and often of coarse sandstones. In such cases there would be no bedding to obliterate; and the production of a massive rock would be a natural result of the metamorphism, whether the heat attending it were great or small. Part of the metamorphic granite of the world may therefore never have been in a pasty state; and so also part of the metamorphic hornblende rocks; some metamorphic felsyte beds, certainly those that are of conglomerate origin, were originally massive.

There is hence reason enough for neglecting the distinction of massive and schistose in drawing out a system or classification of rocks, and for making the question of origin in the case of either kind, the massive no less than the schistose, a subject for careful investigation.

9. *Metamorphic or Eruptive.*—The question whether a crystalline rock is metamorphic and *in place*, or eruptive, is of the highest geological interest; for it is a question as to origin. At the same time, no subject, if we exclude the part of metamorphism relating to the obviously schistose rocks, is in so unsatisfactory a state. With some authors, as above intimated, the question so far as it relates to *massive* crystalline rock is not an open one. On the other hand, when investigation has taken place, opposite opinions have generally been reached. The remedy of this is to be found in more thorough study from a wider basis of facts.

Were the question in all cases rightly decided, lithology would be able to study and compare the two series, and give greater completeness and higher geological value to the descriptions of rocks. Applying different names to the like rocks in the two series is not necessary, unless there is some strong geological reason in favor of it; for when a rock occurs both metamorphic and eruptive the fact is best exhibited if that rock has but one name.

The writer has proposed to distinguish the metamorphic under any *kind* of rock by adding to the name the prefix *meta*; for example, *dioryte* for the eruptive and *metadioryte* for the metamorphic part. But *meta* is here used simply as an abbreviation of the word *metamorphic*, not to indicate a difference of *kind* in the rock.

## Conclusion.

The principal points with regard to rocks which have been brought out in this paper, are the following.

1. The necessities of the science of Geology constitute the most prominent motive for distinguishing *kinds* of rocks; and they should determine to a large extent upon what characters distinctions should be based.

2. In determining the rocks to be grouped as one in *kind* under a common name, near identity in the chemical and mineral composition of the chief constitutents is the main point to be considered; not near identity in their crystalline forms, for isomorphism presupposes diversity of composition.

3. Distinction of *kind* should be based on difference in chemical and mineral constitution as regards the chief constituents. When such difference exists, rocks are different in *kind*, and need, for the purposes of geology, distinct names. If it does not exist, the distinction is only that of *variety;* unless (as in the case of trachyte and felsyte), the very wide extension of the rock under persistent characters makes a distinction of name important to geology.

4. It follows from the preceding, that differences in texture: as coarse, or fine, or aphanitic; porphyritic, or non-porphyritic; stoney throughout, or having unindividualized portions among the stoney grains, and differences in microscopic inclusions; are no basis for distinctions of *kind* among rocks, but only of *variety;* and that *porphyritic structure* is of hardly more consequence than coarse or fine granular.

5. No marked change in the constituents of the earth's erupted material occurred after the close of the Cretaceous period, or just before the commencement of the Tertiary era; and hence, no ground exists for the distinction of "older" and "younger" among eruptive rocks. The "younger" eruptive rocks are essentially like the "older" in chemical composition and their chief mineral constituents; and they differ when at all only in texture and some other points of as little importance—qualities that distinguish merely varieties, and which have proceeded from greater prevalence in these later times of subaerial eruptions.

6. Since "plagioclase" is not the name of a mineral species,—several minerals, of widely different compositions being embraced under it—it is a confounding of differences and resemblances to speak of it as a constituent of a rock. And since it now includes, through the defining of the feldspar microcline, a large part of potash feldspar, which had been supposed to be orthoclase, it has become almost synonymous with the term feldspar. The

"simplicity" its adoption has been supposed to give to lithological system would be greater if "feldspar" were substituted, and with its present range of constitution, the evil would be hardly less.

7. Rocks differing mineralogically, and not chemically, like related hornblendic and augitic rocks (the minerals hornblende and augite being dimorphous), are rightly made distinct rocks, since the difference has depended, to a large extent, on wide-reaching geological operations or conditions, and is, therefore, of great geological significance.

8. Since quartz is the most widely distributed and therefore the least distinctive of the minerals of rocks, it may rightly be regarded as of subordinate importance in the distinguishing of rocks, and hence not only such names as *dioryte* and *quartz-dioryte*, *trachyte* and *quartz-trachyte*, etc., are acceptable, but also *syenyte* and *quartz-syenyte*.

9. Biotite being closely like muscovite in composition, and not less common than it in granites, gneisses and mica schists, and being, moreover, unlike the mineral hornblende in chemical constitution and formula, the rocks in which biotite is a chief constituent cannot rightly be put in the same group with hornblende rocks; or those in which hornblende is a chief constituent in a group of mica-bearing rocks. Consequently the name "mica-dioryte," for a rock containing no hornblende, and the name "hornblende-granite" for a rock containing no mica but hornblende instead, imply alike false relations.

The discussion suggests the following additional remark:

The incapabilities of the microscope and polariscope have favored the use of the term "plagioclase," and have led some investigators to overlook or slight distinctions in chemical constitution. Lithology is to receive hereafter its greatest advances through chemical analyses; for chemistry alone can clear away the doubts the microscope leaves, and so give that completeness to the Science of Rocks which geology requires for right and comprehensive conclusions.

Moreover the researches made in the laboratory to be of real geological value should be, if possible, supplemented by investigations in the field as to transitions among the rocks, and as to other kinds of relations. This field work has often been well done, but not so by all lithological investigators.

The principles presented lead to the following subdivisions in an arrangement of crystalline rocks, exclusive of the Calcareous and Quartzose kinds. Since leucite is a potash-alumina silicate, like orthoclase and microcline (it affording twenty per cent. or more of potash), it is here referred to the same group with the potash feldspars; nephelite, sodalite and the saussurites being eminently soda-bearing species, they are included with the soda-lime feldspars (anorthite to albite). This reference for lithological purposes of these minerals is sustained by their resemblance to the feldspars in constituents, and also in the quantivalent ratios between the alkalies alumina and silica, this ratio being in leucite 1:3:8, as in andesite, and in sodalite and nephelite 1:3:4, as in anorthite. The term *potash feldspar*, as used in the headings below, is hence to be understood as covering orthoclase, microcline and leucite; and *soda-lime feldspar*, as including the triclinic feldspars from anorthite to albite, and also nephelite, sodalite and the saussurites.

The arrangement is as follows. In the first series, the rocks graduate into kinds which are all feldspar, and into others that are all mica; and yet the amount of potash present is approximately the same.

I. The Mica and Potash Feldspar Series: including Granite, Granulite, Gneiss, Protogine, Mica schist, etc., Felsyte, Trachyte, etc., and the Leucite rock of Wyoming.

II. The Mica and Soda-Lime Feldspar Series: including Kersantite, Kinzigite; and the nephelitic kinds Miascyte, Ditroyte, Phonolyte, etc. (These nephelitic kinds belong almost as well in the preceding series.

III. The Hornblende and Potash Feldspar Series: including syenyte (with Quartz-syenyte), Syenyte gneiss, Hornblende schist, Amphibolyte, Unakyte (this last containing epidote in place of hornblende); and the nephelitic species Zircon-Syenyte, Foyayte.

IV. The Hornblende and Soda-Lime Feldspar Series: including Dioryte (with Propylyte), Andesyte, Labradioryte (or Labrador-dioryte), etc., and the saussurite rock, Euphotide.

V. The Pyroxene and Potash Feldspar Series: including Amphigenyte.

VI. The Pyroxene and Soda-Lime Feldspar Series: including Augite-Andesyte, Noryte (Hypersthenyte and Gabbro in part), Hypersthenyte (containing true hypersthene), Doleryte (comprising Basalt and Diabase), Nephelinyte, etc.

VII. Pyroxene, Garnet, Epidote and Chrysolite Rocks, containing little or no Feldspar: including Pyroxenyte, Lherzolyte, Garnetyte (Garnet rock), Eclogyte, Epidosyte, Chrysolyte or Dunyte (Chrysolite rock), etc.

VIII. Hydrous Magnesian and Aluminous Rocks, containing little or no Feldspar: including Chlorite schist, Talcose schist, Serpentine, Ophiolyte, Pyrophyllite schist, etc.

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## REMARKS ON CANADIAN STRATIGRAPHY.

By Thomas Macfarlane, Esq.

Mr. Selwyn's recent paper "On the Stratigraphy of the "Quebec Group and the older crystalline rocks of Canada" marks an important event in the history of the Geological Survey. To those who, like myself, have not heretofore accepted unhesitatingly the theories of the Survey authorities, the publication of this paper is of great interest. At the same time, many will, I think, regret that it is unaccompanied by any geological map or sections of the territory whose stratigraphy is discussed. Without this it is quite impossible for the general public, and quite difficult for the student of Canadian geology, to follow Mr. Selwyn, to obtain a clear idea of the reasons which have caused him to differ so profoundly from his predecessor Sir W. E. Logan, or to form a judgment as to the relative merits of their respective conclusions. Mr. Selwyn indeed informs us that "a considerable amount of careful inves- "tigation and laborious work in the field is yet required before "the indicated divisions can be correctly delineated on the map," but, although this may be an excellent reason for not as yet publishing any map illustrative of Mr. Selwyn's views, still it cannot be regarded as affecting the map of south-eastern Quebec by Sir W. E. Logan, so long promised by him, and upon which he laboured so earnestly. Indeed, I trust that the members of the Natural History Society and the public generally will join with me in urging upon Mr. Selwyn the advisability of publishing this map, for I think that we have all been under the impression that the views of the former Director of the Survey derived their strongest support from stratigraphical considerations.

My object in making these remarks on Canadian Stratigraphy is to eliminate, as far as possible, from Mr. Selwyn's paper, the facts upon which he bases his conclusions, and to examine how far the latter are new, or acceptable. Mr. Selwyn in referring to the opinions of those who have gone before him in the study of Quebec rocks, asserts that "most of these opinions have been ad-"vanced on palæontological, mineralogical or theoretical grounds, "without any study of the actual stratigraphy of the field." Indeed, he has expressed himself to the effect that his views are the result of a careful examination and mapping of the stratigraphy, while those of myself and others are the results of either mineralogical or palæontological comparisons, the former of which especially he supposes to be very misleading. From theses utterances, and from the very excellent opportunities which we know Mr. Selwyn possesses for making observations in the field, we are entitled to expect to find in his memoir a careful description of the new facts and data which have influenced his opinions, and these I shall endeavour to point out. We must, however, distinguish betwixt these and Mr. Selwyn's general geological descriptions, and also try to ascertain whether they involve negligence or inaccuracy on the part of previous observers.

