ART. X.—*Remarks on the late Criticisms of Prof. Dana*; by
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IN this Journal for February last (p. 86) Prof. Dana has criticized certain points in my address "On the Geognosy of the Appalachians and the Origin of Crystalline Rocks," given in August, 1871, at Indianapolis, before the American Association for the Advancement of Science. I am charged by him with rejecting, for many mineral silicates, the view that they are pseudomorphs; that is to say, crystals chemically altered without loss of external form. I have denied that crystals of serpentine having the shape of chrysolite, pyroxene, dolomite, etc., and crystals of pinite having the shapes of nepheline or scapolite, are results of a chemical change of these species; notwithstanding this view is now held by most mineralogists on the grounds of similarities of geometrical form and the existence of what are regarded as intermediate stages in the process of transmutation; and I have maintained another and a very different view, which, in my opinion, is more rational. Until we can watch the transmutation of one of these species into another, the argument from supposed intermediate forms is worth no more in the mineral than in the organic world; the reasoning of the transmutationists, in the one case and the other, resting upon somewhat similar considerations. In either case we may say, with Prof. Warrington Smyth, that in these intermediate forms "lie the materials for a history;" while we venture, with him, to express a doubt whether, from a series of specimens supposed to show a transition from chrysolite to serpentine, or from hornblende to chlorite, "we are obliged to conclude that there has been, *historically speaking*, an actual transition from the one to the other." [See his anniversary address, as president of the Geological Society of London, in 1867.]

Prof. Dana says that Scheerer is the only one who shares my peculiar views on this question. I have, however, asserted in my address that Delesse has maintained the views of Scheerer and myself, as opposed to the popular doctrine of epigenesis, and shall endeavor to make good my assertion. In his essay on Pseudomorphs, published in 1859 [Ann. des Mines, V, xvi, 317-392], Delesse begins his argument by remarking that since, in some cases, a mineral is found to be surrounded by another clearly resulting from its alteration (as for example anhydrite by gypsum), certain mineralogists have supposed that wherever one mineral encloses another there has been epigenesis or pseudomorphous alteration. Such, he says, may sometimes be the case, but it is easy to see that it is not so habitually. A crys-

tallized mineral species frequently includes a large and even a predominating portion of another, and the combination is then considered by many as an example of partial pseudomorphous alteration. In such instances, remarks Delesse, the question arises whether we have to do with the results of envelopment, or of chemical alteration; to resolve which it becomes necessary to study carefully the problem of envelopment. He then proceeds to show that the enveloped substance is, in some cases, crystalline (and arranged either symmetrically or asymmetrically with regard to the enveloping mass), and in other cases amorphous, and enclosed like the sand-grains which predominate in the calcite crystals of Fontainebleau. The difficulty in deciding whether we have to do with envelopment or with epigenesis increases when the enveloped mineral becomes so abundant as to obscure the enveloping species, or when it becomes mixed with it in so intimate a manner as to seem one with the latter, (*se fondre insensiblement avec lui*). The proportions of the enveloped and the enveloping mineral, we are told, may so far vary that the one or the other is no longer recognizable. "As the forces which determine crystallization have a great energy, the enveloping mineral is sometimes found in so small a quantity as to be entirely masked by the enveloping species." "When minerals have crystallized simultaneously, they have been able to become associated with each other, and to envelop each other, in all proportions" [loc. cit., pages 338, 339, 341, 353].

Our author then proceeds to tell us that having carefully studied, in numerous specimens, the supposed mica-pseudomorphs of iolite, andalusite, cyanite, pyroxene, hornblende, etc., he regards them, as in all cases, examples of envelopment, and expresses the opinion that we must omit from our lists a great number of the so-called pseudomorphous minerals, especially among the silicates. The final result of the process of envelopment is, according to Delesse, this—to give rise to mixed mineral aggregates, owing their external forms to the crystallizing energy of one of the constituents, which may be present in so small a quantity as to be completely obscured by the other matter present. From this condition of things, result crystalline forms which, though totally different in their origin from the products of chemical alteration or substitution, are emphatically pseudomorphs.

