

sin living at thy touch, the powers of hell fleeing before thy voice." Faithful Christian workers, mighty in faith and love, are the best evidences of Christianity. We are not to prove that it is from God merely by its great works in the past. We are not to be obliged to point to the primitive church as the most beautiful exhibition and the sufficient proof of the power of the gospel, but to create now an age of Christian purity and power. "The fathers did eat manna in the wilderness"; we thank God for that. "But they are dead"; God now is giving us the living bread, that we may eat thereof, and not die.

ARTICLE V.

LYELL'S STUDENT'S ELEMENTS OF GEOLOGY.

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ANOTHER volume by Sir Charles Lyell¹ appeared a few months ago in England, and is now republished in this country. It is partly a new book, in part a recast and revision of the last edition of the "Elements."² As its title indicates, it is designed for students. It has been the aim of the author to present the matter in such a light as, without sacrificing substance, to adapt the publication to beginners. By the omission of portions of the earlier work, room has been secured for large additions; while effort has been made to exhibit the subject in fullest consonance with the existing state of knowledge.

Of course, on the appearance of any such work, it is all-

¹ The Student's Elements of Geology, by Sir Charles Lyell, Bart. F.R.S. London. 1871. New York: Harper and Brothers. 1871.

² The latter work which was originally prepared as a "Supplement" to the Principles of Geology, was published as a separate duodecimo volume in 1836, and passing through successive forms reached the sixth and last edition in 1857. There was thus abundant occasion for a recension, and an ample opportunity for improvement, so great has been the progress made in geology during the last decennium.

important to know whether the task, gratefully assumed by the author, have been well executed; whether the subject, as profoundly grasped, have been genially and thoroughly elaborated; and whether the work be adapted to its end. One of the most prominent educators in the country having asked my opinion of the volume, I have read it with care, that my judgment might rest, not simply on familiar acquaintance with previous writings of the author, but especially on the merits of the book in question. In thus examining the work, I have had primary reference to its fitness for its proposed end, namely, to serve as a manual for students in elementary geology.¹ Thinking that the results reached might be of benefit to many teachers, and perhaps of interest to others enlisted in studies of this kind, I will transcribe some of the points noted.

It may be said, at the outset, that the volume, while in no sense exhaustive, is a repository of important facts. Being familiar with nearly, if not quite, all the previous editions of the "Elements," from the first impression down to the latest, I wish also to premise that each recension has been, as might have been reasonably expected, in some respects, an improvement on what has preceded, and that the volume now under consideration has some features deserving of praise, for which one will vainly look in any of the earlier forms of the work. While Sir Charles has seldom been in advance, he has labored hard to keep fully up with the march of science.

¹ Another point which stands specially prominent is not directly mentioned. It will be readily understood when it is added that the present Article is furnished as introductory to a series of papers on the Relations of Natural Science to Theology. As such, it is perhaps well suited to suggest, that while the great principles of geology rest on a substantial basis, there is not a little current in geological literature, and even in the writings of so-called standard authorities, that is by no means trustworthy. In a second introductory Article — which will appear in due time and be devoted to a critical review of Mr. Darwin — the question will be tacitly raised whether, while zoölogy as a science has a valid foundation, there be not much zoölogical speculation that is utterly untenable in the light of sound logic, and wholly unsupported by facts. These preliminary Articles will, it is thought, prepare the way for a thorough and impartial consideration of some of the relations of science to religion.

In this way he has been able to evince progress in each new issue of his writings. It is true that the nature of these progressive movements has been largely determined by the labors of others — by the progress actually achieved in science by original research. Still, these improvements, as should be added, have been made not merely in the way of supplements and addenda; there have been constant eliminations of obsolete matter, and a real incorporation of new material, — as it were an interstitial growth, — corresponding with the advance of the times. And this is a marked excellence — one too seldom met with in the publications of the day. It is in this direction that the present work has its special claim to favor.

But, while certain commendable points, which began to show themselves in the first edition of the "Principles," some forty years ago, have gradually become more apparent as the author has matured, there were also equally marked defects, some of which have augmented in like proportion with the lapse of time. These, having escaped correction, now reveal themselves with glaring prominence in this his latest work. A few of them, as seen in contrast with what one might expect to find in any widely-used manual of geology, may be now passed in hasty review.

Among these points, the *evolution of the subject*, including the method adopted, has special claim to attention. As must be evident, a right method is all-important, especially in a work designed for beginners. An examination of the one selected by the author will reveal the character of the work in this direction. To indicate his method, in a word, is not easy. Still, it may be, perhaps, appropriately designated as at once complex and regressive. From the multitudinous objects of to-day the movement is by slow degrees toward the greater simplicity of earlier times. Starting from the present, with all its multiplicity and diversity, the author, if I may so say, advances backward — often backward several steps at once, and then forward, and so gradually toward the more primitive periods by successive hitches. Such a

course almost inevitably necessitates confusion, rendering a mental translation or re-arrangement perpetually needful, if the pupil would get any distinct and just view of the real sequence and progress evinced in nature. Indeed, constant effort is requisite, even on the part of the expert, that he may keep this vast complication, as seen in its reversed order, clear in all its parts. In the "Principles" this regressive movement is appropriate. It is, in fact, admirable; for it is entirely consonant with the aim of the work. But the composition of that book seems to have put the author under a bias from which he has never recovered — a bias which appears in other works requiring an entirely different method. For his adoption of this course in the present volume there might be more show of reason, were the student at the start — as ordinarily he is not — thoroughly master of existing forms of life. But even in this case the historic method is far preferable.

Indeed, a few elementary forms need first to be plainly presented. From these there may be a gradual advance to a greater number, and, if the facts allow, to those of a more complicated structure. The advance is naturally made from the more rudimentary to those of greater diversity; from earlier and simpler to later and more complex; from forms, in short, with which the student at first readily associates the great types of the animal kingdom, to others which he may for the time look upon as representative of particular classes, orders, families, and genera; from the trunk, so to speak, of organic existence, to the branches, thence to the twigs and leaves and flowers, and all in the historic order of development.

To put the matter in a more specific shape, the student in geology should start, so far as may be, from the beginning. Going back, with whatever knowledge he has of the present, to the earliest fossiliferous rocks, he fitly commences his study on a few representatives of two, or at the most three, great divisions of the animal kingdom, and thus is not perplexed, much less overwhelmed, by a great multiplicity of

forms. From these he slowly, but steadily, advances to higher and later groups of rocks, thus to other slightly varying phases of organization, thence to larger and newer circles of existence, and so onward, from gradation to gradation, until he comes at last in some good measure to understand the meaning of things as they now are. This should be substantially true of his course, whether he take up the *composition* of the several different beds as superimposed in space; the *disposition* of strata as marks of orderly movements in time; their *formation* as effected by dynamic processes working under varying conditions; or, finally, *organization* in its progressive steps, as witnessed by distinctive cycles of vegetable and animal life in the rocky record of the ages.

