

sciousness, and *vice versa*, quickly, almost instantaneously, and many persons habitually do so. But the transition period is sometimes prolonged, and stages are observable. The first thing that occurs is the lowering, or cessation, of that control over the mental processes which is the highest of our powers, the one requiring the greatest effort, and the one most easily lost. In this condition the thoughts ramble unchecked, chase one another confusedly over the mental field, and give rise to all sorts of incongruities of the imagination. At the same time, being unrestrained, they are excited, and evince efforts of memory and even of combination, of which, in the regulated state of wakefulness, they are quite incapable. In this way the images of persons and places, events, and items of knowledge, long forgotten in the ordinary state, are recalled with distinctness, and we fancy that new information has been acquired when it is only forgotten facts that are recalled. He did not agree with the physiologists who conceive that dreaming depends upon an inequality in the condition of different parts of the brain, some being excited or wakeful, while others are quiescent or asleep. He rather took the view that all the parts of the cerebral hemispheres combine in each of the efforts of control, consciousness, memory, and other mental acts, that all suffer alike from those efforts, alike need the restoring changes which take place in sleep, and, together, *pari passu*, pass through the stages on the way to and from sleep, in which dreaming, sleep-walking, &c., occur.

NOTICE OF THE ADDRESS OF PROF. T. STERRY HUNT BEFORE THE AMERICAN ASSOCIATION AT INDIANAPOLIS *

IN a brief notice of the recent address of Prof. Hunt, it is stated that, while the discussions show learning and research, and his review of the progress of opinions with regard to the Taconic and associated rocks is an able presentation of the subject, its conclusions are throughout open to doubts and objections. Since it is fairer to an author to make special, rather than general, criticisms, I propose to state here a part of the objections referred to in that remark. They are as follows:—

1. That, while accepting the ordinary views with regard to most "pseudomorphs by alteration" (crystals chemically altered without a loss of form), he rejects them with respect to those that are silicates in composition; that is, he denies that the crystals of serpentine having the form of chrysolite, pyroxene, dolomite, &c., are pseudomorphs; and the same of those of steatite, having the form of hornblende, pyroxene, spinel, &c.; of those of pinite having the form of nephelite, scapolite, &c.; and so in other cases:—notwithstanding that (1) they bear positive evidence of change in having ordinarily no polarising properties, and no other interior features or qualities conforming to the external form; that (2) the crystalline forms are just those presented by the species after which they are supposed to be pseudomorphs, and the idea of their being real forms of a single polymorphous species is wholly inadmissible, as pronounced by every crystallographer who has written on the subject; that (3) the pseudomorphs show all stages in the process of change from incipient to complete alteration, in the latter case not a trace of the original mineral remaining.

In this assumption, for it is little better, he opposes the views of every writer on pseudomorphs, excepting one—Scheerer; and Scheerer's chemical speculations, which are at the basis of his opinions, he rejects, like all other chemists.

This unwarranted assumption has a profound position in the system of views on metamorphism which Prof.

Hunt holds, and gives shape and intensity to his opinions of the views of others.

2. That, in commencing a paragraph with the sentence, "The doctrine of pseudomorphism by alteration, as taught by Gustaf Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof, and many others (meaning thereby other writers on pseudomorphism), leads them, however, to admit still greater and more remarkable changes than these, and to maintain the possibility of converting almost any silicate into any other"—he grossly misrepresents the views of at least Rose, Haidinger, Blum, Rammelsberg, Dana; and that he completes the caricature in the closing sentence of the same paragraph, in which he says, "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, I, for one, had never conceived; and Rose, Haidinger, Rammelsberg, and probably Blum and the "many others," would repudiate them as strongly as myself. Next follows a verse from Goethe, that is made to announce his personal vexation with their "sophistries;" *alias* absurdities, as the context implies.

Prof. Hunt's rejection of established truth alluded to under sec. 1 here manifests its effects in leading him to misrepresent—although unintentionally—the views of writers on pseudomorphism; and to add to his misrepresentation 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; as if it were not possible that external or epigenic agencies might reach and alter crystals under some circumstances of position, when they could not gain access to great beds of rock. Haidinger, the eminent crystallographer, mineralogist, and physicist of Vienna, and one of the most prominent writers on pseudomorphism, never wrote upon the subject of the alteration of rocks at all, and this is true of others, against whom the above charge is made by Mr. Hunt.

With a little clearer judgment, part at least of that vexation of spirit which required the help of a great German poet, and the German language, adequately to express, might have been avoided.

