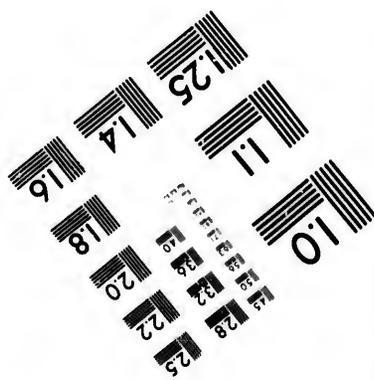
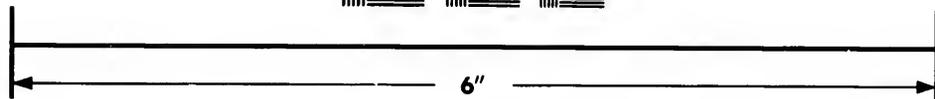
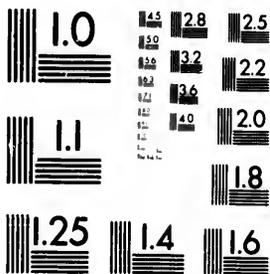


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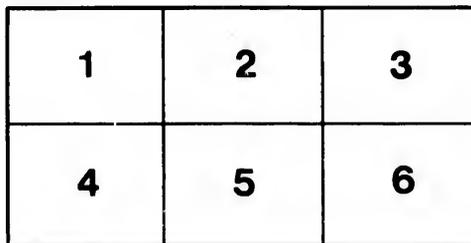
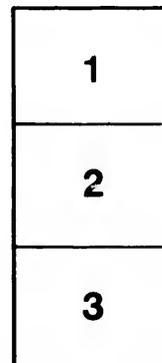
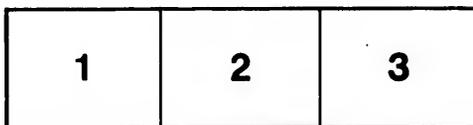
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REMARKS
ON
CANADIAN STRATIGRAPHY.

BY
THOMAS MACFARLANE.

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REMARKS ON: CANADIAN STRATIGRAPHY.

BY THOMAS MACFARLANE.

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 investigation 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 advanced 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 these 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 distinctly 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 indicating 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 sandstones 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

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 acknowledged that, whatever view may be entertained as to their origin, the schistose rocks above referred to underlie the Silurian 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 lithological 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 Pots-
 dam 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 remarkable 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 porphyries. 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 solidified 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 junction 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 crystalline 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 identical 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 calcspar; 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 Keweenaw series overlies the Huronian unconformably, and cites U. S. authorities against this view. Preferable to these would 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 seabeach, 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 Maimanse 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 *Muare*, 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" when any of his own theories require support.

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 represent 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 cutting 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 geologists who have carefully examined their stratigraphical relations. 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.)

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