I. Among these newly observed phenomena is that having reference to the Champlain and St. Lawrence fault. "The line of "this dislocation," says Mr. Selwyn, "or unconformity—which-"ever it may be—has been supposed to pass in rear of the Quebec "citadel. This I hold to be a mistake, and I think it can be dis-"tinctly shewn that it passes from the southwest end of the Island "of Orleans, under the river, and between Point Lévis and Quebec." To an ordinary observer the rocks underneath the city and citadel of Quebec bear a much greater resemblance to the contorted strata of Point Lévis than to the even-bedded shales and limestones which generally occur on the northwest side of the fault. But, after all, even if the fact be as Mr. Selwyn states, he will probably admit that this is not of the slightest importance so far as regards the correctness of his theoretical views.

II. Mr. Selwyn places on record the results of an actual examination of certain supposed Potsdam rocks, described in the Survey Report for 1866-9, and has not observed anything in their architecture or fossils to justify their separation from the Lévis formation. This is quite an important fact, of which I shall take notice when discussing the theory which Mr. Selwyn builds upon it.

III. Mr. Selwyn states that, on the River Etchemin, the rocks of his second division crop out apparently unconformably from beneath the fossiliferous belt or Lévis formation. But he is uncertain whether this "apparent unconformity" may not be a fault, and therefore it would seem hazardous to base much theorizing upon it. I cannot detect, elsewhere in Mr. Selwyn's paper, any unequivocal example of discordance such as would prove that the Lévis formation is quite distinct from the underlying "Volcanic Group."

IV. Mr. Selwyn notes the occurrence in his second division of "altered volcanic products," both intrusive and inter-stratified, and speaks of a great development of those *Volcanic* rocks. The term "volcanic" is very seldom used by modern lithologists as indictaing a particular texture or composition in a rock. Among older authors, Sartorius von Waltershausen writes of the volcanic rocks of Sicily and Iceland, all of which occur in the neighbourhood of active volcanoes. Von Richthofen, in his Natural System of volcanic rocks, written in 1868, refers exclusively to tertiary and post-tertiary eruptive rocks ranging from rhyolite to basalt. Mr. Selwyn in applying the term to intrusive rocks of Cambrian or Silurian age probably uses it in the sense of eruptive, for it would be very difficult to shew any connection between them and volcanic vents. In this case he does not put on record a new fact, but merely an old opinion expressed by previous observers. But Mr. Selwyn claims further in reference to these rocks "that "neither their true stratigraphical position nor their geological "characters have been correctly defined, and they have, regardless "of these, been confounded and incorporated with the true Sillery "sandstones, which are only a local development of thick sand- "stones at several horizons in the Quebec group or fossiliferous "Lévis formation." The geological characters mentioned have probably reference to their lithological features, and we are left to infer that certain eruptive crystalline or sub-crystalline rocks have been described as sandstones by Mr. Selwyn's predecessors, and that he has been the first to determine them correctly. But when Sir William and his assistants classed a certain diorite, for instance, in the Sillery formation they did not therefore determine it as a sandstone. When I speak of the Primitive Gneiss formation I do not necessarily mean that every rock in it is a true gneiss. And, similarly, if previous observers have placed certain "volcanic" rocks in the same formation with the Sillery sandstones, we may be certain that they did so intelligently, and that Sir William Logan and his staff were fully aware of the differences between a crystalline and a fragmentary rock.

V. Mr. Selwyn *calls attention to two characters not pointed* out by Sir W. E. Logan which distinguish the "Volcanic" from the Lévis area on the Rivière du Sud. One of these is the occurrence of fossils in the district north of the river; but this does not seem to be a new discovery. The other distinction is a peculiar schistose structure in the sandstones of the "Volcanic" group, which is not to be observed among those of the Lévis formation. It is worthy of note that here we have Mr. Selwyn himself making use of a lithological peculiarity for separating two different groups of rocks. The absence of fossils from his second or "Volcanic" division is emphasised by Mr. Selwyn; and no doubt this difference, as compared with the Lévis formation, is a most important one. Still we know that Sir W. E. Logan was aware of this distinction; so that here again we have, not the announcement of a new fact by Mr. Selwyn, but simply a new explanation of a certain peculiarity. Sir William accounted for the absence of fossils by metamorphic action; Mr. Selwyn would probably attribute it to volcanic interference: the difference is, after all, only in theory.

Although I have searched very carefully, I have failed to find in Mr. Selwyn's paper any other traces of original observation than those I have enumerated. The first of these items has no bearing upon the mutual relations of Mr. Selwyn's second division and the Lévis formation; the fourth cannot be said to be a new observation at all, and thus we have, as the actual basis of fact for Mr. Selwyn's new conclusions, the absence of Potsdam strata from the neighbourhood of the Lévis formation, the supposed unconformity on the River Etchemin and a trifling lithological peculiarity among the sandstones of the Rivière du Sud. The supposed unconformity is by far the most important part of this basis; but we must recollect that Mr. Selwyn is far from being positive about it, and, further, that the same difficulty occurred to him as regards the contact of the rocks on the northwest edge of the fossiliferous belt. There too, he does not distinguish between an unconformity and a fault, and I believe were this latter point decided it would go far to settling this vexed question of the age of the Quebec group.

After this examination I think it can reasonably be submitted that these new data are altogether insufficient to destroy the confidence which many have heretofore placed in the conclusions of Sir W. E. Logan and in the labours of those who worked under him during the last thirty years of his life. If laborious and painstaking "study of the actual stratigraphy in the field" is to count for anything, it is no discredit to Mr. Selwyn to say that his work in this respect is far outweighed by that performed by Sir William. Further, we all know that the closing years of his life, even after his official connection with the Survey ceased, were devoted to a re-examination of the Eastern Townships rocks and to the completion of his map. Surely all this ought not to be thrown aside as useless work. Surely Sir William, had he *lived, would have had something to say in these days in defence* of his opinions. Although he is gone from us, it is surely our duty to take care that justice is done him, and I contend that it would be only an act of simple justice to his memory to give to the world the results of his labours, just in the shape which they attained at his death. Apart altogether from his theoretical conclusions, the correctness of which Mr. Selwyn disputes, the observations of Sir William and his assistants, as to the actual phenomena exhibited by the rocks of south-eastern Quebec, have a practical value to the country, and to all future observers, which I conceive it to be the duty of the Survey to put on record.

When we consider the very slender foundation of new material upon which Mr. Selwyn's views regarding the Quebec group are built, it would seem that the conclusions he has arrived at are, to a very large extent, theoretical, and therefore just as little entitled to immediate acceptance as those of others who have written upon the subject. In reviewing Mr. Selwyn's conclusions, I shall attempt to state them as briefly and honestly as possible, and I shall first refer to those which from my own point of view appear to be well founded.

1. The principal feature of Mr. Selwyn's essay is of course the new view he takes as to the stratigraphy of the Quebec group. The order, in age, of its different members he maintains is just the reverse of that indicated by Sir W. E. Logan; the fossiliferous belt or Lévis formation is newer than the more crystalline rocks to the south-east, and the latter are probably of Cambrian age. Now although I cannot see that Mr. Selwyn has brought forward any new and adequate proof of the correct

ness of this view, still I feel bound to advocate it, because of my experience among similar rocks in Scandinavia and Germany, and stratigraphical and lithological comparisons which I have made between these and Canadian rocks. Indeed in the first paper which I had the honour of presenting to the Natural History Society of Montreal, dated 8th April, 1862, in speaking of the so-called metamorphic rocks of the Eastern Townships, I maintained that "so soon as the true limits and effects "of metamorphism are recognized, it will probably be acknow-"ledged that, whatever view may be entertained as to their "origin, the schistose rocks above referred to underlie the Silu-"rian and all unaltered or metamorphosed strata." Further, in a pamphlet published by me in 1871, entitled "Observations on Canadian Geology," I made the following remarks: "Indeed "in the attempts which have been made at determining the age "of the Eastern Townships rocks it has always been the rule to "begin with the Potsdam sandstone as the oldest rock, and to "assume that those to the eastward (regardless of their litholo-"gical characters) follow each other in ascending order. Any "one who has studied the structure of similar regions in Europe, "such as those above mentioned, can scarcely fail to come to the "conclusion that the opposite of this assumption is the truth; "that the oldest rocks are those of New England, and that as we "come north-westward, we pass over more and more recent "strata." (p. 13.) In mentioning the Silurian rocks in the same pamphlet, I made a still more distinct statement of my view of the matter, which I give here in full. "We have seen "that in comparing the great mass of the New England and "Eastern Townships rocks with strata of similar lithological "characters in Europe, such as those of Saxony above alluded "to, there is no difficulty in recognising them as Azoic and pre-"Silurian. This applies to the gneiss, mica schist, chlorite "schist, and to much of the clay slate of the region referred to. "As in Saxony, there exists a passage (perhaps only apparent) "from these crystalline and semi-crystalline rocks into others of "a distinctly detrital and fossiliferous character, so in the Eastern "Townships we have a similar passage from roofing slate into "softer grey slates, grauwacke (Sillery sandstone), graptolitic "shales and fossiliferous limestones. This peculiar structure "was indeed the reason why these oldest fossiliferous strata were "formerly called the Transition (*Uebergangs*) formation. The "same series of rocks in the Province of Quebec occupies a belt "along the west side of the Quebec group, having a breadth of "about twenty miles, and including all undoubted sedimentary "and fossiliferous strata. It is the same band of rocks which "continuing southward into Vermont has there been called the "Taconic, and which Dr. Hunt wishes to classify as Upper Cam-"brian. We have already seen that the term Cambrian is much "more applicable to the Green Mountain series, and there would "appear to be no good reason for ceasing to regard these rocks "as belonging to the Silurian system. As has already been ex-"plained, however, it would be proper to exclude from that "series any non-fossiliferous rocks whose aspect is semi-crystal-"line, and which have been so frequently classed as metamorphic "Lower Silurian. These, as we have seen, it is much more "reasonable to class with the Cambrian rocks." (pp. 15 and 16.) From these quotations it will be perfectly evident that Mr. Selwyn's views as to the age and structure of the Quebec group are the same as those I have held for the last seventeen years and repeatedly brought before the public. It may seem a matter of little consequence as to where the merit of priority lies, but I confess I think differently, and maintain that Mr. Selwyn's recent paper ought to have contained some allusion to the passages above quoted.