From this process of mechanical and more or less heterogeneous envelopment, Delesse next proceeds to consider the crystallizing together of isomorphous or homœomorphous species, in relation to the generally received notion of epigenic pseudomorphism. He declares that "isomorphism explains very well facts which are often attributed to pseudomorphism," and

that many "minerals which are still considered pseudomorphs are in reality examples of isomorphism" [pages 364, 365]. Referring to the well-known investigations of Mitscherlich upon the crystallizing together, in all proportions, of isomorphous species, and of the symmetrical crystallization of one salt around a nucleus of another isomorphous with it, Delesse suggests that the different forms and varieties of hornblende and pyroxenic minerals afford many examples of the kind. He then adds, "If, as Scheerer has remarked, water plays in silicates the part of a base, anhydrous silicates may crystallize at the same time with hydrated silicates, and moreover be isomorphous with them." In this way, he suggests, we may explain by isomorphism, or homœomorphism, the association with pyroxene of the hydrous species, schiller-spar, as well as that "of various anhydrous and hydrated minerals" [pages 357, 358].

In further illustration of the words just quoted from Delesse, we may cite from Scheerer, as examples of what he called polymeric isomorphism, the association (in the same crystals) of iolite and aspasiolite, and of chrysolite and serpentine. If these, and similar species, crystallize together because they are isomorphous, it is evident that they may each crystallize separately; and thus the crystals of serpentine with the form of chrysolite, and those of aspasiolite and other so-called hydrous iolites, may be regarded as examples not of epigenesis, but of isomorphism.

We have thus endeavored to set forth, chiefly in his own words, the views enunciated in 1859 by Delesse, according to whom the phenomena of so-called pseudomorphism among mineral silicates are to be explained, for the most part, not by chemical alterations of pre-existing species but by envelopment and by isomorphism. That the above are really his views, and are moreover regarded by himself as contrary to those of the school which I oppose, Delesse does not permit us to doubt; for after having set them forth as his own, (*après avoir exposé notre manière de voir*), he says, "We hasten to add that these facts may also be explained in a manner altogether different (*peuvent aussi s'interpréter d'une manière toute différente*); and some savants of Germany, notably G. Rose, Haidinger, Blum, G. Bischof and Rammelsberg have sought their explanation in pseudomorphism. Their example has been followed by most mineralogists, etc." [pages 358, 359].

That the "pseudomorphism" of the authors just named is chemical alteration or epigenesis, it is not necessary to remind the reader; who will now be able to judge whether it is Prof. Dana or myself who has misrepresented or misunderstood Delesse. Let us, however, add that the long and somewhat diffuse memoir of the latter, from which we have quoted, is

wanting in unity of plan and purpose; and that parts of it, if we may hazard a conjecture, seem to have been written while he still inclined to the views of the opposite school. From the table of pseudomorphs which he has given, and from many passages in the text, it might be inferred that he then held the notions of Rose, Haidinger, etc., which he elsewhere, in the same paper, speaks of as being entirely different from his own. The views of Delesse, about this time, underwent a great change, which has a historic importance in connection with those which I advocate. When, in 1857 and 1858, he published the first and second parts of his admirable series of studies on metamorphism, Delesse held, in common with nearly every geologist of the time, to the eruptive origin of serpentine and the related magnesian rocks. Serpentine was then classed by him with other "trappean rocks;" and he elsewhere asserted that "granitic and trappean rocks" undergo in certain cases a change, near their contact with the enclosing rock, by which they lose silica, alumina and alkalies, and acquire magnesia and water, being thus changed into a magnesian silicate; which may take the form of saponite, serpentine, talc or chlorite. [Ann. des Mines, V, xii, 509; xiii, 393, 415]. It would be difficult to state more distinctly the view, which he then held, of the origin of these magnesian rocks and minerals by the chemical alteration of plutonic (granitic and trappean) rocks. This was in 1858, and in 1859 appeared the memoir on pseudomorphs, already noticed, in which, in place of the theory of epigenic pseudomorphism or chemical alteration of various mineral silicates, taught by the German school, he brought forward, in explanation of the facts upon which this was based, another theory, which was only an extension of that already maintained by Scheerer and myself.

It was not until 1861 that Delesse published the last part of his studies on metamorphism, which appeared in the *Memoirs of the Academy of Sciences of France* (vol. xvii), and in it we find that, consistently with the new views adopted by him in 1859, the old doctrine of the epigenic origin of serpentine and the related magnesian rocks from the alteration of plutonic rocks, is abandoned. In its stead, it is here suggested by Delesse that all these magnesian rocks result from the crystallization of the sepiolites or so-called magnesian clays, which are frequent in many sedimentary deposits. These, according to him, by a molecular re-arrangement of their elements, may give rise to serpentine, talc, chlorite, and their various associated and related minerals. The rocks thus generated are still declared to pass insensibly into plutonic rocks, but instead of maintaining, as in 1858, that they are derived from the latter, Delesse, in 1861, asserts, on the contrary, that "the plutonic

rocks are formed from the metamorphic rocks, and represent the maximum of intensity, or extreme limit of metamorphism."

