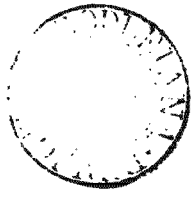
THE

UNIVERSAL REVIEW.

VOL. II.

JULY——DECEMBER.



LONDON:

WM. H. ALLEN & CO., LEADENHALL STREET.

1859.

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me. Thus we get *trev* (cf. the plural *trev-ou*, which Mr. Norris himself quotes in another place), the Welsh *tref*, "hamlet," Old-Irish *treb*, Latin *turba*, Greek τύρβη, English *thorp*, German *dorf*. The *e* of the suffix has come from the *a* of *ma* by progressive assimilation. *En revanche*, we suspect that Mr. Norris is right in reading *dones* "to come," not *doues* i. e. *doves*, Welsh *dyfod*. *Dones* seems exactly the Breton *donet* (Zeuss, G. C. p. 554). If so, Mr. Norris' critic in the *Saturday Review* is here at fault.

We repeat our expression of regret that this notice is not one of unmitigated commendation. But if Mr. Norris will only complete his labours by editing a third volume, giving us therein accurate texts of the Cornish poem on Christ's trial and crucifixion, of Jordan's drama on the creation and flood, and of the two or three other relics of the Cornish language, he will render an important service to the admirers of our early English literature, and will deserve the gratitude of every student of philology.

ILLOGICAL GEOLOGY.

THAT proclivity to generalization which is common in greater or less degree to all minds, and without which, indeed, intelligence cannot exist, has unavoidable inconveniences. While through it alone can truth be reached, yet it almost inevitably betrays into error. But for the tendency to predicate of every other case, that which has been found in the observed cases, there could be no rational thinking; and yet by this indispensable tendency, men are perpetually led to found, on limited experience, propositions which they wrongly assume to be universal and absolute. In one sense, however, this can scarcely be regarded as an evil; for without these premature generalizations the true generalization would never be arrived at. If men waited till all the facts were accumulated before attempting to formulate them, the vast unorganized mass would be unmanageable. Only by provisional grouping can they be brought into such order as to be dealt with; and this provisional grouping is but another name for premature generalization. The history of Astronomy illustrates at once how uniformly men follow this course, and how needful the errors are as steps to truth. The heavenly bodies move round the Earth in circles, said the earliest observers: led partly by the appearances, and partly by their experiences of central motions in terrestrial objects, with which, as all circular, they necessarily classed the celestial ones from lack of any alternative conception. Without this provisional belief, wrong as it was, there could not have been that comparison of positions

which showed that the motions could not be represented by circles, and which led to the theory of epicycles and eccentrics. Only by the aid of this theory, equally untrue, but capable of reducing the apparent motions to a nearer correspondence with the appearances, and so of inducing more accurate observations—only thus did it become possible for Copernicus to show that the heliocentric theory was more feasible than the geocentric theory; or for Kepler to show that the planets move round the sun in ellipses. Yet again, without the aid of this approximate truth discovered by Kepler, Newton could not have established that general law from which it follows, that the motion of a heavenly body round its centre of gravity is not necessarily in an ellipse, but may be in any conic section. And lastly, it was only after the law of gravitation had been verified, that it became possible to determine the actual courses of planets, satellites, and comets; and to prove that, in consequence of perturbations, their orbits always deviate, more or less, from regular curves. Thus, there followed one another five provisional theories of the Solar System, before the sixth and absolutely true theory was reached. In which five provisional theories, each for a time held as final, we may trace both the tendency men have to leap from scanty data to wide generalizations that are either untrue or but partially true; and the necessity which there is for these premature generalizations as steps to the final one.

In the progress of geological speculation the same laws of thought are clearly displayed. We have crude and utterly untrue dogmas for a time passing current as universal truths. We have evidence collected in proof of these dogmas; by and by a colligation of facts in antagonism with them; and eventually a consequent modification. In conformity with this somewhat improved theory, we have a still better classification of facts; a greater power of arranging and interpreting the new facts now rapidly gathered together; and further resulting corrections of theory. Being, as we are at present, in the midst of this process, it is not possible to give an adequate account of the development of geological science as thus regarded: the earlier stages are alone known to us. Not only, however, is it interesting to observe how the more advanced views now received respecting the Earth's history, have been evolved out of the crude views which preceded them; but we shall find it extremely instructive to observe this. We shall see how greatly the old ideas still hold sway, not only over the general mind, but over the minds of geologists themselves. We shall see how the kind of evidence that has in part abolished these old ideas, is still daily accumulating, and threatens to make other like revolutions. In brief, we shall see whereabouts we are in the elaboration of a true theory of the Earth; and, seeing our whereabouts, shall be the better able to judge, among various conflicting opinions, which best conform to the ascertained direction of geological discovery.

It is alike needless and impracticable here to enumerate the many speculations which were in earlier ages propounded by acute men—speculations some of which contained much truth. Falling in unfit times, these speculations did not germinate; and, as not having developed into science, do not concern us. We have nothing to do with ideas, however good, out of which nothing grew; but only with those which gave origin to the system of Geology that now exists. We therefore begin with Werner.