But, in spite of all this, I feel bound to say that the matter is not ended here; that the independent student of our geology will neither accept Mr. Selwyn's views nor any others, unless they satisfactorily dispose of the difficulties which have all along beset this subject. Mr. Selwyn banishes Potsdam strata from the proximity of the Lévis rocks, and claims that his new divisions have "at least the advantage of simplicity." This may readily be admitted for what it is worth, but they do not in the slightest degree meet the question with which Sir W. E. Logan found himself face to face during the latter part of his lifetime, and which may thus be stated: How can this Lévis formation be really Lower Silurian in age when it underlies, unconformably, the lowest of Lower Silurian rocks, namely, the typical Potsdam sandstone of the St. Lawrence valley? Mr. Selwyn says, that the Lévis formation is Lower Silurian, and the horizontal Potsdam sandstone is Lower Silurian too, and thinks that he has effectually disposed of the question "without invoking any of "the numerous almost impossibilities in physical and dynamical "geology which are required to explain the previous theory of "the structure." But we must not imagine that such a simple explanation could not possibly have occurred to Sir W. E. Logan, and that his introduction of those "almost impossibilities" was unnecessary. I am inclined to think that the phenomena which Sir William worked so indefatigably and so loyally to explain, remain to this day as tangible as ever, and that Mr. Selwyn's new theories afford no solution of the problem.

2. Mr. Selwyn maintains the igneous origin of many of the crystalline rocks of his second division, and especially of the "diorites, dolerites and amygdaloids" which occur in connection with certain copper ores. This is another view I have often maintained, and I might readily quote passages from my papers giving the authority of Naumann and others in support of it.

3. Mr. Selwyn particularly insists upon this point, "that the "fact of crystalline rocks (greenstones, diorites, dolerites, felsites, "norites, &c.) appearing as stratified masses and passing into "schistose rocks, is no proof of their not being of eruptive or "volcanic origin." This is a principle of very wide application, and cannot in my opinion be controverted. In my paper on the Acton mine, dated 28th October, 1862, I described a striking instance in support of this very point. I said, "Between the "cupriferous limestone and the underlying shale, there is often "intruded a fine grained greenstone, which sometimes forms "very considerable and irregular masses; sometimes intersects "the limestone strata, and often presents a peculiar banded "structure, resembling more that produced by igneous flow than "that due to deposition from water." Further, when discussing the origin of eruptive and primary rocks in January, 1864, I insisted upon the view now brought forward by Mr. Selwyn, and gave an explanation of it in the following words: "The instances "of a similar modification of structure among the greenstones are "very numerous, and they are even more important as shewing "more clearly the cause of this structure among igneous rocks. "The diorites usually occur in the form of veins, irregular masses "and layers. The veins sometimes exhibit the following re- "markable phenomena: In the middle they consist of granular "diorite, and at the sides of slaty diorite or hornblende schist, a "gradual transition being generally observable from the granular "to the schistose rock. The cause of these phenomena may "most reasonably be sought for in the circumstances attending

" the cooling of the rock, and they are most likely the same as " those which occasioned a similar structure among the porphy- " ries. The fluid rock of the diorite vein was probably in motion " in the centre, while the parts adjoining the side walls were " solidified. The current in the centre would have a distending " and arranging action at the junction of the fluid with the soli- " dified parts, and an elongation and parallel grouping of the " minerals there being formed would be the consequence. Not " only has this slaty texture been observed in connection with " veins, but it has also been remarked that the more irregular " masses of diorite assume a slaty structure towards their junc- " tion with the older rocks, the stratification being as in the case " of the veins parallel with the line of such junction."

I have thus brought into prominence three of Mr. Selwyn's conclusions with which I feel bound to agree, but I have yet to notice those of whose correctness I have very grave doubts.

1. In discussing the distribution of copper ores in the Quebec group, Mr. Selwyn asserts that the copper ores of his third division occur both in beds and lenticular layers parallel with the stratification, " but in no case accompanied by intrusive crys- " talline rocks." This position cannot be maintained with regard to the mines of Capelton. In the Capel mine intrusive dykes are met with, and in the Hartford there is one about twenty feet thick, almost vertical, with separation joints exactly resembling, on its sides, the mortar seams in a stone wall. This dyke appeared to influence the copper deposit quite favorably. It was of a basaltic nature, but intrusions of diabase are also to be found at this mine both underground and on the surface. Mr. Selwyn's reference to the Acton mine is equally unfortunate. The " diorite " there occurring is not itself cupriferous, and as for the limestone which carries the ore, although I had opportunities for observing it daily, it never occurred to me to regard the whole mass as " vein-like," nor did it seem to behave otherwise than as a bed " belonging to the stratification."

2. Mr. Selwyn is unwilling to assign " either an age or an " origin to the cupriferous diorites, dolerites and amygdaloids of " the Eastern townships different from that of the almost iden- " tical rocks of Lake Superior." Leaving age and origin aside, I shall mention a few particulars in which the two groups are scarcely " identical." In Quebec the eruptive rocks are mostly fine grained, frequently schistose, never sufficiently cupriferous to

furnish a paying mine; their small percentage of copper is combined with sulphur; amygdaloids are comparatively rare, and seldom contain anything else than calespar; these beds are intruded amongst or interstratified with slates, shales and limestones; contorted strata are often observable, and a belt of fossiliferous rocks adjoins them to the north west. On Lake Superior the supposed identical rocks are distinctly granular, seldom schistose, frequently support remunerative mines on their native copper; amygdaloids are abundant, and filled with native copper, calcspar, quartz, zeolitic and other minerals in profusion; they have the form mostly of overflows, not intrusions, and they are associated with sandstones and the coarsest of conglomerates, shewing porphyritic, Laurentian and Huronian boulders; the strata are not contorted, have a regular dip in one direction, are innocent of fossils themselves, and are far distant from any formations containing them.

3. Mr. Selwyn disputes Dr. Hunt's contention that the Keweenian series overlies the Huronian unconformably, and cites U. S. authorities against this view. Preferable to these would have have been Mr. Selwyn's own testimony as regards this question, and it is to be regretted *that he has not yet devoted much time* to the Lake Superior region. When any one wanders along a sea-beach, with overhanging cliffs on one hand, and observes on the other the water-worn boulders, pebbles and sand derived from it, he feels pretty certain that the shingle is unconformable to the cliffs. So, on Lake Superior, along its eastern shore, between Sault Ste. Marie and Michipicoten, there are frequently found, betwixt the water and the Huronian or Laurentian hills, narrow strips or patches of the rocks of the Upper group, which often jut out as small islands into the lake, and doubtless extend out great distances beneath its waters. Such limited strips of these rocks are found, for instance, skirting the base of Gros Cap, along the south shore of Bachewahnung Bay and at Cape Gargantua. Among these rocks the conglomerates are full of Huronian débris, and in those of *Bachewahnung Bay* boulders may be observed of red jasper conglomerate, the characteristic rock of the typical Huronian. If this, and the position of the Mainanse series, unconnected with any Huronian background, be not sufficient, I would mention the attitude of the rocks of Michipicoten Island. Here the strata, igneous as well as sedimentary, have an average strike of N. 68° E. and a dip of 25° southeastward. The nearest Huro

nian rocks on the north *shore run nearly east and west, dipping* 34° to 55° northward. To these, the island rocks are consequently as unconformable as are the walls of a house to its roof.

It is further to be remembered that the discordant relation of the Nipigon group to the Huronian system is admitted by Mr. Selwyn *to be an* "apparent great unconformity," and as the Nipigon group is held by Mr. Selwyn to be part of the "Upper Copper bearing rocks," this is almost conceding Dr. Hunt's position. This admission is not at all weakened by Mr. Selwyn's supposition that they are the products of an ancient volcanic crateriform vent, and that Lake Nipigon is an extinct volcano, a gigantic *Maare*, or water-filled ancient crater, like the Lake of Laach. This invention almost justifies the opinion that Mr. Selwyn is himself sometimes ready to invoke "almost impossibilities in physical and dynamical geology."

4. Writing of Dr. Hunt's Norian system, Mr. Selwyn pens the following remarkable passage: "If it is admitted—which, "in view of the usual associations of Labrador feldspar, is the "most probable supposition—that these anorthosite rocks repre- "sent the volcanic and intrusive rocks of the Laurentian period, "then also their often massive and irregular and sometimes "bedded character, and their occasionally interrupting and cut- "ting off some of the limestone bands, as described by Sir W. E. "Logan, is readily understood by any one who has studied the "*stratigraphical relations of contemporaneous and sedimentary* "strata of volcanic, palæozoic, mesozoic, tertiary and recent "periods. Chemical and microscopical investigation both seem "to point very closely to this as the true explanation of their "origin. That they are eruptive rocks is held by nearly all geo- "logists who have carefully examined their stratigraphical rela- "tions. But I am not aware of any one having suggested that "they are the products of volcanic action in the Laurentian or "perhaps lower Huronian epoch." It is unnecessary here to combat the doctrine that norites are not merely eruptive, but volcanic rocks. I must content myself with remarking that on Mr. Selwyn rests the burden of proving any new theories he may choose to bring forward, and consequently of shewing that volcanoes were active in the Azoic age.