This recognition of the notion that the great masses of serpentine, with their constantly associated hornblendic, talcose and chlorite rocks, have been directly formed from the molecular re-arrangement or *diagenesis* of aqueous magnesian sediments, and not from the chemical alteration, or *epigenesis* of erupted plutonic masses, marks a complete revolution in our views of the history of the crystalline rocks. The new doctrine did not, however, originate with Delesse, but was previously put forward by myself in a paper "On Some Points of Chemical Geology," read before the Geological Society of London in January, 1859, appearing in abstract in the *Philosophical Magazine* for February, and published at length in the *Geological Journal* for November, in the same year. I there maintained that serpentines were "undoubtedly indigenous rocks, resulting from the alteration of silico-magnesian sediments;" and moreover asserted that the final result of heat, aided by water, on such rocks, would be their softening, and, in certain cases, their extravasation as plutonic rocks; which were regarded "as, in all cases, altered and displaced sediments." When this paper was written, in 1858, I still supposed that the reactions between the elements in beds of siliceous magnesian carbonates (which, I had shown, may give rise to certain magnesian silicates in immediate proximity to eruptive rocks) might serve to explain the origin of great areas of serpentine and related crystalline magnesian silicates; but my studies of the silicates deposited during the evaporation of natural waters, and of the magnesian sediments of the Paris basin, soon led me to seek the origin of these rocks in the alteration of previously formed uncrystalline magnesian silicates. This view was set forth by me in this *Journal* for March, 1860 [II, xxix, 284] and more fully in the *Canadian Naturalist* for June, 1860 [also in this *Journal*, xxxii, 286], where it was pointed out that steatite, chlorite and serpentine were probably derived from sediments similar to the magnesian silicates found among the tertiary beds in the vicinity of Paris, the so-called magnesian clays.

We have seen that these various novel views, put forth by me in 1859 and 1860, though totally different from those taught by Delesse in 1858, were integrally adopted by him in 1861. These dates are circumstantially given in my address of last year, and yet Prof. Dana, in his review of it, charges me with "following nearly Delesse" as to the origin of serpentine. He also asserts that I "make Delesse the author of the theory of envelopment," when I have there declared that the view of Delesse—"that the so-called cases of pseudomorphism, on

which the theory of metamorphism by alteration has been built, are, for the most part, examples of association and envelopment, and the result of a contemporaneous and original crystallization,—is identical with the view suggested by Scheerer in 1846, and generalized by myself, when, in 1853, I sought to explain the phenomena in question by the association and crystallizing together of homologous and isomorphous species." To Delesse therefore belongs the merit not of having suggested the notion of envelopment in this connection, but of having pointed out the bearing of the envelopment of heteromorphous and amorphous species on the question before us.

Prof. Dana moreover asserts that while Scheerer is the only one who maintains similar views to myself, I, in common with all other chemists, reject the chemical speculations which lie at the base of his views. On the contrary, unlike most chemists, who have failed to see the great principle which underlies Scheerer's doctrine of polymeric isomorphism, I have maintained [this Journal, II, xv, 230 ; xvi, 218] that it enters into a general law, in accordance with which bodies whose formulas differ by nM_2O_2 or nH_2O_2 , may (like those differing by nH_2C_2), have relations of homology, and moreover be isomorphous. The existence of these same relations was further maintained and exemplified in a paper on Atomic Volumes, read by me before the French Academy of Sciences and published in the *Comptes Rendus* of July 9, 1855. This view, which I have never repudiated, is reiterated in my address last year, and declared to include the polymeric isomorphism of Scheerer.