Now, the book under consideration is exactly the contrary of this in its plan. By Sir Charles's so-called method no progress of this kind is made, no such view of creation secured. The movement is backward—the very opposite of that implied in the evolution of a principle or the unfolding of an orderly plan. It is as if one should write the history of the United States upon a regressive scheme, beginning with the last acts of to-day, or perhaps with the close of the “great conflict,” describing, first, the surrender of General Lee, then each event that preceded it, and so, step by step, backward through the war, through the various presidential administrations, the revolutionary struggle for independence, the provincial period, and the colonial—all this, while no given point of more recent date can be adequately understood without a knowledge of much, if not of all, that went before it. To put it in a word, it is like telling a story backward.

Viewed in this light, the unfitness of the method must be evident, even to such as have never given the subject a moment's thought. To others its awkwardness may seem more striking, if looked at under some other aspects. Its futility will be, perhaps, more specially apparent to many, when it is remembered that existing species are not in any peculiar sense the standard by which nature is to be judged;

that they are not distinctively the types of creation, as some have seemed to suppose ; that the more primitive forms, and thus in one sense the more typical examples, from which the existing are variations, belong to the past ; that, while a special plan is revealed in each portion of animated existence, the earliest are the primal manifestations of the original archetype ; that thus the present phases of life can be adequately understood only in the light of those that preceded ; that the most ancient — for instance, the Taconic and Silurian, — being far fewer in number, are more readily mastered ; and that, therefore, as always contemplated in connection with what now exists, they are the proper material on which a well-organized and wisely-devised method of study suggests that the student should begin his work.

If such be the case, Mr. Lyell's mode of proceeding is certainly unnatural. Indeed, it seems to be just the reverse of that of nature. It opens not with the premises, but with the conclusion ; it sets out from the myriad forms of the present, instead of starting from the primordial with the aim of seizing the plan of creation in its more rudimentary aspects, and tracing it onward and upward through all the advancing stages of its evolution. It is not, therefore, at once simple and progressive, as the true unfolding of a subject should be. While it is in a sense orderly, it is not really methodical, because it is not according to the method of nature, which is ever historical. Genuine method, as even the very word implies, is a movement forward — a way onward from point to point ; thus it is an orderly advance by progressive stages.

And still Sir Charles speaks of his so-called method as "chronological." "I proceed," he says (p. 100), "to treat of the aqueous or fossiliferous rocks, considered in chronological order." Of course, in one view, his scheme is chronologic, that is, according to a time order ; but it clearly does not move in the direction in which time flows. Thus, in spite of the forced sense put upon the word, it is really anti-chronologic. Indeed, he elsewhere virtually confesses

this much. In giving the sequence of the formations, in a cut (p. 114), he properly numbers them from below upward, or from older to newer. So, in another place he calls, perhaps inadvertently, the order of superposition that of chronological succession. This, to him probably unconscious, play on the word reveals the incongruity of his method. That he sometimes dimly felt its unnaturalness seems evident from his occasional apologies for it. And yet, as should be freely admitted, there are some advantages in studying the recent and newer formations before the older. Indeed, there ought always to be a comparison of the ancient with the modern. So a regressive movement is occasionally desirable in a work of this kind; still, it should be one occupying not the main body of the treatise, but at most only a few chapters, or resorted to incidentally to prepare the way for a survey of the past in the light of the present, and thus for an intelligent and systematic advance from the introduction of life upon the planet through all the varying stages of its progress. Thus contemplated, the historic method is thought to have advantages immeasurably overbalancing its contingent defects—advantages rendering it overwhelmingly superior to that of Sir Charles, and which, all things considered, leave little room for doubt that it should be adopted in a work for beginners.

But let us turn to another point. While the method, as a whole, is thus vicious, it may be presumed that the *composition of the book* is faultless. In such a volume, one naturally looks for a clear, good style. A few specimens, selected, if not at random, at least from a large number of others, will show whether the "Student's Elements" can serve as a model of English "pure and undefiled." One may read, p. 78, "The *excavation* of both the valley and quarry *have* been gradual"; p. 102, "*None* [i.e. no one] *have* ever reappeared"; p. 118, "The *whole* of them *were* confounded"; and p. 129, "The *greater number* of each kind *are* obviously fashioned." If it be objected that these ungrammatical sentences would be, in whole or in part, awkward with their

verbs in the singular, I reply : They may, as some of them certainly should, be recast — transformed, if possible, into correct, if not into elegant, English.

Take another sentence (p. 27) : “ Great surprise was *created* some years *since* by the discovery that a certain kind of siliceous stone *was* composed of millions of the remains of organic beings.” Surprise “ was occasioned,” or “ experienced,” would be better than “ was created.” After the closest scrutiny, it still remains doubtful just what “ some years since ” properly means, while “ some years ago ” is perfectly plain. So the expression “ *was* entirely composed,” indicating a fact of unvarying import as to time, should obviously be, “ *is* entirely composed.” There is a similar example on page 144 : “ Geologists were not long in seeing that the boulder formation was [properly, *is*] characteristic of high latitudes.”

A clause (p. 29) reads, “ The rocks scarcely contain any *other* fossils *except* snail-shells.” It is difficult, in this instance, to see the use of “ other.” If it be retained, “ than ” should be substituted for “ except.” One might drop both words, and use simply “ but.”

In respect to the sentence (p. 47), “ It appeared clear that certain spaces had been “ *alternately sea, then land, then estuary,* ” it may be asked what the words “ alternately sea ” mean ; also, what is the relation expressed by “ alternately,” “ then,” “ then,” — “ alternately ” supposing *two* points of contrast, and seldom three.

Such language as the following is of frequent occurrence : p. 99, “ Which of the two may be the *oldest* ; and p. 106, “ The chalk was the *oldest* of the two formations,” — cases in which the merest tyro in grammar would be expected to use the comparative, and not, like Sir Charles, the superlative.

On p. 91, there occurs : “ The quantity of detritus *now being distributed* would cause an elevation,” — strange language for an Englishman ; English critics having condemned expressions like “ now being distributed ” as outlandish

Americanisms. Certainly, "now in process of distribution" is more elegant.

I find on p. 110, "This great work and *those* [viz. "great work"] of A. Brongniart show" — a construction which, if occasionally pardonable in oral discourse, I trust has not yet become classic.