5. Mr. Selwyn underrates the very praiseworthy efforts which Dr. Hunt has made towards bringing order out of the chaos of our primary geology, and can see no utility in the names he

employs for distinguishing certain non-fossiliferous formations. He not only condemns such "mineralogical stratigraphy," but adverts, in a manner which must grate very harshly on the feelings of many, to the "palæontological stratigraphy" of his revered predecessor. Yet on the next page Mr. Selwyn proceeds to class together groups of rocks of almost every conceivable origin, and very questionable age, as belonging to the Huronian system. I need not detail the extraordinary differences which distinguish the various members of this heterogeneous combination, both in mineralogical, lithological and stratigraphical respects. Mr. Selwyn himself points out one of these differences when he maintains that the copper ores of the Huronian and "Upper Copper bearing" rocks occur under conditions quite as distinct as those of his first and second divisions in the Quebec group. Mr. Selwyn's own recapitulation of what is to be classed as Huronian is a proof that his plan of applying stratigraphy pure and simple is not likely to be a great improvement on the methods of those who have preceded him. It may, like his views on the Quebec group, have the merit of simplicity, but we must not allow ourselves to be influenced overmuch by the advantages of this peculiarity. Instead of disparagement, such efforts as those of Dr. Hunt merit our warmest thanks, and we must wish him every success in his efforts to determine the value of mineral fossils in crystalline rocks. As he himself very fitly remarks, "In no other way did William Smith prove, in Great "Britain, the value of organic fossils, and thus lay the foundations "of palæontological geology."

(Read before the Natural History Society of Montreal, 28th April, 1879.)

# A CANADIAN PTERYGOTUS.

(*Pterygotus Canadensis.*)

Among some specimens kindly presented to the museum of the University in the winter of 1877–8, by Lieut.-Col. Grant of Hamilton, was a slab of Niagara limestone holding a well-preserved ectognath or mandible of a large *Pterygotus*. (Fig. 1.)

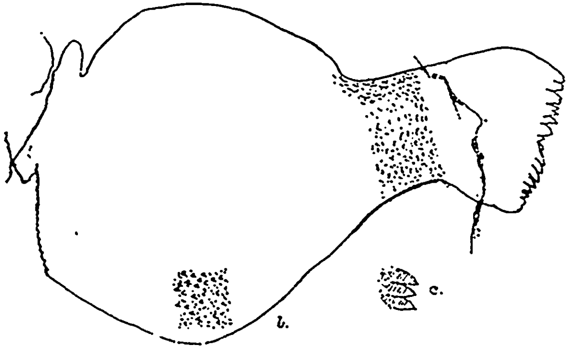


Fig. 1. Ectognath of *Pterygotus Canadensis*, natural size. (The shaded portions represent the slaty character of the surface.) c. teeth enlarged.

As the species seemed to be new, and I could not learn that anything similar had been found in rocks of this age in Canada, a notice of it was communicated to the Natural History Society of Montreal at its meeting of April 28, 1879, and was reported as follows in the *Daily Witness* of the 30th:

"A very remarkable discovery recently made in the Niagara limestone is that of some fragments of a gigantic Crustacean of the genus *Pterygotus*, comparable in size with the great *Pterygotus Anglicus* of the Devonian of Scotland, though of much greater geological age. Some small species of *Pterygotus* have been described by Hall from the Waterlime formation of New York, and a fragment of an undescribed species has been found by the same palæontologist in the Clinton; but the present is, so far as known, the first example of a large and well-developed species of this genus from so old a formation. Col. Grant hopes to obtain additional remains. In the meantime the well-preserved maxilliped or ectognath before us, with rounded scaly basal part and narrow maxillary process with about 12 denticles and 3½ inches in length, is sufficient to indicate the existence of a new and large species, which may for the present be named *P. Canadensis*, and which was a Canadian predecessor of *P. Anglicus*."

Since the above was written the removal of a part of the stone has exposed a little more of the mandibular edge, so as to show a few more teeth, making about fifteen in all, which are of the form represented in Fig. 1, *c*, except the posterior one, which is broad and slightly notched in front.

Lieut.-Col. Grant has more recently obtained another fragment of an animal apparently of the same species. It seems to be a segment of the thorax, somewhat crushed at the ends, but showing the characteristic markings very distinctly at the anterior edge. The portion preserved is barely six inches in length, and an inch and a half wide. Fig. 2 represents the middle portion of it.

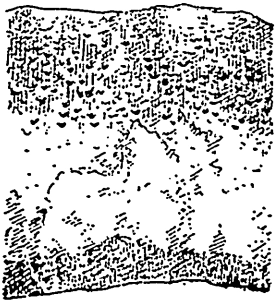

Fig. 2. Body ring of the same, central portion, natural size.

The first-mentioned specimen was found by its discoverer in the corporation quarry at Hamilton, in the lower cherty beds of the Niagara limestone, which at this place contains also a large *Conularia* and species of Graptolites. The second specimen was found at some distance from the first, but apparently in beds of the same age. The specimens are sufficient to indicate the existence of a very large crustacean of this genus, apparently the first found in the Niagara limestone formation of Canada.

According to the Rev. Mr. Symonds, in Woodward's Monograph of the British Merostomata,* the oldest known *Pterygotus* in

* Publications of Palæontographical Society, Vol. XXV.

Britain, is represented by a jaw foot found in the Upper Llandovery sandstone, which is somewhat older than the Niagara limestone. It has been named *P. problematicus*, and fragments referred to the same species have been found in the Wenlock limestone, the English representative of the Niagara.

This note is published merely to secure to the discoverer, who has laboured with much diligence and success in the palæontology of the Niagara formation, the credit which belongs to him, and to direct attention to this interesting fossil, which it is hoped may some day be represented in our collection by perfect specimens.

J. W. Dawson.

March 20th, 1879.

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# MÖBIUS ON EOZOON CANADENSE.*

By J. W. Dawson, LL.D., F.R.S.

*Eozoon Canadense* has, since the first announcement of its discovery by Logan in 1859, attracted much attention, and has been very thoroughly investigated and discussed, and at present its organic character is generally admitted. Still its claims are ever and anon disputed, and as fast as one opponent is disposed of, another appears. This is in great part due to the fact that so few scientific men are in a position fully to appreciate the evidence respecting it. Geologists and mineralogists look upon it with suspicion, partly on account of the great age and crystalline structure of the rocks in which it occurs, partly because it is associated with the protean and disputed mineral Serpentine, which some regard as eruptive, some as metamorphic, some as pseudomorphic, while few have had enough experience to enable them to understand the difference between those serpentines which occur in limestones, and in such relations as to prove their contemporaneous deposition, and those which may have resulted from the hydration of olivine or similar changes. Only a few also have learned that *Eozoon* is only sometimes associated with

* Der Bau des Eozoon Canadense, von Karl Mobius, Professor der Zoologie in Kiel. Palæontographica, Band xxv.

serpentine, but that it occurs also mineralized with loganite, pyroxene, dolomite, or even earthy limestone, though the serpentinous specimens have attracted the most attention, owing to their beauty and abundance in certain localities. The biologists on the other hand, even those who are somewhat familiar with foraminiferal organisms, are little acquainted with the appearance of these when mineralized with silicates, traversed with minute mineral veins, faulted, crushed and partly defaced, as is the case with most specimens of *Eozoon*. Nor are they willing to admit the possibility that these ancient organisms may have presented a much more generalized and less definite structure than their modern successors. Worse, perhaps, than all these, is the circumstances that dealers and injudicious amateurs have intervened, and have circulated specimens of *Eozoon*, in which the structure is too imperfectly preserved to admit of its recognition, or even mere fragments of serpentinous limestone, without any structure whatever. I have seen in the collections of dealers and even in public museums, specimens labelled "*Eozoon Canadense*" which have as little claim to that designation as a chip of limestone has to be called a coral or a crinoid.

The memoir of Professor Möbius affords illustrations of some of these difficulties in the study of *Eozoon*. Professor Mobius is a zoologist, a good microscopist, fairly acquainted with modern foraminifera, and a conscientious observer; but he has had no means of knowing the geological relations and mode of occurrence of *Eozoon*, and he has had access merely to a limited number of specimens mineralized with serpentine. These he has elaborately studied, and has made careful drawings of portions of their structures, and has described these with some degree of accuracy; and his memoir has been profusely illustrated with figures on a large scale. This, and the fact of the memoir appearing where it does, convey the impression of an exhaustive study of the object, and since the conclusion is adverse to the organic character of *Eozoon*, this paper may be expected, in the opinion of many not fully acquainted with the evidence, to be regarded as a final decision against its animal nature. Yet, however commendable the researches of Möbius may be, when viewed as the studies of a naturalist desirous of satisfying himself on the evidence of the material he may have at command, they furnish only another illustration of partial and imperfect investigation, quite unreliable as a verdict on the questions in hand. The following considerations will serve to indicate the weak points of the memoir.

1. A number of errors and omissions arise from want of study of the fossil *in situ*, and from want of acquaintance with its various states of preservation. Trivial errors of this kind are his referring to my photograph in Plate III, of the "Dawn of Life," as if it were natural size, and his stating that the larger specimens have fifty laminæ, whereas they often have more than an hundred. More important is his failing to appreciate aright the occurrence of *Eozoon* in certain layers of regularly bedded limestones, the rounded or club-shaped forms of the more perfect specimens, the manner in which the layers become confluent at the edges of the forms, as described by Sir W. E. Logan and myself, or the amount of crushing and fracture which most of the specimens exhibit. Thus he fails to convey any adequate idea of the Stromatoporoid forms and mode of occurrence of the organism, or indeed of its general character and probable mode of growth. Farther he treats it from the first as a mere laminated aggregate of calcite and serpentine, without reference to its occurrence in any other state, and also without reference to the fragmental limestones in part made up of its remains. He objects strongly to the want of definiteness of form and distribution in the chambers and connecting passages, without making allowance for defects of preservation, or mentioning the similar want of defined form in some *Stromatoporæ*. He admits, however, that the modern *Carpenteria* and its allies are in some respects equally indefinite. He farther objects to the impossibility of detecting regular primary chambers like those in modern foraminifera, but seems not to be aware that, as I have recently shown, some *Stromatoporæ* originate in a vesicular, irregular mass of cells, and that in *Loftusia*, both the Eocene *L. Persica*, and the Carboniferous *L. Columbiana*, the primary chamber is represented by a merely cancellated nucleus.*

2. With reference to the finely tubulated proper wall of Eozoon, he has fallen into an error scarcely excusable in an observer of his experience, except on the plea of insufficient access to specimens. He confounds the proper wall with the chrysotile veins traversing many of the specimens, and obviously more recent than the bodies whose fissures they fill. That he does so