Prof. Dana next says that in asserting that "the doctrine of pseudomorphism by alteration, as taught by G. Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof and many others, leads them * * * to maintain the possibility of converting almost any silicate into any other," I have, in his language, "grossly misrepresented the views of at least Rose, Haidinger, Blum, Rammelsberg and Dana," and that I "complete the caricature" by this sentence, to be found in my address: "In this way we are led from gneiss or granite to limestone, from limestone to dolomite, and from dolomite to serpentine; or more directly from granite, granulite or diorite to serpentine at once, without passing through the intermediate stages of limestone and dolomite;"—"part of which transformations," says Prof. Dana, "I, for one, had never conceived; and Rose, Haidinger, Rammelsberg, and probably Blum, and the 'many others,' would repudiate them as strongly as myself." The '*many others*,' as he rightly remarks, are "other writers on pseudomorphism," among whom it would be unjust not to name their progenitor, Breithaupt, von Rath and Müller, at the same time with Volger and Bischof. According to Prof. Dana, I "add to the misrep-

resentation by means of the strange conclusion that because such writers hold that crystals may undergo certain alterations in composition, therefore they believe that rocks of the same constitution may undergo the same changes." This "*strange conclusion*" I have always supposed to be Prof. Dana's own. No one has perhaps asserted it so clearly or so broadly as himself, and I shall therefore quote his own words in my justification. As early as 1845, in an article entitled "Observations on Pseudomorphism," [this Journal I, xlviii, 92] he wrote: "The same process which has altered a few crystals to quartz has distributed silica to fossils without number, scattered through rocks of all ages. The same causes that have originated the steatitic scapolites, occasionally picked out of the rocks, have given magnesia to whole rock-formations, and altered, throughout, their physical and chemical characters. If it be true that the crystals of serpentine are pseudomorphous crystals, altered from chrysolite, it is also true, as Breithaupt has suggested, that the beds of serpentine containing them are likewise altered; though often covering square leagues in extent, and common in most primary formations. The beds of steatite, the still more extensive talcose formations, contain everywhere evidence of the same agents." Again, in 1854, in his *Mineralogy*, 4th edition, [page 226], Prof. Dana, after a complete list of pseudomorphs, compiled from the writers of the school in question, says: "These examples of pseudomorphism should be understood as cases not simply of alteration of crystals, but in many instances of changes in beds of rock. Thus all serpentine, whether in mountain-masses, or the simple crystal, has been formed through *a process of pseudomorphism, or in more general language, of metamorphism*; the same is true of other magnesian rocks, as steatitic, talcose or chloritic slates. *Thus the subject of metamorphism, as it bears on all crystalline rocks, and of pseudomorphism, are but branches of one system of phenomena.*" If there could be any doubt as to the meaning of the words which we have italicized, in quoting them from Prof. Dana, it is removed by his language in 1858. Then, as now, adversely criticising my views on this question, he refers to the statements, above cited, made in 1845 and 1854, as expressions of his doctrine, mentioning especially the first one, in which, he says, "*metamorphism is spoken of as pseudomorphism on a broad scale.*" [This Journal, II, xxv, 445]. I confess that I do not understand Prof. Dana, when in his last criticism of me, fourteen years after the one just quoted, he reproaches me with having charged him with holding the doctrine that "*regional metamorphism is pseudomorphism on a grand scale*;" and declares that he makes no such remark, neither expresses the sentiment in his *Mineralogy* of 1854.

With these citations before us, and remembering the views Scheerer, and the later ones of Delesse, together with the language of the latter in his essay on Pseudomorphs, let us notice the words of Naumann, addressed to Delesse in 1861, in allusion to the essay in question. "Permit me to express to you my satisfaction for the ideas enunciated in your memoir on Pseudomorphs, ideas which my friend Scheerer will doubtless share with myself" (*idées que mon ami M. Scheerer partagera sans doute comme moi-même*). Then follows the language which I have quoted in my address, in which he combats the error of those who hold that gniesses, amphibolites, and other crystalline rocks are "the results of metamorphic epigenesis, and not original rocks," and adds, "It is precisely *because pseudomorphism has so often been confounded with metamorphism* that this error has found acceptance." [Bull. Soc. Geol. de Fr., II, xviii, 678]. The reader must now judge whose opinions it is that are here denounced as erroneous, and whether Naumann was on the side of Prof. Dana, or, with Delesse, on the side of Scheerer and myself. I insist the more strongly on this matter, because Prof. Dana not only declares that Delesse and Naumann have always avowed the doctrines of the transmutationist school, and do not in any way whatever countenance my views, but implies that I have dealt unfairly with these authorities. In regard to another point raised by Prof. Dana, I may remark that their explicit declarations are not to be set aside, because traces of the doctrine of epigenic pseudomorphism still hold a place in the last edition of Naumann's *Mineralogy*, or in the *Revue de Géologie*, of which Delesse is one of the editors.