"Palaeontology" and its derivatives, with some other kindred words, are spelled sometimes with a diphthong, sometimes with a simple "e." Uniformity is surely preferable.

"Density" is used (p. 87) — probably from sheer carelessness — in the expressions, "maximum density" and "enormous density," the meaning clearly being not density, but a "thickness of 40,000 feet."

A specimen of carelessness in another direction occurs (p. 118): "Their geographical area [i.e. that of the Tertiary series] being usually small compared to the Secondary formations." These words, as comparing incompatibles, are, of course, sheer nonsense as they stand; the meaning evidently is "small compared with that [viz. the area] of the Secondary formations."

This example reminds me that "compare," "correspond," "conform," "parallel," and some other kindred words, with their derivatives, constantly occur with the particle "to," while their etymology suggests that "with" is their proper, and should be their usual, accompaniment.

The author (p. 118) speaks of "successive sets of strata" "lying one upon the other." "One upon the other" implies that there were only two, and would be proper if that were the case. There being more than two, he should have written "one upon another."

It may be remarked that the sense is often obscured by the misplacement of adverbs and adverbial phrases. These are almost invariably made to separate the compound auxiliaries, when they would more elegantly, not to say more consistently with the sense, follow them so as directly to modify the principal verb. For instance, in the clauses (p. 210), "The Miocene may *best* be studied," (p. 215)

“Cones have *recently* been obtained,” and (p. 477) “Might *fairly* have inferred,” what should “best,” “recently,” and “fairly,” specially modify—the auxiliaries, or the main verbs? If the latter, they ought by all means to be placed as near them as the other words will consistently allow. In a few instances, over against hundreds of misplacement, the collocation of the words is right; e.g. (p. 445) “It has been *already* stated.”

Instead of “some few,” which frequently occurs, as on p. 180, “*some few* of those eleven shells,” “a few” would certainly be in better taste, if not more correct.

An instance of affected correctness, which is, after all, inaccurate, may be seen (p. 177), “At the close of the Newer Pliocene, and in the Post-Pliocene periods.” Now “Newer Pliocene” and “Post-Pliocene,” in the sense of the passage, cannot agree with “periods.” Rarely, if ever, does an additional adjective necessarily require a change from singular to plural in the noun described. Instead of this, sound criticism and exactness of thought suggest that the substantive is understood with the first adjective, it being expressed with the second. Take, as a good instance, “the Old [Testament] and the New Testament,” not “the Old [Testaments] and the New Testaments.” So it is properly, “the Newer Pliocene [period] and the Post-Pliocene period.”

As is frequent in England, Sir Charles often uses the “present-perfect” tense when the sense and strict propriety require the indefinite past; e.g. (p. 144) “Erratics have not unfrequently travelled hundreds of miles from the parent rocks from which they have evidently been detached.” While the tense of the first verb is perhaps defensible, that of the second is certainly incorrect. The last clause should clearly stand: “From which they were [indefinite past time] evidently detached.”

The author almost invariably employs the indicative form of the verb after “if.” It gives me great pleasure to cite one out of a few instances noted in which he more elegantly uses the subjunctive mood. Speaking of the tests of age, he

says (p. 505) "If a volcanic rock *rest* upon an aqueous deposit," it is still the newer mass.

In the clause (p. 78), "It could be assumed that *both* the upward *or* downward movement are everywhere uniform," "both" and "or" are brought into strange correlation.

Instances of the inelegant use of "where," for "in which," are frequent; e.g. (p. 122) "Cases will occur *where* it may be scarcely possible to draw the boundary line between the Recent and Post-Pliocene deposits."

In the sentence (p. 133), "Schmerling examined forty cases near Liège, and found *in all of them* the remains of the same fauna," good sense, not to say grammatical propriety, demands that "found in all of them" be changed to "in them all found."

I had almost forgotten to remark that words ending in "ward," and thus denoting tendency, as "toward," "forward," "southward," and the like, are almost invariably burdened with a superfluous "s." This corruption, so contrary to etymology and the sound usage of the classic writers in English literature, I am sorry to say has of late crept into use, and is now countenanced by many of the so-called standard dictionaries. The inappropriateness of this form becomes evident, if one of the words, e.g. "southward," be made an adjective, as it often is when preceded by an article. Thus Sir Charles somewhere says, "the southward [not southwards] inclination of the country."

This reminds me of an equivocal use of "upward" on p. 146: "These angular blocks have been [were] brought for a distance of fifty miles and upwards [upward]." Is it meant by "and upward" that they were carried "to a higher level," or "more than fifty miles"? If the latter, as seems evident from the context, the words might have stood, "They were borne fifty miles or [and] more."

On p. 516, I find the prim and by no means well-authorized form "firstly," "secondly," for "first," "secondly," etc.

After the mention of certain features in a formation, it is said (p. 576), "*The same* phenomena are also repeated in

the beds below," by which is probably meant "kindred" or "similar," but not "the same."

In the sentence (p. 571), "Clay, marl, etc often contain a considerable proportion of alkali, so much so frequently to make them unfit to be burnt into brick," it may be asked what is the use of the second "so." If one attempt to complete the clause, he will see the incongruity. It is, in fact, one of the fag-ends so frequent in Sir Charles's style.

Without citing any more examples illustrative of the composition of this new volume, I think all will agree that it has many faults of style which should never mar a manual for beginners.

I proceed, next, to notice the *distribution of the matter*, having reference to classification generally. In the book now occupying attention, there is less excellence in this direction than might be at first supposed. While there is considerable formal regularity, some show of a systematic arrangement, the classification seems to be defective, as not resting on principles or founded in a profound apprehension of the nature of the things discussed.

Take, as an instance, the author's general, and it should be a fundamental, division of the rocks of the globe. He distributes them all into four great classes, viz. Aqueous, Volcanic, Plutonic, and Metamorphic. Now, as he professes to divide them "according to their origin," it may be fairly asked, whether, in this light, the so-called metamorphic rocks can be properly regarded as co-ordinate with the other divisions named. If the foliated or schistose masses were, as he supposes, really formed as sedimentary beds, they belong to the aqueous series, and thus are not an independent and comprehensive group. If crystallization was superinduced, as he holds, *after their formation*, another mark of subordination is added. Thus they constitute, on the one hand, only a part of the aqueous series; meanwhile, the transformation they have undergone, which is regarded as their characteristic feature, is certainly incidental. The

metamorphic rocks, therefore, according to the description given, must be looked at as a subordinate, and not, as Sir Charles, who named them, would have people suppose, a grand division. Indeed, metamorphic agency, while important in its place, is not sufficient to constitute a primary class; for it is, at the best, local and exceptional in its working, and cannot furnish characters more general than itself. Whether these rocks, in whole or in part, may not compose a grand division, is an entirely different point, which I do not propose here to discuss.