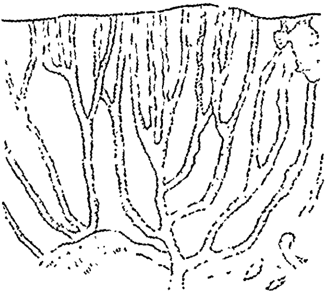
* See Journal of London Geol. Soc., January, 1878.

is apparent from his stating that the proper-wall structure sometimes crosses the bands of serpentine and calcite, and also that it presents a series of parallel four sided prisms, whereas, when at all perfectly preserved, it shows a series of cylindical threads penetrating a calcite wall. That some of his specimens have contained the proper wall fairly preserved is obvious from his own figures, in which it is possible to recognize both this structure and chrysotile veins, though confounded by him under the same designation. He objects, somewhat naïvely, that many of the *chambers fail to exhibit this nummuline wall, and that it sometimes* presents a ragged appearance or is altogether opaque. In point of fact it can appear distinctly, either in decalcified specimens or in slices, only when the minute tubes are filled with some substance optically distinguishable from calcite, or not acted on by dilute acid. When the proper wall is merely calcareous (and I have specimens showing that it is often in this state, and without any serpentine in its pores), its structure is ordinarily invisible, and it is the same when the calcareous skeleton has from any cause lost its transparency or has been replaced by some other mineral substance. Even in thickish slices, the tubes, though filled with serpentine, may be so piled on one another as to be indistinct. All this may be seen in Tertiary *Nummulites.* When wholly calcareous their tubulation is often quite invisible, and when imperfectly injected with glauconite or other silicates, they often present a very irregular appearance. If Professor Möbius will study the Nummulites injected with glauconite from Kempten,* Bavaria, in addition to the casts of *Polystomella* from the Ægean to which he refers, he will be better able to appreciate these points. It may be worth repeating here that, in examining the original specimens of Eozoon, I did not recognize the proper wall. I did not doubt that it must have existed in some form, since I could easily detect the canals in the supplemental skeleton; but I did not wonder at its non-appearance, knowing the chances against its preservation in a recognizable form. Its discovery was due to the subsequent investigations of Dr. Carpenter.†

* I am indebted to Dr. Otto Hahn for specimens of these most interesting fossils.

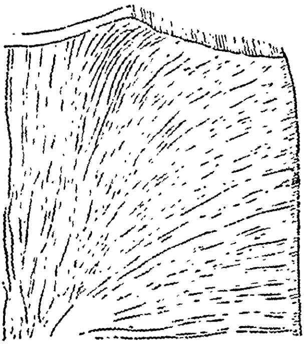
† It may deserve mention here that the Carboniferous *Fusulina* very rarely shows its tubulated wall, and that Dr. Carpenter had maintained its Nummuline affinites before he obtained specimens showing this particular structure. Structures so delicate as these are indeed only preserved exceptionally in fossil specimens.

1.



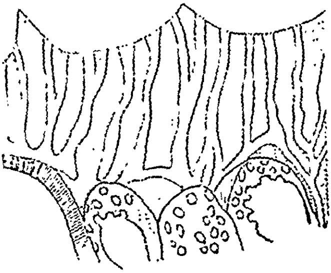
Canals of *Eozoon* (after Mobius).

2.



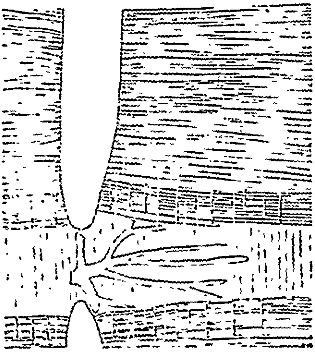
Finer canals of *Eozoon* (after Mobius).

3.



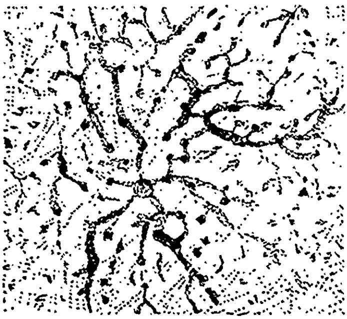
Canals of modern *Calcarina* (after Mobius).

4.



Canals and Tubule of Tertiary *Nummulina* (after Mobius).

5.



Canals of *Cœnostroma*—Upper Silurian—(original).

3. To the canal system, Professor Möbius does more justice, and admits its great resemblance to the forms of this structure in modern *Foraminifera*. This indeed appears from his own figures, as will be seen from the fac-simile tracings reproduced here, figs. 1, 2, 3 and 4, which well show how wonderfully this structure has been preserved, and how nearly it resembles the similar parts of modern *Foraminifera*. He thinks, however, that these round and regularly branching forms are rather exceptional, which is a mistake, though it is true that the sections of the larger canals are often somewhat flattened, and that they become flat where they branch. They are also sometimes altered by a vicinity of veinlets or fractures, or by minute mineral segregations in the surrounding calcite, accidents to which all similar structures in fossils are liable. Another objection, not original with him, is derived from their unequal dimensions. It is true that they are very unequal in size, but there is some definiteness about this. They are larger in the thicker and earlier formed layers, smaller or even wanting in the thinner and more superficial. In some slices the thicker trunks only are preserved, the slender branches having been filled with dolomite or calcite. It is difficult, also, to obtain, in any slice or any surface, the whole of a group of canals.* Farther, as I have shown, the thick canals sometimes give off groups of very minute tubes from their sides, so that the coarser and finer canals appear intermixed. These appearances are by no means at variance with what we know in other organic structures. Another objection is taken to the direction of the canals, as not being transverse to the laminæ but oblique. This, however, may be dismissed, since Möbius has of course to admit that it is not unusual in modern *Foraminifera*. It may be added that some of the appearances which puzzled Möbius, and which are represented in his figures, evidently arise from fractures displacing parts of groups of canals, and from the apparently sudden truncation of these at points where the serpentine filling gives place to calcite. It would also have been well if he had studied the canal systems of those *Stromatoporæ* which have a secondary or supplemental skeleton, as *Cœnostroma* and *Caunopora*. In illustration of this I give in fig. 5 a group of these canals from a recent paper of my own.†

* I have succeeded best in this by etching the surface of broken specimens.

† Journal of London Geological Society, January, 1878.

4. A fatal defect in the mode of treatment pursued by Möbius is that he regards each of the structures separately, and does not sufficiently consider their cumulative force when taken together. In this aspect, the case of *Eozoon* may be presented thus: (1.) It occurs in certain layers of widely distributed limestones, evidently of aqueous origin, and on other grounds presumably organic. (2.) Its general form, lamination and chambers, resemble those of the Silurian *Stromatopora* and its allies, and of such modern sessile foraminifera as *Carpenteria* and *Polytrema*. (3.) It shows under the microscope a tubulated proper wall similar to that of the Nummulites, though of even finer texture. (4.) It shows also in the thicker layers a secondary or supplemental skeleton with canals. (5.) These forms appear more or less perfectly in specimens mineralized with very different substances. (6.) The structures of *Eozoon* are of such generalized character as might be expected in a very early Protozoan. (7.) It has been found in various parts of the world under very similar forms, and in beds approximately of the same geological horizon. (8.) It may be added, though perhaps not as an argument, that the discovery of *Eozoon* affords a rational mode of explaining the immense development of limestones in the Laurentian age; and on the other hand that the various attempts which have been made to account for the structures of *Eozoon* on other hypotheses than that of organic origin have not been satisfactory to chemists or mineralogists, as Dr. Hunt has very well shown.

Professor Möbius, in summing up the evidence, hints that Dr. Carpenter and myself have leaned to subjective treatment of *Eozoon*, representing its structure in a somewhat idealized manner. In answer to this it is necessary only to say that we have given photographs, nature-prints, and camera tracings of specimens actually in our possession. We have not thought it desirable to figure the most imperfect or badly preserved specimens, though we have taken pains to explain the nature and causes of such defects. Of course, when attempts at restoration have been made, these must be taken as to some extent conjectural; but so far as these have been attempted, they have consisted merely in the effort to eliminate the accidental conditions of fossilized bodies, and to present the organism in its original perfections. Such restorations are not to be taken as evidence, but only as illustrations to enable the facts to be more easily understood. It is to be observed, however, that in the study of such fossils as *Eozoon*, the observer must expect that only a small proportion of his specimens will show the structures with any approach to perfection, and that comparison of many specimens prepared in different ways may be necessary in order to understand any particular feature. A single figure or a short description may thus represent the results of days spent in the field in collecting, of careful examination and selection of the specimens, of the cutting of many slices in different directions, and of much study of these with different powers and modes of illumination. My own collection contains hundreds of preparations of *Eozoon*, each of which represents perhaps hours of labor and study, and each of which throws some light more or less important on some feature of structure. The results of labor of this kind are unfortunately very liable to be regarded as subjective rather than objective by those who arrive at conclusions in easier ways.

Taken with the above cautions and explanations, the memoir of Professor Möbius may be regarded as an interesting and useful illustration of the structures of Eozoon, though from a point of view somewhat too limited to be wholly satisfactory.—*Amer. Journal of Science and Arts, March*, 1879.

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*The following notice of the Memoir on Eozoon by Prof. Möbius, referred to in the above paper, is from the "Annals and Magazine of Natural History Society" for April*, 1879.

The author first enumerates the published memoirs on *Eoozon*, and states how he was led to look specially into the matter, having met with his *Carpenteria rhaphidodendron*, of Mauritius, which at first sight he thought would present some striking analogy to the presumed Laurentian fossil. The sources whence he obtained Eozoonal preparations and the methods of examination are also mentioned. The form and size of *Eozoon*, as recognized by Dawson and Carpenter, and their comparison of its structure with that of certain Foraminifera, are given in some detail; also the shape, size, and arrangement of the serpentinal bodies ("chamber-casts," "concretions," &c.), their connexion, and the fibrous layer ("acicular crust," "nummuline layer," &c.) between these bodies and the limestone (calcite) are treated of as figured in the accompanying plates. The little Eoozonal stalk-like bodies traversing the associated limestone (calcite), and regarded by Eoozonists as "casts of canals," are next dealt with (p. 185). The structure, as a whole, is compared with that of Foraminifera at pages 186–189. The absence of any primary or central chamber, the apparently capricious distribution of both the "tubuline layer" and the "canals," the impossibility of representing the *Eozoon* as a whole by any drawing of one natural specimen, and the consequent necessity of using diagrammatic figures to illustrate the reconstructed body, are points dwelt upon in this chapter, leading to Prof. Möbius's conclusion that he does not believe *Eozoon* to be a Foraminifer or organic at all.