Prof. Dana says "If there was any occasion for a notice of my opinions, a critic of 1871 should have referred to the formal expression of them in my *Manual of Geology*, first published in 1863. The reader will there find the *diagenesis* of Gümbel, which Mr. Hunt takes occasion to commend, * * * with but a brief allusion to pseudomorphism." The doctrine of diagenesis, it is hardly necessary to say, I have never attributed to Gümbel, nor does he claim it. It is the old doctrine of Hutton, Playfair and Boué, is taught by Bischof [Chemical Geology, III, 318, 325, 342], and pervades my papers of 1859 and 1860, already referred to. But while it has been generally admitted that what, in my address, I have called the first class of crystalline rocks (consisting chiefly of quartz and aluminous silicates) might result from the molecular re-arrangement of the elements of clay and sand-rocks, I maintained in those papers that what I have called the crystalline rocks of the second class (in which protoxide silicates predominate) have been generated, by a similar process, from deposits of chemically-formed silicates. This view being adopted by Delesse and by Gümbel to explain

the origin of the various magnesian silicated rocks, hitherto generally regarded as the product of epigenesis, the latter has proposed to designate the process as diagenesis; a term which I adopt, as one well fitted to denote the generation of all kinds of crystalline rocks through a molecular re-arrangement of sedimentary deposits, of whatever origin. Prof. Dana, in common with most other geologists, admits in his *Manual* the production by diagenesis of the rocks of the first class, but in the case of serpentine and steatite declares them to have been formed by epigenic pseudomorphism or chemical alteration of pyroxenic and other crystalline rocks; *the origin of which is left entirely unexplained*. It is true that his allusions to pseudomorphism in this volume are confined to very brief notices, on pages 704 and 710; a fact which is the more noticeable, when we recall that the author had formerly expressed the belief "that pseudomorphism will soon constitute one of the most important chapters in geological treatises." [This Journal, I, xlviii, 66.] That Prof. Dana has receded from the extreme views on this subject which he maintained from 1845 to 1858, and which I have constantly opposed, seems probable; but until he formally rejects them, the student of geology will not unnaturally suppose that he still gives the sanction of his authority to the doctrine which he once taught, without any qualification, but now repudiates, that "*metamorphism is pseudomorphism on a broad scale*."

Prof. Dana having clearly defined the proposition that the chemical alterations which are recognized in individual crystals are to be conceived as extending to rock-masses; and having moreover asserted that the principle of the identity of metamorphism and pseudomorphism "bears on all crystalline rocks," is logically committed to all the deductions as to the changes of rocks which the transmutationist school has drawn from the supposed alteration of minerals. By reference to the table of pseudomorphs in the fourth edition of Dana's *Mineralogy*, it will be seen that each one of the metamorphoses of rocks mentioned in the above extract from my address is based upon an asserted epigenic change or conversion of the constituent species. I shall however show, in addition, that in each case the application of the principle to rock-masses has been recognized by one or more of the authorities already named, and that the so-called caricature has been drawn by their own hands. It would be easy, did space permit, to extend greatly this list of supposed transmutations. The various associations of rocks and minerals in nature, when interpreted according to the canons of this school, seem in fact, as remarked by Prof. Warington Smyth, in his address already quoted, "to offer a premium to the ingenious for inventing an almost infinite

series of possible combinations and permutations." Before proceeding further it is to be noted that no distinction can, in many cases, be established between the results of alteration (or partial replacement) and substitution (or complete replacement); since successive alterations may give the same product as direct substitution. Thus, for example, quartz might be directly replaced by calcite, or else first altered to a silicate of lime, which, in its turn, might be changed to carbonate. The alteration of quartz to a silicate of magnesia, and that of both pyroxene and pectolite to calcite, is maintained by the writers of the present school.

Metamorphosis of granite or gneiss to limestone. Calcite, we are told, is pseudomorphous of quartz, of feldspar, of pyroxene, and of garnet, besides other species: it moreover replaces both orthoclase and albite "by some process of solution and substitution." [Dana's Mineralogy, 5th edition, 361.] Since quartz, orthoclase and albite can be replaced by calcite, the transmutation of granite or gneiss into limestone presents no difficulty. I cannot, at present, give the reference to the statement of Volger that some gneissoid limestones owe their origin to such a process.