Another question: Are "granite, gneiss, and the other crystalline formations" to be classed together, as they are on p. 10? Surely not on the ground that, as suggested, the first is plutonic, while the latter are metamorphic; for these words, as ordinarily used by the author, suggest marked differences, and apply to rocks which were, according to the terms employed, of very unlike origin. If it be said that their origin may have been the same, I reply, (1) that is the point to be proved, and (2) the supposition ignores Sir Charles's distinction between plutonic and metamorphic. No more are they to be reduced to one division on the supposition that both are "nether formed"; since no positive proof has been yet given that all gneiss, for instance, is hypogenic. Indeed, for anything that has yet appeared to the contrary, the great mass of foliated rocks, with the exception of certain metamorphic aqueous beds lying in troughs, etc., may have been formed *before* any sedimentary strata were deposited. Accordingly, if they be classed together, it must no doubt virtually be from the fact that all are crystalline. As to how they took this form, whether under kindred or diverse conditions, and thus whether they should be referred to one circle, or to different series, I may have more to say hereafter.

If an additional instance of Sir Charles's infelicitous and, shall I say, shallow classification were needed, I might cite his major divisions of the Tertiary; viz. Eocene, Miocene, and Pliocene. These terms, as most know, were originally

given to particular beds, on the hypothesis that each contains a certain percentage of living species. It is now, however, the opinion of some of the best Palaeontologists that no fossil, for instance of the Eocene, belongs to a species still existent. But the error thus implied is not the worst feature in this system of classification: every new study of given beds, or series of strata, is liable and likely to reveal a different percentage of living forms, to say nothing of species once supposed to be now living, and thus from time to time to necessitate readjustments of their places in Sir Charles's numerico-Tertiary scale. As an instance, take those beds which Professor Beyrich has termed "Oligocene." These, in the last edition of the "Elements," Sir Charles arranged as Eocene; in the present volume, however, they are called Lower Miocene, as they must be, in order to sustain certain points in the proposed theory. Thus the tendency is to vacillation, not to stability.

I may add that, evidently with a like purpose, many other changes have been made. For instance, of beds of drift, some are arranged in the Tertiary, while others on the same island, and substantially identical in age, are placed in the Post-Tertiary; all this, while the demarcation between the Tertiary and the Post-Tertiary is far more distinctly marked in nature than that between the Miocene and Pliocene. Surely, such changes are a mending of matters with a vengeance, and for the most part, so far as I can see, simply with the aim to keep that time-honored percentage classification in harmony with new discoveries. Of course, as must be evident to all, a general division of the Tertiary into Lower, Middle, and Upper, and the latter into subordinate stages,—the beds being then arranged simply as they occur in nature;—would save much of this tinkering, and perhaps be equally favorable to a healthy advance in geologic knowledge. It must thus be clear that Sir Charles's classification is signally inadequate; it being neither fundamental nor really systematic, and therefore failing either to represent the true system of nature, or to serve as a matter of permanent convenience in aiding the memory of the student.

But it is time to consider a more special feature of the volume, viz. the *definition of terms*. This is a point of primary importance, particularly in a manual for beginners. Let us, accordingly, look at some of Sir Charles's work in this direction, and first at his definition of a fossil. "By fossil," he says (p. 5), "is meant any body, or the traces of the existence of any body, whether animal or vegetable, which has been buried in the earth by natural causes." The word as used at an early day included minerals, no less than organic forms, and thus meant, as etymologically defined, something dug up, anything excavated; though, according to its more recent employment, it mainly comprises only vegetable and animal remains. Of course, the implication is that the thing was once covered, no matter how. The author, in thus disregarding the etymon and the historic derivation of the term, for the most part merely gives an inference from the true definition. His statement, therefore, lacks, on the one side, breadth, for it fails to cover the whole ground; and on the other, precision, since it contains irrelevant matter. Comprehensiveness and exactness in this case, while not detracting in the least, but ministering to the student's information, would have added to his interest and the distinctness of impression.

In laying out his subject, the author (p. 11) defines the fossiliferous rocks as of three sorts,— "the siliceous, the argillaceous, and the calcareous." Instead of "siliceous," the word "arenaceous," as more comprehensive, might be used with manifest advantage, without any sacrifice of exactness; the siliceous beds being properly regarded as simply a variety of the arenaceous. While this may seem to some an unimportant distinction, it yet, when properly put, teaches the pupil to think according to the logic of nature.

This suggests another example of somewhat similar character. "Conglomerate" and "pudding-stone" (p. 12) are spoken of as synonymous. With a view to convenience, as when we wish to designate two or more kinds by one comprehensive term, and to exactness, if we would name one sort

by a single word, "conglomerate" may be well retained as a generic designation, "pudding-stone" and "breccia" indicating two varieties.

From the definitions of "conformability" and "unconformability of strata" (see pp. 15, 71, 72), it is evident that the author confines these terms to the agreement or disagreement of beds in respect to dip. Had he defined the words more comprehensively, as Jukes has done, so as to be able to speak of strata as conformable or discordant as to (1) dip, (2) strike, and (3) succession in time, he would have saved himself and his readers many cumbersome circumlocutions. He might perhaps with advantage even go further, describing beds as conformable or discordant in regard to mineral constituents and organic remains, thus bringing out even in definitions, the harmonies, circles within circles, which meet us everywhere in nature.

On p. 14, he mentions "magnesian limestone" and "dolomite" as synonymous; and yet, in the same paragraph, he speaks of the latter as a variety of the former. With a little more care, he might have increased at once the comprehensiveness and the exactness of his language, avoided the seeming inconsistency, and thus have aided the student.

Take another example: "Denudation," it is said (p. 73), "is the removal of solid matter by water in motion, whether of rivers or of the waves and currents of the sea, and the consequent laying bare of some inferior rock." So far as the definition goes, the student is not told that a particle of the surface of the earth beyond that occupied by seas and rivers is affected by the process. Such a removal of a portion of solid matter, in the bed of a stream or of the ocean, by water in motion, as lays bare a lower rock, might constitute, according to the terms, the whole of denudation. But, waiving this point, one cannot fail to see that the definition is, even on other grounds, very narrow and restricted. When specifications are limited, as in the present case, by general terms, such special points as are not expressly named as free from the limitation are of course excluded. In this

instance, there is an exclusion of one of the more important, if not of the most prominent agencies in the erosion of the earth's surface, viz. ice. So atmospheric action, in connection with moisture, heat, and cold, to mention no other omissions, seems to be overlooked, indeed virtually excluded, by the definition. It should be added that subsequent remarks show that the author recognizes the atmosphere as an eroding power; but in showing this they also evince the incongruity of the definition with what follows. Had he simply said: Denudation is mainly due to the action of water in some of its forms, the definition, while by no means exhaustive, would have been far more comprehensive. Meanwhile, it would have allowed exactness in specifications, and thus more harmony between the definition and subsequent statements.