At pages 189–191 the authors refers to the brief published observations on *Eozoon* emanating from the lamented Max Schultze, who stated that he could not agree in the opinion that the so-called "nummuline layer" was really of Foraminiferal origin; and expressed his intention of giving further study to the other peculiar structure, which had been referred by Dawson and Carpenter to the "canal-system," and with specimens of which his friends were supplying him.

The reason for referring the structure of the Eozoonal marble to a Rhizopodal organism have been given in detail, with illustrations, in many papers and notes by Carpenter and Dawson in this and other periodicals. The objections now again raised by our author have been already dealt with in those papers. Of the structures treated of by Prof. Möbius the branching and lobular infillings of the "canal-system" are particularly valued by Eozoonists as good evidence, on account of their peculiar arrangement, so agreable to the disposition of canals in certain Foraminiferal shells. Such appearances in *Calcarina*, &c. were figured and published without reference to and before the discovery of *Eozoon*. That ancient organisms, though belonging to the same groups as represented in nature to-day, should differ widely in details of structure, is a truism illustrated by many newly discovered fossil (and even recent) forms of life, whose structure is found to be wonderfully different from, and yet wonderfully consonant with, the make-up of the already known types of organic structures; and this invalidates our author's objection to a reliance on the possibilities of Nature. What zoologist or botanist can predicate the structural details of the next discovered plant or animal, however narrow the limits we may suppose to define its alliance to any previous known form?

Although many mineralogists regard the eozoonal rock as having been as inorganic in its origin as it now is in its material, yet Dr. Sterry Hunt, for one, who has long studied it, thinks that its peculiarities are not due to a mineral genesis alone. We know also that not only Foraminiferal shells, but other calcareous tests and skeletons, both recent and fossil, have their tubes and cavities filled by various minerals, with results very similar to what is regarded as having taken place and as being visible in *Eozoon*.

It is not that here and there, and, indeed, in very many parts of a true Eozoonal rock there are lines and patches, fibrous and concretionary, of purely mineral origin, as well as their mineral matrix,—the point to be kept in view is that the structure of certain portions is best explained by reference to mineral infiltration of tubular and cavernous shells, which grow and spread after the manner of Foraminifera, though not identical with any known form in particular. Also it has to be remembered that not only has the enclosing rock been itself subjected to mineral changes, but has been crushed, broken and twisted, and that the scarcity of large areas of perfect and undisturbed structure, in such a relatively large *Rhizopod*, has to be supplemented, in the study of its whole, by such diagrammatic constructions of what the experienced observer recognizes and wishes to explain, as our author condemns at p. 188, because, he thinks, the Eozoonists in their diagrams have overstepped the line of probability. Without such illustrations, showing (like models) both the elevation and perspective of internal arrangements, we may remark, external appearance and microscopic sections would very imperfectly elucidate the descriptions of large Foraminifera. The correlation of the mineral representatives of, at least, the "canal-tubes" and "chambers" in *Eozoon*, both of which are cut at many different angles in sections, and can rarely be seen in elevation, and then only to a small extent, are best shown by this method—especially, too, as the student has, in this case, to make a mental translation of threads into tubes, and nodules into chambers.

At page 198 Prof. Möbius consoles the Eozoonists with his opinion that the doctrine of evolution need not be despaired of because he removes the primordial *Eozoon* from the category of

Beings. We do not see the value of this commonplace and wordy little chapter, except to illustrate what (at pp. 178, 179) he warns Eozoonal and other naturalists to avoid, namely, time-wasting and immature talk, in which words take the place of ideas.

Plates xxiii. to xxxiv. inclusive contain carefully drawn figures (coloured) of preparations of the Eozoonal ophitic marble, as thin slices, as etched surfaces, and as separated particles, communicated by Drs. Carpenter and Dawson.

Plates xxxv. to xl. inclusive (excepting one figure) contain enlarged sections of the shell-structure of *Polytrema miniaceum*, *Cycloclypeus*, *Nummulina*, *Calcarina Spengleri*, *Tinoporus baculatus*, *Orbitoides papyracea*, *Polystomella*, and *Carpenteria rhaphidodendron*. All (except one) of these drawings have been made by the Author himself.

In none of the preparations of known recent and fossil Foraminifera here figured does Prof. Möbius see any thing more than a distant resemblance to Eozoonal structure, which latter, as before said, he regards as inorganic.

This memoir is a handy *résumé* of the objections made by anti-eozoonists to the presumed organic origin of the object under notice; and the plates brought together by Prof. Möbius, with no little labour and skill, are useful as a compendious set of sectional figures of *Eozoon* and many of its more modern relations; and, though he fails to see their alliance, close as the analogies may be, yet his work is highly useful and praiseworthy; it is disinterested, straightforward, and consciencíously offered for the advancement of knowledge.—*Annals Nat. Hist., April*, 1879.

# ON THE WATER SUPPLY OF MONTREAL AND ITS SUBURBS.

By J. Baker Edwards, Ph.D., D.C.L., F.C.S., Professor of Chemistry, University of Bishop's College, and Public Analyst.

(Read before the Natural History Society of Montreal, April 28th, 1879.)

In order to render this review as complete as possible, I will commence by quoting the analyses of Dr. T. Sterry Hunt, in his report to the Water Committee of this city in 1854.

The samples were collected in the months of March and April, and gave the following amount of mineral matter per imperial gallon after destruction of the organic matter by heating to redness. These results are interesting for comparison with more recent analyses:

| | R. Ottawa Ste. Anne's. | St. Lawrence Cascades. | Lachine. | Old City Water Works. |
|---|---|---|---|---|
| Mineral matter per gallon. | 3.73 | 10.76 | 8.41 | 9.62 |

Dr. Hunt, in his report, states that the amount of chlorides found in the city water taken from the old works on Commissioners street always contained an excess of chlorides over the water of the St. Lawrence, showing local sources of impurity, probably due to the drainage of the city.

The nature of the organic matter does not appear to have been very closely investigated; but it is suggested that it was of a vegetable and harmless character.

In the very valuable and elaborate reports of the Geological Survey, published in 1863, Dr. Hunt furnishes us with fuller analyses, and makes the following "comparison" of the waters of the Ottawa and the St. Lawrence:

"The comparison of the waters of the two rivers shows the following differences: the water of the Ottawa, containing but little more than one-third of the solid matter of the St. Lawrence, is impregnated with a much larger quantity of *organic matters*, and contains a large proportion of *alkalies* uncombined with *sulphuric acid* or *chlorine*."

The organic matter determined by loss after ignition was estimated as follows:

| | Ottawa R. | St. Lawrence. | Lachine. | River front of City. |
|---|---|---|---|---|
| Grains of organic matter per gallon. | 1.11 | .98 | 1.49 | 1.29 |

We have, therefore, on the best authority, the condition of the old water supply and of the river waters in question before the present works were completed.

My own analyses date from 1870; but the first series of results which I now submit were made in 1872, from samples of water which I collected myself during a trip from Niagara, in the month of June of that year.

The quantity of water at my disposal was too small to determine the organic nitrogen; but as a record of the solid contents of the waters of the St. Lawrence it may possess some interest.

| | Organic Carbon. | Mineral Salts. | Total. | Hardness, by Clark. |
|---|---|---|---|---|
| River Niagara, | 1.10 | 6.60 | 7.70 | 3.5° |
| Lake Ontario, | 1.01 | 6.50 | 7.51 | 3.3° |
| Toronto Bay, | * 2.50 | 8.50 | 11.00 | 4.5° |
| St. Lawrence, Long Sault, | 1.20 | 6.60 | 7.80 | 3.3° |
| Do Pointe Cascades, | 1.20 | 6.60 | 7.80 | 3.5° |
| Do S. Shore Aqueduct, | 1.20 | 7.60 | 8.90 | 3.5° |

* Containing nitrogen.

With the exception of the water of Toronto Bay, these waters are all clear and pellucid, and run sufficiently near in mineral contents to justify us in accepting the mean as a fair estimate of the quality of the St. Lawrence water. This gives us an average of about 1 grain per gallon of organic carbon, and 7 grains of mineral matter for St. Lawrence water above Lake St. Louis, in the month of June, 1872. In water taken from near the south shore, in May, 1873, I found:

| | |
|---|---|
| Organic Matter................. | 1.1 |
| Mineral Salts..................... | 7.8 |
| Hardness, 3.5° | 8.9 |

In December, 1879, water from the inlet of the Longueuil water works gave me:

| | |
|---|---|
| Organic Matter............... | 1.5 |
| Mineral Salts....... .......... | 10.0 |
| Hardness, 6° | 11.5 |

In May, 1879, water from the same point, as supplied from the Longueuil water works, contains:

| | |
|---|---|
| Organic Matter................ | 2.03 |
| Mineral Salts.................... | 9.72 |
| Grs. per imperial gallon, | 11.75 |

Hardness, 6.25°, Clarke.

(At this time the river was pretty full and brimming the wharves at Montreal.)

From the above it will be seen that the water gains 3 degrees in hardness on the south shore between the Lachine rapids and Longueuil, while there is no great increase in mineral lime solids. The alkaline silicates disappear in the dried residue, and saline chlorides and sulphates are increased in quantity.

These are indications, therefore, that the water at Longueuil is somewhat affected by passing the city, but not to such an extent as to render the water unwholesome, although it would be much safer if sand filtered.

On the other hand, the water at Hochelaga gives considerable indications of nitrogenized impurity, the result of animal decay, and it is doubtful whether simple filtration would render it fit for human consumption. It is evidently affected by the sewage of the city both near the shore and in mid-stream. Any attempt to utilize it for a water supply would be attended with great expense, and still involve some risk of typhoid impurities.

WATERS OF THE NORTH DISTRICT.—1872 and 1873.