3. That he charges me with the opinion of Bischof, that "regional metamorphism is pseudomorphism on a grand scale:" when I make no such remark, neither express the sentiment, in my *Mineralogy* of 1854, in which I give an abstract of Bischof's views and make my nearest approach to them; and when, 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 Gumbel, which Mr. Hunt takes occasion to commend, applied, as had been done by others, although Gumbel had not then announced it; and also other points discussed, with but a brief allusion to pseudomorphism.

The above remark by Mr. Hunt is not made with special reference in his address to magnesian silicates, or any other particular class of siliceous minerals; but, as the context shows, to rocks in general. I have held to views respecting the origin of serpentine which Prof. Hunt rejects, and have sustained them on the ground that the pseudomorphous crystals of serpentine show what transformations are chemically possible, and that hence they may possibly illustrate the changes which beds of rock have undergone. I have not applied this principle in accounting for the origin of ordinary metamorphic rocks, because, as above observed, crystals may often be reached by agencies which can never reach or affect rock-formations, and for various other reasons against it. But the case of serpentine has been regarded as somewhat

* Prof. Hunt's address has been published in the "*American Naturalist*" for September, 1871, and, since then, in part, in *NATURE*, Vol. v. Nos. 105, 106, 107. Prof. Dana's reply is reprinted from advance-sheets of *Silliman's Journal* forwarded to us by the author.

different; and I have believed, and still believe, that extended beds of rock have been turned into this mineral by a method analogous to that which takes place in pseudomorphism. Had Mr. Hunt's statement been made a special one, restricted to this case, I should have had little objection to it. I may add that the method of origin for serpentine which I have deemed most probable (though perhaps not the only method) is one which he once advocated—that of the alteration of beds of dolomite, or magnesian carbonate of lime, by waters containing alkaline silicates in solution; and it has appeared to me that the facts (1) that serpentine is commonly associated with beds of limestone or dolomite, (2) that chrysolite crystals are sometimes found in these rocks, and (3) that the forms of crystals of both dolomite and chrysolite occur among serpentine pseudomorphs, give strong support to this view.

Prof. Hunt's opinion on this point in 1857 he thus expressed in a letter to the writer, sent for insertion in "Siliman's Journal," where it appears in volume xxiii. (1857) at p. 437, as a conclusion to his brief statement.

"Suppose a solution of alkaline silicate, which will never be wanting among sediments where feldspar exists, to be diffused through a mixture of siliceous matter and earthy carbonate, and we have, with a temperature of 212° F., and perhaps less, all the conditions necessary for the conversion of the sedimentary mass into pyroxenite, diallage, serpentine, talc, rhodonite, all of which constitute beds in our metamorphic strata. Add to the above the presence of aluminous matter, and you have the elements of chlorite, garnet, and epidote. We have here an explanation of the metamorphism of the Silurian strata of the Green Mountain range, and I believe of rock metamorphism in general." Again, in a letter dated July 6th, published in volume xxiv., at page 272, he says:

"I have already in a previous note indicated the manner in which I suppose these siliceous and argillaceous magnesian and dolomites to have been in certain parts of the formation transformed by the intervention of solutions of alkaline carbonates into silicates, such as talc, serpentine, chlorite, pyroxenite, &c. A further development of my views of the metamorphism of sediments, with the results of the investigation of a great many altered rocks, will appear in the Report of Progress of the Geological Survey of Canada for the last three years—now in press."

It should be added, that Prof. Hunt acknowledges his change of opinion in his address. But, in view of it, some moderating of his positiveness of assertion would have been reasonable.

4. That he attributes the origin of *beds* of serpentine and steatite,—here following nearly Delesse,—to the alteration of beds of different hydrous magnesian silicates related to sepiolite (meerschau), formed in the surface waters of an era—Palæozoic or earlier—while fossiliferous rocks were in progress:—when, as a matter of fact, no such sepiolite-like beds are known to occur anywhere in *unaltered* stratified formations of Palæozoic or pre-Silurian time, and they are found of limited extent only in some strata of comparatively recent origin. The hypothesis, although deserving of consideration, is therefore without any solid foundation. The doubts that have been recently thrown about the Eozoön affect unfavourably the hypothesis, since these supposed fossils have been made prominent in its support. The view, if true, would, as Prof. Hunt implies, bring the making of serpentine and steatite rocks under the kind of metamorphism styled by Gumbel diagenesis, instead of that of epigenesis; making them a result of change without an addition of ingredients from any external source, like most other metamorphism, instead of through the agency of outside ingredients. But it wants facts to rest upon.

5. That he attributes an origin similar to that for serpentine and talc to beds of chlorite and hornblende; notwithstanding the fact that chlorite schist and horn-

blende schist—the purest forms of any large beds of these minerals—are always more or less impure, and often graduate into clay slate on one side, and mica schist on the other; and that these schists are thus so involved with others, that if one is derived from ordinary sedimentary beds, all must be.