The expression "fresh-water algae" occurs on p. 28 — an expression which, while not a definition, reveals the need of exact defining. While aware of the loose way in which "alga" is often used, even by many botanists, I may add that it originally denoted, and properly designates, a sea-weed; while "conferva," which was primarily applied to certain fresh-water plants of low grade, should be with strict exactness confined to this group. Thus the forms which Sir Charles had in mind when he wrote "fresh-water algae," and which he might have designated in a word, are properly termed "confervae," and not "fresh-water sea-weeds."

This reminds me of an additional instance of somewhat kindred character. Explaining the position of the beds of an anticlinal axis, the author says (p. 62): "They slope upwards [upward], forming an arch." Now, what he means is of course clear from the connection; but this does not excuse, much less vindicate, the self-contradictory expression. In strict propriety, "to slope" is to slant, not simply in any direction, but downward. Hence the combination "slope upward" is as incongruous as the expressions "descend to heaven," "ascend to hades"; indeed, it is more so. Such unions of incompatibles would be cheerfully overlooked in

playful conversation, but their use in sober earnest, and in what is intended to be a standard work, is a very different matter. This example, especially as taken in connection with the peculiar method of the "Elements," vividly recalls the expressive paradox implied in the old Greek word ἀποκατάστασις, a word which has been wittily, if not happily rendered, "progress backward"; with this difference, however, in favor of the ancient Grecian astronomers, — they had an established fact to announce — a fact to which the term very well answered, viz. a return of the heavenly bodies, after a given period, to their recognized point of departure; while Sir Charles's language, in this instance, whatever may be said of his "progress-backward" method, is clearly due to sheer carelessness.

As an instance of a loose, if not of the equivocal, use of language, take the following rule (p. 12): "Pure siliceous rocks may be known by not effervescing when a drop of nitric, sulphuric, or other acid is applied to them, or by the grains not being readily scratched or broken by ordinary pressure." According to the first test, the student may set down sulphate of lime, or gypsum, as pure silex, since it does not effervesce on the application of an acid. Simply following the second test, he might take a specimen of almost any of the so-called igneous rocks for pure silex. Perhaps, however, the author means that a rock may be known to be silex, if it do not yield *either* to the first *or* to the second test. In this case, his use of "or" is ambiguous. But, even when thus interpreted, the rule is altogether inadequate; for varieties of feldspar fail *either* to effervesce under the proposed conditions, *or* to be readily scratched or broken under ordinary pressure. Surely, such a rule from one who, like Sir Charles, is an adept in the determination of metamorphic rocks, is a marvel; and there need be no surprise if a student under such guidance make strange work in his identification of minerals.

In this place, perhaps, I should quote what the author says (p. 86) specially in regard to the student: "It is par-

ticularly his business to understand." Of course, this sage remark will be generally admitted, but on the ground that the statements made for the enlightening of the student are in good English, and that the rules and definitions are to some small extent free from ambiguity, not to say comprehensive and exact. Surely, comprehensiveness is necessary in definitions, in order that they may be, so far as possible, exponential of great principles, and that every particular legitimately within their scope may be included, certainly not excluded by infelicitous verbal improprieties. There is also need of exactness, that there may be no doubt precisely what is meant; the words just answering to the things intended. The few specimens cited will show that, even in this direction, the volume under review has defects and faults which ought never to appear in a work intended as the student's *vade-mecum* in geology.

Advancing to another point, I may notice Sir Charles's *delineation of facts*. In his exhibition of the European Tertiary and Post-Tertiary beds he evinces a considerable degree of explicitness and accuracy; for with these he is probably better acquainted than with any other part of the geologic record. And yet, in examining what he has to say even of these formations, on points respecting which I specially desire light, I fail to find it. Compared with his presentation, the minuteness of knowledge revealed by Carl Mayer in every utterance he makes on the Tertiary is marvellous, and, amidst the platitudes of the day, strangely refreshing.

As illustrative of the author's lack of explicitness, even in regard to the more recent formations, take a single point: On p. 157, he speaks of "marine shells having been found in glacial drift." What he thus calls "glacial drift" is not drift, in the strict sense of the term, but, according to his own showing, a stratified marine deposit, which, as having a very different form, and thus probably an unlike origin, ought to be everywhere closely discriminated from typical drift. Such want of precise discrimination is likely to lead

— as I well know it has led — many a beginner to suppose, what I believe occurs only in rare cases easily explained, that marine shells are found in the typical deposit of the drift period. The matter is no doubt left in this shape because of an early bias unconsciously fostered by Sir Charles in his effort to explain all the drift phenomena in consonance with his favorite iceberg hypothesis.

I may cite an additional instance, which, among other points, is perhaps indicative of the author's limited acquaintance with the exact history of the progress of geologic inquiry. He says. (p. 160): "It was first pointed out by Professor Ramsay, in 1862, that lakes are exceedingly numerous in those countries where [in which] signs of ice-action abound." Unless he mean that Mr. Ramsay, in pointing out this fact several times, did it first in 1862, the statement is erroneous. In reality, the fact was distinctly indicated by Professor Emmons some twenty years earlier, and repeated by him in 1858. The sentence, however, is ambiguous; still, the meaning probably is: Professor Ramsay was the first to point out, in 1862, etc. Without presuming that Professor Ramsay borrowed, I wish simply to suggest that he was anticipated by Dr. Emmons, and that Sir Charles is doubtless ignorant of the fact — a point easily understood by such as are intimately acquainted with the history of American Geology.

An example of the author's lack of accuracy, both from ambiguity and from defective statements, I take from p. 2. Speaking of the earth's crust, which he seems to divide into two parts, (1) what "is accessible to human observation," and (2) "the whole of that outer covering of the planet on which we are enabled to reason by observations made at or near the surface," he proceeds to say: "These reasonings may extend to a depth of several miles, say ten miles." Now, according to the language used, he appears to affirm that some ten miles embrace all those parts of the earth's crust respecting which one may reason from observations made at or near the surface. But, waiving this ambiguity,

and supposing that he means that the thickness of what is accessible to human observation is about ten miles, one may still demur, and think that the statement is made from very old estimates, and not in the light of present knowledge. The fossiliferous strata must have a maximum thickness of some one hundred thousand feet. To these should be added the foliated series of beds which clearly underlie the primordial formations, and are probably about thirty thousand feet in thickness. Thus the rocks that are open to the eye of man, to say nothing of those that may be reasoned about from observations made on these superficial masses, are, no doubt, from twenty to twenty-five miles in thickness — an estimate suited to remind the student that there is still room for original research. While an under-estimate may be in some respects better than its opposite, it still involves an injustice to the science; the exact truth being what is wanted, and what simple justice demands.