In contrast to the table of the waters of the St. Lawrence and the south shore, the following analyses of the waters of the north district will be found of interest, showing that, whilst the lake waters are of remarkable purity being taken at a great elevation and above the ordinary sources of impurity, the river waters of the north district which drain from the Laurentides, all contain alkaline silicates, and are slightly coloured with organic spores giving a yellow marsh-like tinge, to the waters. These waters, when conveyed for some distance in iron pipes, become of an ochreous tinge, from the precipitation of the vegetable matter in solution, which is unpleasing to the eye and somewhat difficult of filtration. A water of similar character has been introduced into Liverpool, England, and was for some time disliked on account of its peculiar color; but it has proved a wholesome and useful water, and the color is no longer deepened by the iron pipes which convey it from Rivington, a distance of twenty-five miles.

The waters of the north district gave the following results per imperial gallon :

| | Organic Carbon. | Mineral Salts. | Total. | Hardness, by Clark. |
|---|---|---|---|---|
| Lake Kilkenny, | 1.10 | 2.15 | 3.25 | 0.5 |
| Lake Masson, | 1.05 | 2.05 | 3.10 | 0.5 |
| Rivière du Nord, | 1.80 | 2.70 | 4.50 | 1.2 |
| River Ouareau, | 2.05 | 3.95 | 6.15 | 1.1 |
| River Ottawa, | 1.90 | 2.30 | 4.20 | 1.3 |
| Do at Ste. Anne, | 1.80 | 4.40 | 6.20 | 2.5 |

The Lake waters were perfectly colourless, while the River waters were more or less tinged.

The waters which supply the city of Montreal and the municipality of St. Cunegonde are taken from the aqueduct on the north shore of the river, just below Lachine, and are the mingled waters of the St. Lawrence and of the Ottawa Rivers in varying proportions at different periods of the year.

During the winter months the waters of the St. Lawrence are higher and more uniformly fed than those of the Ottawa: being confined under the ice, they therefore displace the Ottawa water, and, pressing over the rapids at Ste. Anne, they drive the northern waters chiefly over the "Back River" to the north of the Island of Montreal. The extent of this diversion depends partly upon the grounding of the ice about the western shore of the Isle Perrot and the ice block at Lachine rapids, circumstances which differ in extent and duration at every season, and contribute to the frequent variation of the character of the water supply at Montreal. This difference is more apparent in the color, flavor and comparative clearness of the water than in the results which appear by analysis of the salts which they contain,—the chief difference being in the organic constituents and in the aëration by carbonic acid, and in the presence of alkaline silicates or their neutralization by calcium salts.

The present system of supply on the rising main exaggerates the evil of a mixture of incompatible waters by carrying into all the houses below the level of St. Catherine street the suspended matter or dirt, with its accompanying disgusting lower organism, which fill the pipes and accumulate in the closet cisterns, especially in the spring, when the ice breaks up and renders the water muddy, and again during the heavy rains of the fall.

Of this suspended matter, my friend Dr. G. P. Girdwood has published a record in the *Canada Medical Journal*, Vol. vii, page 102, showing that in three months' ordinary summer supply the average daily deposit of *insoluble mud* varies from 2 grains to 4.8 grains per gallon, while under the exceptional circumstances of disturbance, the amount rises as high as 14 grains per gallon. As inhabitants of this mud, he enumerates fifteen forms of animal life, which he found in addition to those which I had previously described in the *Canadian Illustrated News* (Dec. 7, 1870).

The following table gives the result of analyses of the Montreal water supply in recent years, and during different seasons of the year:

## Table of Analyses of Montreal Water Supply.

| Date of Collection. | | Locality. | Total Solids per Impl. Gallon. | Organic Carbon. | Mineral Salts. | Hardness by Clark. | Albuminoid Nitrogen, per million by Nessler. | Appearance. |
|---|---|---|---|---|---|---|---|---|
| May, | 1872 | Water Workshops - - | 14.01 | 4.01 | 10.00 | 2.7 | .001 | Turbid, discolored —river high. |
| Sept., | 1872 | Wheelhouse - - - | 9.25 | 2.00 | 7.25 | 2.3 | .001 | do. |
| Feby, | 1873 | Laboratory, Beaver Hall | 8.50 | 1.00 | 7.50 | 3.6 | .001 | Turbid. |
| June, | 1873 | do. | 9.10 | 2.90 | 7.10 | 3.8 | .010 | Clear. |
| Sept., | 1873 | do. | 7.25 | 1.00 | 6.25 | 3.0 | .010 | Clear. |
| Feby, | 1874 | do. | 9.10 | 1.50 | 7.60 | 3.8 | .001 | Turbid. |
| Dec., | 1874 | High Level Reservoir - | 6.50 | .50 | 6.00 | 2.0 | none. | Very clear. |
| Dec., | 1877 | Laboratory, Beaver Hall | 8.60 | 1.00 | 7.60 | 2.5 | trace. | Clear. |
| Jany, | 1878 | do. | 9.10 | 1.00 | 8.10 | 3.0 | trace. | Clear. |
| March, | 1878 | do. | 9.50 | 1.00 | 8.50 | 2.0 | .010 | Turbid. |
| July, | 1878 | do. | 8.00 | 1.50 | 6.50 | 2.5 | trace. | Clear brown. |
| Nov., | 1878 | do. | 7.50 | 1.50 | 6.00 | 2.0 | trace. | Clear. |
| Jany, | 1879 | do. | 5.95 | 2.15 | 5.80 | 0.6 | trace. | Clear. |
| April 7, | 1879 | do. | 11.20 | 4.90 | 6.30 | 1.75 | 1.00 | Turbid and very impure. |
| April 24, | 1879 | Service at I. R. Office - | 8.05 | 1.40 | 6.65 | 2.50 | .03 | Nitrogen in excess. |
| May 9, | 1879 | Hydrant at St. Cunegonde | 6.80 | 2.10 | 4.70 | 3.25 | .02 | Not clear. Nitrogen in excess. |

It will seen from this table of analyses that the Montreal water supply *is a most valuable product, and that it often contains* excess of mineral matter in suspension, and sometimes organic debris from local or temporary causes. A far more wholesome water supply would be secured from the same source by the addition of settling beds of masonry and filter beds of gravel and sand after the Liverpool model, which I am informed should *not cost more than 10 cts. per 1000 gallons, and would certainly* contribute largely to the health of the inhabitants and to the hygienic reputation of the city. Moreover, upon other economic grounds, this is a wise and prudent improvement, which has been too long denied to the well-taxed public of Montreal. This I have urged to the successive Mayors, Chairmen of Boards of Health, *and of the* Water Committees, *and I* wish *once more to* urge these considerations on the municipal and sanitary authorities.

The waters of the Ottawa and of the north district generally which flow past Montreal island are remarkable for the sandy or flinty character of their minute animal and vegetable organisms, and for *the* presence of *alkaline silicates*, which when co-mingled with the waters of the St. Lawrence become precipitated into gelatinous hydrate of silica. As the result of frequent microscopic examinations of the deposits formed by subsidence of the water supplied to my laboratory, and also the deposits separated by the process of filtration in my house filter, I find that the deposits consist of

1. *Angular fragments of sand and flint.*
2. *Gelatinous silicious magma.*
3. *Organic silicious filaments of Diatoms*, also *spicules and gemmules of fresh-water Sponges* and *skeletons of Algæ*. This deposit resembles in general character the well-known "Tripoli powder" used for the burnishing of metals, the keenness and polishing power of which, is due to the presence of similar vegetable sandy fragments, which are scarcely less hard than "Emery powder" and will cut fine scores in the brass work of taps and valves, which, followed by hard particles of sand, give rise to continual leakage.

Therefore, I submit that the filtration of the water, *before it is pumped into the mains of the city*, would, by removal of this *gritty flinty* matter accomplish a *saving of waste* alike in water, taps, valves and working machinery, which would *more than repay* the cost of filtration, and prove at the same time a great sanitary benefit. With regard to the *cost of filtration* I ascertained when last in Liverpool, that the cost of filtering 11 millions of gallons per diem, including cleansing and change of filters and interest of Capital, involved a comparatively small outlay and was maintained at a rate of £1250 sterling per annum, say $575 per annum for each million gallons per diem. The balance of the Liverpool supply is drawn from well water naturally filtered through the red sandstone rock.

Under the intermittent system the consumption in Liverpool was on the whole average 33½ gallons per head, per day; in certain districts 58 to 69 gallons per head per day. Under the constant service system this fell to 19½ gallons per head per day. Under the system of district meters and inspection this is now reduced to 12 gallons per head per day, with a constant, more uniform and ample supply. Now a consumption of 33½ gallons per head per day indicates a waste of 21 gallons per head per day and this saving is effected at a cost of one farthing per 1000 gallons, whilst an additional supply must be reckoned to cost from 5d to 6d per 1000 gallons.

I venture to think that the adoption of the Liverpool district plan in Montreal, of which filtration is the first element, would

1stly. *Double the available supply.*

2ndly. Afford also, a *spare head of water for flushing sewers and cleansing streets.*

3rdly. *Improve the sanitary condition* of the city by the supply of *filtered water* and thus guard against prevailing endemic and threatened epidemic disease, reduce the rate of infant mortality, and promote the general health and sobriety of the citizens at large.

Next to a really satisfactory supply of water to the city, the important and increasing necessities of the suburban districts demand consideration and timely relief. Either by extension of the city limits or by developments of the water supply to the suburbs, it is obvious that some better provisions for water supply ought to be made for those who very wisely forsake the crowded streets and lanes of the city and resort to the beautiful and healthful suburbs of Montreal Mountain. Why should not a head of purified water be here maintained sufficient to supply the whole island of Montreal? A liberal and enlightened municipal policy would not rest content with the present area of distribution, but would seek powers by which this water should be accessible in every direction, in which enterprize may seize upon a good locality, in which to plant real estate.

In the district of Hochelaga, the future Leith of our city, we have, as shown in an earlier portion of this paper, cut off the inhabitants from a reasonable enjoyment of our common river, by impregnating the same with sewage at the new outlet of Colborne avenue.

We also stand in great danger of permanently contaminating the water of Longueuil, and therefore the extension of the city southward; and the projection of the sewage at a more northern point much beyond the present will be an absolute necessity in the near future. For the provision of an ample supply of good water the municipality of Hochelaga have made diligent search, but no available source has been discovered nearer than the Back river. Hochelaga must therefore depend on Montreal for a water supply.