6. That he devotes some pages to a "theory of envelopment" as a method of accounting for the silicate pseudomorphs referred to, beginning a paragraph with the sentence:—

"By far the greater number of cases on which this general theory of pseudomorphism by a slow process of alteration in minerals has been based, are, as I shall endeavour to show, examples of the phenomenon of mineral envelopment, so well studied by Delesse in his essay on Pseudomorphs."

While, in fact, this theory has almost nothing to do with the subject, since pseudomorphs of serpentine, steatite, and other species, with regard to which there is the dispute, consist often of *pure* serpentine, steatite, &c., and therefore have no enveloper, and are not cases of envelopment. This theory supposes the material of the so-called pseudomorph to be an impurity taken up into a crystal in process of formation—a thing of common occurrence; and, if satisfactory, would account for the want of conformity between internal qualities and external form. It is unfortunate for it that, as just shown, it does not apply where it is wanted.

7. That he makes Delesse the author of the "theory of envelopment":—when Delesse has not proposed any such theory for cases of ordinary pseudomorphism, but has simply commenced, and very judiciously, his work on Pseudomorphs (1859) by distinguishing the examples of mere impurity, or envelopment, in crystallisation, in order to clear the way for the actual facts; and then gives a long list of admitted pseudomorphs, including in it nearly all kinds so recognised by other authors, and all that affect the question discussed by Prof. Hunt; serpentine occurring in the list as forming pseudomorphs after chrysolite, hornblende, garnet; steatite after pyroxene, hornblende, epidote, scapolite, mica, topaz, magnesite, dolomite, &c. In his work on metamorphism (1861), Delesse takes back none of his views on pseudomorphism; and in his late "Reviews of the Progress of Geology," down to the last just out (1871), he reiterates the ordinary views with regard to pseudomorphism, and mentions the occurrence of other pseudomorphs consisting of talc, serpentine, &c.

8. That he cites Naumann as sustaining the "theory of envelopment":—when this learned crystallographer and mineralogist has only commended Delesse's chapter on the envelopment of minerals in crystals, and presents in his "Mineralogy" (the last edition of which, that of 1871, is now before me) the subject of pseudomorphism in the usual way, with nothing whatever on the theory of envelopment; and, under the description of the species serpentine, he speaks of "large pseudomorphous crystals of serpentine from Snarum which still contain a nucleus of altered chrysolite."

There is hence no foundation for Mr. Hunt's statement that his views are "ably supported by Delesse," or any occasion for the "no small pleasure" he derived from Naumann's letter; or any warrant for the remark (p. 47) that Delesse and Naumann hold the "view" "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 crystallisation." These men of science are not to be counted upon for aid, countenance, or comfort; though claimed as friends, it has not been their fault, as they have always avowed the opinions of Haidinger and the "many others." It is a strange fact that, neither these claimed friends, nor the many announced opponents, with one or two exceptions,

hold the views which Prof. Hunt has attributed to them in his address. We are glad to know that this is not the usual American method of dealing with authorities.

Gümbel and Credner are the other two claimed supporters of his views. They have sustained Mr. Hunt's opinions as regards the Eozöon and the origin of the serpentine constituting it. But whether they disagree with Haidinger and all others as to pseudomorphs of serpentine, and of other hydrous silicates, I cannot say.

9. That while setting down the Taconic rocks, and rightly, as Lower Silurian in age, he denominates the micaceous gneisses, diorites, epidotic and chloritic, steatitic and serpentinous rocks, talcoid mica schists, quartzites, and clay-slates (which are always without staurolite or andalusite), in fact, the whole range of metamorphic rocks, with small exceptions, between the Connecticut river and the great limestone formation of the Green Mountains (admitted to be Lower Silurian), as the *Green Mountain Series*, and makes the whole "pre-Cambrian" in age, although the region has not been examined by any one stratigraphically with the care necessary for a positive opinion; and, although there are gneisses, mica schists, and chloritic talcoid (or mica) schists in the Taconic series, and therefore of admitted Lower Silurian origin, which are closely like those of his Green Mountain Series.

10. That he denominates, in like manner, the gneisses, mica schists (said to be richer in mica than those of the Green Mountain Series), hornblending gneisses and schists, micaceous and clay-slates containing andalusite, cyanite, or staurolite, and certain limestones, existing east of the Connecticut river, as a *White Mountain Series*, and makes these a newer "pre-Cambrian" than the Green Mountain Series:—when there is the same want of stratigraphical evidence as to age as in the former; and when Prof. C. H. Hitchcock's discoveries of Helderberg corals (Lower Devonian, according to Billings, or else upper beds of the Upper Silurian), at Littleton, not far north of the western extremity of the White Mountain Series, makes it more probable that part of the White Mountain Series of beds are of Helderberg age rather than pre-Silurian; and his discovery of labradorite rocks on the south-western margin of the White Mountains, wholly unlike any of the so-called White Mountain Series, shows further that a vast amount of study in the field is needed before the dictum of any one respecting the age of New Hampshire rocks is worth much.