As an instance of a presentation of facts which is explicit, and in one sense correct, while it yet gives a very exaggerated impression as a whole, take what is said, in several editions of the "Elements," of the agency of icebergs. Particular cases, most of which are comparatively exceptional, are given with such a degree of explicitness, while so little is presented in a favorable light in regard to certain other instrumentalities, that false inferences and implications are almost unavoidably forced upon the mind of the student — implications and inferences calculated to impart a very distorted view of the actual state of things at given times in the earth's history. This being a kind of special pleading in which Sir Charles occasionally indulges, and by means of which some of his writings are likely to suggest a very one-sided aspect of nature, they are certainly not the best suited to go into the hands of beginners. For them a narration of facts should be at once explicit and accurate, and thus evenly balanced — explicit, it being duly spread out that they may get a distinct impression; accurate, there being an exhibition of the subject as it is, without defect or exaggeration, that their apprehension of it may be true.

To proceed: I would now briefly refer to Sir Charles's *exposition of progress*. A word touching what he says of the older fossiliferous rocks must suffice in that direction: The Obolus sandstone of Russia, sometimes known as the Ungulite grit of Pander, — which, indeed, with all the sedimentary beds around St. Petersburg lying beneath the Orthoceratite limestone, should be provisionally recognized as Primordial, — the author places, seemingly without a question, in the Lower Silurian. But when he treats of the older Palaeozoic rocks of England, which are rapidly gaining prominence in public estimation, he arranges the Tremadoc slates as Cambrian, though as a series of transition beds they appear to be as closely allied to the Silurian as to the Primordial.

While Sir Charles's exposition of what is known of these older rocks suggests many points of stricture, I must pass them all by, in order to notice his treatment of some of the more recent strata with which he is more especially at home. In arranging the Tertiary and the Post-Tertiary beds, he formerly placed the Drift and the immediately overlying stratified deposits, in the Tertiary, calling them "Newer Pliocene," or "Plistocene." Other strata, of more recent date, he termed "Post-Pliocene," and arranged them as a part of the Post-Tertiary, just above the Plistocene; the deposits known as Recent succeeding them. With the advance of knowledge it has become evident that the Drift, or Plistocene, is also strictly Post-Tertiary, and thus in a marked manner separated from the Older Pliocene. Now, to sunder the Newer Pliocene (or Plistocene) from the Older Pliocene, calling it Post-Tertiary, to be followed by the Post-Pliocene and Recent, would be very awkward, not to say, strangely unfit. Something, therefore, is to be done; the advance of science cannot be safely overlooked; but then the old percentage theory must be kept up. With a view to these ends, and as a sort of compromise, the author arranges the greater part of the Drift proper, with the overlying stratified beds, in what he now calls the Post-Pliocene. Reserving the remainder of the typical Drift, with its superimposed strata,

which in given localities seem to have a little greater percentage of extinct forms, he places it, with a few other beds, in what he designates in the "Student's Elements" as Newer Pliocene, silently dropping the "Plistocene" altogether. But this is not all. Having appropriated the term Post-Pliocene for the most part to the Drift and its associate stratified beds, he seems largely to ignore the time which really intervened between what he formerly called the Plistocene and the Recent. And this interval, which, in consonance with Sir Charles's terminology, I have sometimes called the Holocene, is not a mere cipher. It properly consists of two parts, viz. the Marl Period and the Peat Period, each of which sober geologists have estimated as at least twenty-five thousand years in length.

Now, whether such an exposition of progress be ingenuous, or whether a proceeding of this kind be trustworthy in its results, I leave others to judge. It is certainly fashionable in some quarters. The student, meanwhile, needs in a text-book, not the whims of a man, or the fashions of the day, but that on which he may rely.

A few words may be next devoted to Sir Charles's *recognition of investigators*. Speaking of the studies of Cuvier and Brongniart on the Tertiary beds in the neighborhood of Paris, which were published in 1810 the author says (p. 117), "Strata were soon afterwards [afterward] brought to light in the vicinity of London, and in Hampshire, which were justly inferred by Mr. T. Webster to be of the same age as those of Paris, because" of the marked similarity of the fossil shells. While this statement is in one aspect true, it is none the less unjust to previous laborers. Brander long before "brought to light" fossils of the Lower Tertiary "in Hampshire," which he deposited in the British Museum. Of these, in 1766, he published descriptions made by Solander, in a volume illustrated with excellent figures, and for the most part comparing favorably with work done to-day. As is clear from his preface, two remarkable results were reached, viz. (1) that most of the fossils differ from existing species,

and (2) that the representatives of the few forms which closely resemble species now living are only found in warmer latitudes. Thus Brander, while he by no means fully anticipated the grand generalizations of Cuvier, yet evinced extraordinary insight, and did much to prepare the way in England for the recognition of the light shortly to dawn. On these accounts, therefore, he was eminently deserving of credit, in some respects more so than any other man who has had to do with the English Tertiaries; and yet he receives in the "Student's Elements" no mention. Such failure to do justice to an Englishman not widely recognized would not be surprising in a foreigner; but in Sir Charles, who certainly knows Brander, or at least of him, and is supposed to be oracular in regard to the English Tertiaries, it is strangely surprising.

A second instance, out of many that might be cited, must suffice for the illustration of the point under consideration. If students simply read what is said of glacier agency in the eleventh chapter of this volume, and especially the reference (p. 145), "*I have described elsewhere*" (Principles, etc.), taken in connection with the fact that express mention is made of only Vinetz and Charpentier as early observers of the Swiss glaciers, they can hardly fail to get the impression that Sir Charles was foremost in noticing the evidences of glaciation, and that the main credit of the glacier-theory is due to him. Again, if they merely read what he says of the glaciation of Scotland, they will naturally infer that Professor Jamieson was the first to find evidence of the former existence of glaciers in that region. Now, the fact is that Professor Agassiz, having followed up the inquiries of the few earlier workers by critical investigations of his own on the glaciers of Switzerland, was the first to make the grand generalization that drift phenomena generally are due to the agency of continental ice-masses; that, in 1840, he was the first to extend this generalization to the British Isles; and that, during the same year, he designated the period of drift as the Ice or Glacial period. It is also a fact that Sir Charles,

having already proposed his iceberg hypothesis, probably did more than any other man in England, when the glacier-view was propounded, to prevent its immediate recognition, and continued by his influence and books to delay its acceptance for years. It is likewise a fact that the iceberg-hypothesis having proved inadequate as an explanation of the phenomena in question, the glacier-theory has gradually come to be more and more widely recognized as true, and that Sir Charles now appears indirectly as its advocate, — indeed, as one might readily infer, as virtually its original propounder. This neglect to recognize the labors of others, this underhand appropriation of credit not his due, this withholding from his peers the meet reward of merit, this speaking of the glacier-theory as if he had always held to it, this silently ceasing to advocate the iceberg-hypothesis as the main agency concerned in the formation of drift, certainly must be regarded, when duly weighed, as anything but fair and just. How much nobler and more honest would his part appear, if he cordially put things as they are; how much better it would be for himself, the student, and all concerned.