Metamorphosis of limestone to dolomite. This change is maintained by von Buch, Haidinger and many others. I am blamed for mentioning in connection with this school the name of Haidinger, who, Prof. Dana says, "never wrote upon the subject of the alteration of rocks." It has, however, never before been questioned that Haidinger was the first, if not to suggest, to clearly set forth the theory of the supposed conversion of limestone into dolomite by the action of magnesian solutions, aided by heat and pressure; a theory which I have elsewhere refuted. [Bischof, Chem. Geology, iii, 155, 158; Zirkel, Petrographie, i, 246; Liebig and Kopp, Jahresbericht, 1847-48, 1289, and this Journal, II, xxviii, 376].

Metamorphosis of dolomite to serpentine. This change is maintained by G. Rose [Bischof, Chem. Geol., ii, 423], and by Dana [this Journal, III, iii, 89].

Metamorphosis of granite, granulite, and eclogite directly into serpentine, chlorite and talc. These transmutations are maintained by Müller, and adopted by Bischof. [Chem. Geol., ii, 424, 434.]

Metamorphosis of limestone to granite or gneiss. This is taught by Blum and Volger. [Ibid., ii, 186; iii, 431.]

Having thus given the authorities for the examples cited in my address, I may notice some further illustrations of the doctrine from the pages of Bischof's work already quoted. Metamorphosis of diorite, hornblende-rock and labradorite to serpentine; G. Rose, Breithaupt, von Rath [ii, 417, 418]. Diorite and

hornblende-slate to talc-slate and chlorite-slate; G. Rose [iii, 312]. Mica-slate to talc-slate, and steatite and mica to serpentine, steatite and talc; Blum, C. Gmelin [ii, 405, 468]. Quartz-rock to steatite; Blum [ii, 468].

With regard to New England rocks, Prof. Dana asserts that "there are gneisses, mica-schists, and chloritic and talcoid schists in the Taconic series." I have, however, shown in my address that Emmons, the author of the Taconic system, expressly excluded therefrom the crystalline rocks, which he included in an older primary system; excepting, however, certain micaceous and talcose beds, which he declared to be recomposed rocks, made up from the ruins of the primary schists, and distinguished from these by the absence of the characteristic crystalline minerals which belong to the Green Mountain primary schists.

Again, Prof. Dana states that I make the crystalline schists of the White Mountains a newer series than the Green Mountain rocks. A careful perusal of my address will show that I nowhere assert that the rocks of the third series, on my line of section, are younger than the second series. Such a view of their relations has, however, been maintained for the last generation by the Messrs. Rogers, Logan, and many others, all of whom assigned the crystalline schists of the White Mountains to a higher geological horizon than the Green Mountains. In support of this view of their relative antiquity, I have, it is true, brought together observations from South Carolina, Pennsylvania, Michigan, Ontario, and Maine, all of which point to the same conclusion; and I might now add similar evidence from New Brunswick and from Nova Scotia. My "chronological arrangement" of New England crystalline rocks, as it is called by Prof. Dana, so far as it is my own, is limited to my affirmation that they are all of pre-Cambrian age; in proof of which it need only be mentioned that the crystalline schists of both the types in question are, in southern New Brunswick, directly overlaid by uncrystalline shales, sandstones and conglomerates, made up in part of the ruins of these, and holding a Cambrian (Menevian) fauna.

As regards the mica-schists with staurolite, cyanite, andalusite and garnet, I have in my address pointed out the fact that they appear to belong to a great series of rocks, very constant in character, which have a continuous outcrop from the Hudson river to the St. John, a distance of 500 miles, and, in the latter region are clearly pre-Cambrian. I have moreover brought together the evidence of observers in other parts of North America, in Great Britain, in continental Europe, and in Australia, showing that similar crystalline schists, holding these same minerals, always occupy, in these regions, a similar geological horizon. Prof. Dana hereupon inquires whether any

one has yet *proved* that these mineral characters are restricted to rocks of a certain geological period. I answer, that in opposition to these facts, it has not yet been proved that they belong to any later geological period than the one already indicated; and that it is only by bringing together observations, as I have done, that we can ever hope to determine the geological value of these mineral fossils. In no other way did William Smith prove, in Great Britain, the value of organic fossils, and thus lay the foundations of paleontological geology.

Montreal, April, 1872.