It is now proved that there are labradorite rocks in Waterville and Albany, N.H., on the borders of the White Mountain region, which are probably of Laurentian age; that on the other side of the White Mountain line, but 25 miles to the north-northwest, there are fossil-bearing, metamorphic rocks of the *Helderberg* (upper or lower) period; that 100 miles south-southwest, in Bernardston, Mass., or central New England, there are other fossil-bearing metamorphic *Helderberg* rocks, some of the well-preserved crinoidal stems (as the writer has seen, as well as read of in the account of Prof. Hitchcock) *an inch in diameter*. Who then knows whether all, or any, of the long intermediate periods of geological time, from the Laurentian to the Devonian, are represented in the New Hampshire metamorphic rocks lying between these limits? When observation has given positive knowledge, we may then have several "White Mountain Series."

11. That he has relied, for his chronological arrangement of the crystalline rocks of New England and elsewhere, largely on lithological evidence, and commends this style of evidence, when such evidence means nothing until tested by thorough stratigraphical investigation. This evidence means something, or probably so, with respect to Laurentian rocks; but it did not until the age of the rocks, in their relations to others, was first stratigraphically ascertained. It may turn out to be worth something as regards later rocks when the facts have

been carefully tested by stratigraphy. A fossil is proved, by careful observation, to be restricted to the rocks of a certain period, before it is used—and then cautiously—for identifying equivalent beds. Has anyone proved by careful observation that crystals of staurolite, cyanite, or andalusite, are restricted to rocks of a certain geological period? Assumptions and opinions, however strongly emphasised, are not proofs.

It is no objection to stratigraphical evidence that it is difficult to obtain; is very doubtful on account of the difficulties; may take scores of years in New England to reach any safe conclusions. It must be obtained, whatever labour and care it costs, before the real order and relations of the rocks can be known. Until then, lithology may give us guesses, but nothing more substantial.

Mr. Hunt's arguments with reference to the White Mountain Series, as urged by him in 1870, will be found in *Silliman's Journal*, ii. l. 83. Both there, and in his address, may be seen the kind of evidence with which he fortifies, or supplements, that based on the character of the rocks. Direct stratigraphical investigation over the region itself, in which all flexures, faults, and unconformabilities have been thoroughly investigated, is not among the foundations of opinion which he brings forward.

He endeavours to set aside the objections to his views suggested by the existence of Devonian or Helderberg rocks in central and northern New England; but he presents, for this purpose, only some general considerations of little weight, instead of definite facts as to the extent and variety of the metamorphic strata that are part of, because conformable to, these Helderberg beds. Had he studied up these stratigraphical relations with the care requisite to obtain the truth, and all the truth, perhaps he would no longer say—it is "contrary to my notions of the geological history of the continent to suppose that rocks of Devonian age could in that region have assumed such lithological characters." Notions often lead astray.

JAMES D. DANA

NOTES

THE Royal Horticultural Society has taken a step which may prove very advantageous to the interests of science, namely, the appointment of a botanical Professor, who, by lectures, answers to personal inquiries, and other means, shall assist in establishing a more correct knowledge of the principles of botany and horticulture, and of the names of plants, among those of the Fellows and their gardeners who are desirous to profit by the opportunity. Among the duties of the Professor of Botany will be to conduct the scientific business of the society, both horticultural and botanical; to enter into communication with horticultural and botanical establishments at home and abroad; to conduct the meetings and edit the publications of the society; to give courses of lectures on scientific botany to the gardeners and others; and to have a general superintendence of the gardens at Chiswick. The appointment to this office of Mr. W. T. Thiselton-Dyer, late Professor of Botany at the Royal College of Science, Dublin, is a guarantee that the cultivation of scientific botany will not be neglected.

DR. DAVID FERRIER has been appointed Professor of Forensic Medicine at King's College, London, *vice* W. A. Guy, M. B., resigned.

THE Secretary of State for India has appointed Mr. A. G. Greenhill, Fellow of St. John's College, Cambridge, Professor of Applied Mathematics at the Civil Engineering College, Cooper's Hill. Mr. Greenhill graduated as Second Wrangler in 1870, and was bracketed equal with the Senior Wrangler for the Smith's Prize; he also gained a Whitworth Scholarship while an undergraduate.