Taking another step forward, I would inquire how far Sir Charles has exercised a proper *discrimination of novelties*. A single instance may serve to show the character of the book in this regard. On p. 476, it is said: "The discovery of the Laurentian Eozoön, in Canada, discountenances such views" as are implied in M. Barrande's "term Primordial." This is so, if the Eozoön be organic. First observed in limestone of the foliated series from the Grand Calumet on the River Ottawa, it has been since found in rocks supposed to be of the same age in Bavaria, Bohemia, and Massachusetts. Citing as evidence several distinguished men who had to do with the discovery and examination of this curious form, the author says (p. 476): "On this oldest of known organic remains Dr. Dawson has conferred the name Eozoön Canadense." Now, if mere authority be appealed to in regard to the more ancient Palaeozoic organisms, surely few *savants* can stand as an offset to M. Barrande. But this

may not have been the aim. Without, therefore, questioning in the least the scientific insight of the persons named, or discussing the mode in which they have presented the supposed discovery, it may be remarked that Sir Charles distinctly assumes as facts, (1) that the form is *organic*, (2) that it is *known* to be such, and (3), by implication, that the rocks in which it occurs are *metamorphic, fossiliferous beds*. Surely these are wonderful assumptions, and seem to preclude all necessity for any further examination of points which I had supposed could be only settled as the result of patient, profound, and critical investigation. Waiving, then, all special consideration of these noteworthy assumptions, I have simply to add: In due time, I propose to show that genuine specimens of Eozoön — those recognized as such by the one who described them — are from non-sedimentary rocks — from rocks which were deposited in a veinlike form, and really constitute vein-stones. It is accordingly probable, if it cannot be demonstrated beyond a question, that this "oldest of organic remains" belongs to the departments of Chemistry and Crystallography, and therefore that it may be newer than some have supposed. Thus, in a word, I find not only probability, but even positive evidence, against the assumed organic nature of the so-called Eozoön. The form, while it resembles certain animal structures, as Dentrites simulate vegetable organization, seems clearly to be the result of chemico-crystalline agency. Without further comments on the matter, I may add that in some respects it is certainly singular that a man so cautious as Sir Charles should have committed himself, unreservedly and without critical examination, to what has ever appeared to me a wild vagary. The recent discovery that the particular beds from which authentic specimens of the supposed Nummulite were derived, are wholly of a non-sedimentary character, only renders the marvel still more strange.

It may be proper, next, to notice the author's *discussion of hypotheses*. As suited to reveal how far he is discriminating and logical in such discussions, his treatment of his pet child,

the doctrine of Metamorphism, may be passed in review. The hypothesis that the foliated rocks are sedimentary beds which have been changed by heat, and so disguised as scarcely to show their aqueous origin, has been very widely indorsed; and yet it by no means rests on so wide an induction of facts as is sometimes supposed, or has in its favor that logical evidence which ought to be required in the case of every generally accredited doctrine. The difficulty, perhaps, lies in the subject itself, and not merely in Sir Charles's treatment of it. Still, if this be so, it should be distinctly understood. The argument is substantially this: "Plumbago, associated with hypogeous rock, *may* have been coal." Since it has been *already shown* that plumbago may have been coal changed by heat, therefore it *is* metamorphic coal. Or the case may be put in this more specific form: *Certain* sedimentary beds have been in *given parts* clearly changed by intrusive masses, or in some way so transformed by heat as to resemble the ordinary Schistose rocks. Thus far very well; for the position is according to fact. But again: *Other* beds, in which no unmistakable organic remains, no angular or water-worn pebbles, have been ever found,—which are, in other words, Schistose throughout, and thousands of feet thick,—*may* have been thus changed. This position, also, is legitimate, so long as the word "may" is used to express a mere supposition in aid of inquiry. When, however, "may have been changed" is followed by what is virtually, "*therefore* they were changed," and still more, when it is said, "*therefore all* the foliated rocks are metamorphic, fossiliferous strata," one who does not recognize the legitimacy of that kind of discrimination, and cannot see the force of the logic, must surely be allowed to question the conclusion. Because sedimentary beds have been changed, to a limited extent, by dikes and kindred agencies,—the most marked case respecting which there is certainty being less than a quarter of a mile in thickness,—it clearly does not follow that rocks from twenty to thirty thousand feet thick, and many hundred, if not thousand, miles in

strike, were originally aqueous beds which have been in this wise transformed, especially if they now show no positive sign of such origin.

A few special examples evincing Sir Charles's loose mode of reasoning and illogical statements respecting these rocks may be in point. When he speaks (p. 117) of "organic remains obliterated entirely," the question naturally rises: How does he know this? So, when he says (p. 8), "In some cases, dark limestones, replete with shells and corals, have been turned into white statuary marble, every vestige of the organic bodies having been obliterated," the query might be raised, whether it be meant that the limestones were changed in their whole extent, or only in portions. But, waiving the ambiguity that runs through the sentence, I should like the proof of the last clause. If it be admitted that no organic trace could be found in the metamorphic part, to say nothing of the whole rock, I fail to see any stable foundation on which to build an argument for the previous existence of fossils. Again, though organic forms may be sometimes effaced, it is a well-known fact that limestone may be burnt in a kiln, the included fossils remaining as distinct at the end of the process as they were at the beginning; also, that shells often occur in the changed portions of rocks near dikes, retaining their forms perfectly. Besides, it is difficult to understand how Sir Charles should know of this obliteration, or how he learned that there were originally any "shells and corals" in the changed parts of the rock, unless he were present when the transformation took place; since every vestige of organization was effaced, according to his showing, when the rocks were metamorphosed.