The district of St. Cunegonde at the west also requires water, and a large water supply. The farm of Prof. Macgregor at Braeside furnishes a remarkable spring, which would afford a wholesome and large supply of water from the Laurentian hills on the north.

My analysis of this spring, made in April last, gave the following result:

| | |
|---|---|
| Total solid contents per imperial gallon - | 31.30 |
| Hardness by Clark, 19° | |
| Albumenoid nitrogen - - - | no trace. |
| Carbonate of lime and magnesia - | 22.00 |
| Organic carbon - - - - | 1.75 |
| Silicious carbon - - - - | 2.30 |
| Silica wh. iron and alumina - - | .10 |
| Chlorine (combined) - - - | 1.72 |
| Sulphuric acid do. - - - | .73 |
| Alkaline bases - - - - | 2.70 |
| | 31.30 |

This is a very excellent water, but rather hard for domestic and industrial purposes. I am informed that the flow of the spring is equal to about 4000 gals. per diem.

At Cote St. Antoine, outside the city limits, the residents are supplied by water carts, which are sometimes replenished from the flushets of melting snows in neighbouring fields, sometimes from the canal direct. These carted waters are usually very impure. The water flowing from Montreal mountain is however of good quality. At the Mile End also, water carters purvey water from the quarry ponds, full of animalculæ and vegetable matter, which is unfit for domestic use.

At Mount Royal Vale surface water is collected which is well mingled with clay, and when clear this water appears to be of good quality but rather hard. An analysis of this water in April last, gave the following result:

| | |
|---|---|
| Organic carbon - - | 4.20 |
| Carbonate of lime - - | 14.40 |
| Silica and alkaline salts - | 2.40 |
| Hardness by Clark 14° | 21.00 |

At Lachine also, better waters, although somewhat harder, are obtained from local wells than from the river water. I found that in the month of March, 1878, the river water gave

| | |
|---|---|
| Organic carbon - - | 3.1 |
| Mineral salts - - - | 9.6 |
| | 12.7 grains, |

and that it also contained excess of albuminoid nitrogen.

It would therefore appear highly desirable for the hygienic welfare of our suburban residents and our summer visitors, that the Montreal water works should be considerably extended, and the filtered water distributed from our mountain reservoirs to the whole outlying districts. This *great improvement*, which I have consistently and persistently advocated for some years, I hope to live to see an *accomplished fact*.

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## PROCEEDINGS OF THE NATURAL HISTORY SOCIETY OF MONTREAL.

The fifth meeting of this Society for the present session was held on the evening of Monday, April 7th.

Principal Dawson occupied the chair.

The minutes of last ordinary meeting and also of last meeting of Council were read and approved.

Five new members were elected, and two proposed.

A fine specimen of fresh-water black-bass, *Centrarchus fasciatus*, was presented to the Museum by Mr. Alex. Fowler.

Mr. J. T. Donald then read a paper on "Elephant remains from Washington Territory."

This paper was a statement of the result of an effort to determine the species to which Elephant remains, represented by a molar forwarded to the Society, belonged. The remains were referred to *Elephas primigenius*, var. *Jacksoni*.

The chairman next addressed the meeting "On the origin and history of successive floras of America." He showed that these floras had all originated in the north and then moved southward.

The various theories as to the causes by which these polar regions had been rendered fit habitations for plants of our temperate climes were presented and discussed, after which the meeting adjourned.

The sixth regular meeting took place on Monday Evening, April 28th. The President occupied the chair. After routine business, Kenneth Campbell, Esq., presented the Society with a specimen of coca, *Erythroxylon coca*, from Mexico.

Mr. Thomas Macfarlane, of Acton, Que., read a paper entitled "Remarks on Canadian Stratigraphy." This was a reply to, and criticism of Mr. Selwyn's paper "On the Stratigraphy of the Quebec Group and the older crystalline rocks of Canada," read before the Society in February last.

Mr. Macfarlane's paper appears in the present number of the *Naturalist*.

Mr. Selwyn replied to Mr. Macfarlane, explaining some of the statements he had made in the article referred to, and maintaining the correctness of the position assumed by him in these statements.

Dr. T. Sterry Hunt also spoke in reference to Mr. Selwyn's late paper. He contended that the Norian rocks are not eruptive, and objected to Mr. Selwyn calling his "systems" of rocks such as Norian and Montalban *theoretical*, when thirty years labor had been spent upon them.

Dr. J. Baker Edwards then presented the meeting with "Notes on the Water Supply of Montreal and its Suburbs," which we publish in full.

## MISCELLANEOUS.

SOME REMARKS ON INTER-GLACIAL EPOCHS, IN REFERENCE TO FAUNA AND FLORA EXISTING AT THE PRESENT DAY IN THE NORTHERN HEMISPHERE, BETWEEN THE PARALLELS OF 81° AND 83° N., BY H. W. FEILDEN, F.G.S.—In the brief paper that I have the honour of submitting to your notice, it is my desire to draw your attention to the theory of intercalation of series of warmer climates during what is called the Glacial Epoch.

In accordance with the opinions of Professor Oswald Heer and the late Sir Charles Lyell, the existence of Inter-Glacial Periods has been indisputably evidenced by the Dürnten beds of Switzerland, and the Forest bed of our Norfolk coast; and while *Professor Heer considers that the Dürnten lignite beds represent* the existence of a climate similar to that now existing in Switzerland, Lyell remarks that the Forest bed of Cromer presents a singular analogy to that of Dürnten, and that "both of them alike demonstrate that there were oscillations of temperature in the course of that long period of cold."*

Mr. James Geikie in his valuable work "The Great Ice Age," has likewise adopted the theory of the *intercalation of warmer* climates to account for the inter-glacial beds of Scotland. In fact, so many of our greatest modern authorities have given their adhesion to this theory, that it may almost be regarded as an accepted fact amongst modern geologists. That the so-called inter-glacial beds of Scotland and England were deposited between the commencement of the Glacial Epoch and its final withdrawal from Great Britain, is a well-established fact; but the question I am desirous of presenting to your consideration is, whether the so-called inter-glacial beds represent what Lyell terms "oscillations of temperature," or merely modifications of temperature due to alteration in the levels of land-masses, and the consequent change in their character as condensers of moisture, with probably a change also in the direction of the oceanic currents.

My suggestion, that it may not be necessary to connect the so-called inter-glacial beds with sudden changes or oscillations of temperature, is based upon the results of observations in Grinnell Land during 1875-76.

* Lyell, Principles of Geology, vol. i. p. 196, eleventh edition.

Having been fortunate enough to pass twelve months in the most northern portion of the earth that civilized man has yet visited, a region subjected to as rigorous extremes of cold as any yet recorded, where the sun remains below the horizon at mid-day for five months, where the mean annual temperature is—3°·473, where a minimum of—73°·75 was registered during the month of March, and where for only three months of the year the mean temperature rises up to and above the freezing point of fresh water, viz. +32°·455 in June; +38°·356 in July; +31°·913 in August. I was impressed with the fact that this region is undergoing less glaciation than Greenland, lying twenty degrees of latitude to the southward in the parallel of Shetland, and differing remarkably from the northern part of Greenland, lying between the same parallels, and separated by a narrow water-way not twenty miles across.

In Grinnell Land, from lat. 81°·40′ N. to lat. 83°·6′ N., no glaciers descend to the sea, no ice-cap buries the land; valleys from which the snow is in a great measure thawed during July and part of August stretch inland for many miles, and the peaked mountains, snow-clad during the greater portion of the year, in July and August have great portions of their flanks which rise to an altitude of 2,000 feet bared of snow.

The opposite coast of Greenland presents a very different aspect, a *mer-de-glace* stretches over nearly its entire surface, its fiords are the outlets by which its great glaciers protrude into the sea. In Petermann Fiord the ice cap with its blue jagged edge lying flush with the face of the lofty cliffs was estimated to be forty feet thick.

When we turn to the Flora and Fauna of Grinell Land the difference is equally astonishing; some fifty or sixty flowering plants are found in its valleys, and between latitudes 82° and 83° N., I have seen tracts of land so profusely decked with the blossoms of Saxifraga oppositifolia that the purple glow of our heath clad moors was brought to my recollection.

Musk oxen in considerable numbers frequent its shores; the Arctic fox, the wolf, and ermine, with thousands of lemmings live and die there. The bones of these mammals, along with those of the ringed seal (*Phoca hispida*), are now being deposited in considerable quantities in the fluvio-marine beds now forming in the bays and at the outlets of all the streams, or rather summer torrents of Grinnell Land. With these bones will be associated those of birds, such as geese and sea-gulls. Numerous mollusca and crustacea, many species of rhizopods, with the remains of land and sea plants, will there find a resting place.

Supposing that these beds were examined at some future period under conditions, when the glacial epoch had disappeared from the sourrounding area, it would be difficult to realise that they were contemporaneous with the beds formed under the Greenland ice cape in the same parallel of latitude and on the opposite shore of a channel not twenty miles across.

In the one case, enormous thicknesses of till with ice-scratched stones have in all probability been deposited; in the other, fluvio-marine beds containing a comparatively rich assemblage of marine and land forms, with river rolled pebbles, would be brought to light.

In the face of these facts is it incredible to suppose that the inter-glacial periods of Great Britain are due not so much to "oscillations of temperature" as to alterations in the amount of moisture in the atmosphere, and the position of the land-mass regarded as a condenser?

It is evident that the glaciation of Greenland and the west shore of Baffin's Bay and Ellesmere Land is not a result altogether of degrees of heat and cold, or in other words, temperature, but equally the result of geographical position which causes these regions to act as mighty condensers, throwing down in the form of snow the heated vapour of the south, and so effectually eliminating the moisture from the air that a tract of country like Grinnell Land lying still further to the north and subjected to an equally rigorous climate, is comparatively exempt from glaciation.—*From the Scientific Proceedings of the Royal Dublin Society.*

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THE ROCKY MOUNTAIN LOCUST.—At its last session Congress appropriated $10,000 for the completion of the investigation of the Rocky Mountain locust by the United States Entomological Commission. The work during the coming season will be carried on in Colorado and the Western Territories, particularly Utah and Eastern Idaho, where the locust abounds each summer, doing more or less damage. Parties will also be sent into Montana, the main breeding place of the destructive swarms periodically visiting the Western Mississippi States.—*American Naturalist, May*, 1879.

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