To look at another phase of the matter. The author is much disposed to make the so-called Eozoön and metamorphism prove each other. Speaking of the Laurentian rocks as metamorphic, he says (p. 477): "Even if we had not discovered the Eozoön [since, however, it is discovered, there remains no doubt!] we might fairly have [might have fairly?]

inferred from analogy that as the quartzites were once beds of sand [a point yet to be proved], so the calcareous masses were originally of organic origin" [in other words, contained Eozoön]. Again (p. 558), he refers to the Laurentian rocks as "known fossiliferous strata." How so known? Of course, from the Eozoön. Thus the *assumed* sedimentary origin of the foliated rocks is, in one instance, presumptive proof of the organic nature of the Eozoön; while, on the other hand, the *known organic* character of the latter is taken as irrefragable evidence of the aqueous derivation of the beds in which it occurs.

But, waiving further instances, and granting all that the facts warrant, I would ask, what is proved? Simply this: Aqueous masses have been changed to a distance ordinarily of a few feet, rarely of yards, or rods by intrusive matter. And so much, or whatever more the facts clearly indicate, should be cheerfully granted. When, however, it is inferred that rocks some thirty thousand feet thick, and of almost indefinite range, have been as a rule so changed as now "to contain no distinct fragments of other rocks, whether rounded or angular," and to "be wholly devoid of organic remains" (p. 560), the famous Eozoön alone excepted, it is quite another thing. The overflows of recent lavas are often in beds. In this respect they have something in common, at least in appearance, with sedimentary strata. Shall we, therefore, infer that they are aqueous deposits, and that all the remains of ancient overflows had a like origin, they having in some cases lost their bedding by metamorphic agency? Surely, more evidence than this is needed. We should rightly require a vast amount of proof before calling recent lavas, or similar outbursts of the olden time, metamorphosed sedimentary rocks. It is even so with the vast pile of foliated beds, after we have eliminated all that is foreign. To call that thirty-thousand-foot mass metamorphic aqueous rock, while we have so little evidence to sustain us, is a stretch of the fancy, a tax upon credulity, a straining of proof, which needs to be shown up, if we would not teach

our children that much of our logic is mere jugglery, and what some call scientific discussion only *leger-de-main*—a skilful sleight of hand. To the proposal of such an hypothesis as a point for investigation, to its thorough consideration as a matter placed under review, or to its logical exposition as a thing to be candidly weighed, I do not object. But when it is assumed to be true, and put forth for acceptance with so slight a foundation for its support, I am constrained to demur, and to regard its presentation in such a light as wholly unsuited to grace a Student's Manual of Geology.

But this Article, already too long, must be brought to a close. Though there be other points as much open to criticism as those noticed, they must be waived. Accordingly, taking up the order of Sir Charles, who from the most recent pages of the book of nature proceeds to the earlier, I may now advance from the "finis" to the "title-page." Opposite to this stands the "frontispiece," presumed to be the crowning part of the work, and intended, as I suppose, to give in miniature the grand features of the ages. It has as characteristic of the Palaeozoic times a Trilobite, which, on the whole, does very well; of the Secondary, an Ammonite, which, though evincing progress in inferior and class characters, fails to show the distinctive advance thus far made; while the Tertiary is represented by a Nummulite—a form the position of which in the scale of organization, though still in dispute, is universally admitted to be low; there thus being, according to the scheme adopted, an actual regression as we go up the geologic ladder. Verily, this is "progress backward," in one view the proper title of Sir Charles's work. While these three forms respectively belong to the eras which they are intended to signalize, the Ammonite is certainly not the most significant type of the Mesozoic. So the Nummulite, a non-descript form with which palaeontologists are still unagreed what to do, is anything but a good representative of the third grand step in the onward aeonian movement, which witnessed the introduction of Mammals, at the head of which stands Man, the crowning work of creation. It is almost as if one should

characterize the Augustan period of Roman literature by means of the most illiterate and least known individual of the time. Surely, if we take the frontispiece as a specimen and evidence, we are virtually compelled to infer that the method adopted by the author has vitiated his taste, blunted his finer powers of intuition, and, while certain lower faculties have been sharpened, left him incapable either of appreciating the grand harmonies of the universe, or of duly unfolding and representing the majestic strides taken during the geologic ages.

And yet to judge a man with "final judgment" in this way, or by a single example, is not quite fair; and I would not do it. The impression, while it is certainly made by the book, is still partial; relating not to the whole man, but to particular points which I hope to see amended. Even Homer, as the report is handed down by the prince of Roman critics, was liable sometimes to doze. Possibly, this frontispiece, with some portions of the volume, was conceived in one of Sir Charles's dozing moods. Be this, however, as it may, the faults remain and mar the work. Indeed, the usefulness which might be expected from it, in view of much of its matter and of its many excellences, cannot fail to be greatly lessened by the method chosen; since, in its present form, it is not, what every such work ought in a measure to be, a natural and progressive exhibition at once of the primal revelation and of the gradual unfoldings of the divine plan as witnessed by the geologic record.

A closing explanation is needful, that I may not wrong myself. To write as I have written has caused me pain; for I have been indebted to Sir Charles for many hints, and for not a little instruction, in regard to the structure of the earth's crust. Having principally in view, however, not personal feelings, but the advance of science, as well as its dissemination, and particularly the establishment of sound processes of investigation and wise methods of instruction, I have not known how to deal otherwise than plainly. Upon the excellences of Mr. Lyell's new book I have made few

comments ; for they speak for themselves, and will be lauded by every critic. Of parts which I regard as defective or faulty I have written more at large and freely, "not that I love Caesar less, but Rome more." It is mainly in this direction that good, yea, a constant increase of good, is likely to come. Teachers of geology need to see the defects in existing books and modes of instruction, that they may demand, and so be prepared to find, better ways than they have before known. And then the frank and honest exposure of faults and deficiencies in a work which has many and great excellences may incite and constrain the author, before it is too late, to make improvements which, as tending to the progress and diffusion of wholesome knowledge, and thus to the welfare of the race, will redound, as false praise never can, to his permanent honor and undying glory.

ARTICLE VI.

CHRIST AS A PRACTICAL OBSERVER OF NATURE, PERSONS, AND EVENTS.

BY REV. SELAH MERRILL, SALMON FALLS, N.H.

IN studying the words and discourses of Christ, one cannot but notice that his mind was intensely practical. It is impossible to find in his teachings and conversations a single visionary or dreamy sentiment. Every thought and statement of his is fresh, vigorous, and pointed. There is also in all that he has said, whether in plain instruction, in reproof, or in sympathy, a healthy tone, which commends his sayings to our ideas of every-day life, as well as especially to our minds and hearts. Although a "carpenter" (Mark vi. 3), he was, as we should say, an exceedingly well-informed man on almost all the social and business affairs of the day. His power of observation was very great ; and not less was the skill he had in elevating the humblest employment or