Taking for data the appearances of the Earth's crust in a narrow district of Germany; observing the constant order of superposition of strata, and their respective physical character; Werner drew the inference that strata of like character succeeded each other in like order over the entire surface of the Earth. And seeing, both from the laminated structure of many formations and the organic remains contained in others, that they were sedimentary; he further inferred that these universal strata had been in succession precipitated from a chaotic menstruum which once covered our planet. Thus, on a very incomplete acquaintance with a thousandth part of the Earth's crust, he based a sweeping generalization applying to the whole of it. This Neptunist hypothesis, mark, borne out though it seemed to be by the most conspicuous surrounding facts, was utterly untenable if analyzed. That an universal chaotic menstruum should deposit, one after another, a number of sharply-defined strata, severally differing in composition, is incomprehensible. That the strata so deposited should contain the remains of plants and animals, which could not have lived under the supposed conditions, is still more incomprehensible. Physically absurd, however, as was this hypothesis, it recognized, though under a distorted form, one of the great agencies of geological change—that of water. It also served to express the fact that the formations of the Earth's crust stand in some kind of order. Further, it did a little towards supplying a nomenclature, without which much progress was impossible. And lastly, it furnished a standard with which successions of strata in various regions could be compared, the differences noted, and the actual sections tabulated. In short, it was the first provisional generalization; and was useful, if not indispensable, as a step to truer ones.

Following this rude conception, which ascribed geological phenomena to one agency, acting during one primeval epoch, there came a greatly-improved conception, which ascribed them to two agencies, acting alternately during successive epochs. Hutton, perceiving that sedimentary deposits were still being formed at the bottom of the sea from the detritus carried down by rivers; perceiving, further, that the strata of which the visible surface chiefly consists, bore marks of having been similarly formed out of pre-existing lands; and inferring that these strata could have become land only by being upheaved after their deposit; concluded that throughout an indefinite past, there had been periodic convulsions, by

which continents were raised, with intervening eras of repose, during which such continents were worn down and transformed into new marine strata, fated to be in their turns elevated above the surface of the ocean. And finding that igneous action, to which sundry earlier geologists had ascribed basaltic rocks, was in countless places a source of disturbance, he taught that from it resulted these periodic convulsions. In this theory we see:—firstly, that the previously recognized agency of water was conceived to act, not as by Werner, after a supposed manner of which we have no experience, but after a manner daily displayed to us; and secondly, we see that the igneous agency, before considered only as a cause of special formations, was recognized as an universal agency, but assumed to act in a way of which experience gives no proof. Werner's sole process, Hutton developed from the catastrophic and inexplicable into the uniform and explicable; while that antagonistic second process, of which he first adequately estimated the importance, was regarded by him as a catastrophic one, and was not assimilated to known processes—not explained. We have here to note, however, that the facts collected and provisionally arranged in conformity with Werner's theory, served after a time as evidences for the establishment of Hutton's more rational theory—in so far at least as aqueous formations are concerned; while the doctrine of periodic subterranean convulsions, crudely as it was conceived by Hutton, was a temporary generalization, needful as a step towards the theory of igneous action.

Since Hutton's time the development of geological thought has gone still further in the same direction. These early sweeping doctrines have received additional qualifications: the agencies at work have been discovered to be more heterogeneous than was at first thought; and the igneous hypothesis has been rationalized, as the aqueous one had previously been—the gratuitous assumption of vast elevations suddenly occurring after long intervals of quiescence, has grown into the consistent theory that islands and continents are the cumulative results of successive small upheavals, like those experienced in ordinary earthquakes. Or to speak more specifically, we find:—First, that instead of assuming the denudation produced by rain and rivers to be the sole means of wearing down lands and producing their irregularities of surface, geologists have now shown that denudation is only a part-cause of such irregularities; and further, that the new strata deposited at the bottom of the sea, are not solely the products of river sediment, but are in part due to the action of waves and tidal currents on the coasts. In the second place, we find that Hutton's conception of subterranean forces, as causing elevations, has not only been modified by assimilating it to ordinary earthquake forces; but modern inquiries have shown that, besides elevations of surface, subsidences are thus produced; that local upheavals, such as result in mountain chains, as well as the general upheavals that raise con-

tinents, come within the same category; and that in all probability these changes are severally consequent upon the progressive collapse of the Earth's crust upon its cooling and contracting nucleus —the only adequate physical cause. In the third place, we find that in addition to these two great antagonist agencies, modern geology recognizes sundry minor ones: as those of glaciers and ice-bergs; those of coral-polypes; those of infusoria having siliceous or calcareous shells—each of which agencies, insignificant as it seems, is found capable of slowly producing terrestrial changes of considerable magnitude. Thus, then, the recent progress of Geology has been a still further departure from primitive conceptions. Instead of one special catastrophic cause, once in universal action, as supposed by Werner—instead of one general continuous cause, antagonized at long intervals by a catastrophic cause, as taught by Hutton; we now recognize several causes, all more or less general and continuous. We no longer resort to hypothetical agencies to explain the phenomena displayed by the Earth's crust; but we are day by day more clearly perceiving that these phenomena are explicable as due to natural forces like those now at work, which have acted in all varieties of combination, through immeasurable periods of time.

Such having been the evolution of geologic science, and such being the form which it daily tends more and more to assume, let us go on to observe the way in which it is still swayed by the crude hypotheses it set out with; so that even now, old doctrines which are abandoned as quite untenable in theory, continue in practice to mould the ideas of geologists, and to foster sundry beliefs that are logically indefensible. Let us note, not only how those simple sweeping conceptions with which the science commenced, are those which every fresh student is apt at first to seize hold of, and be afterwards insensibly biased by; but also, how several influences conspire to maintain the twist thus resulting—how the original nomenclature of periods and formations necessarily carries with it more or less of the original implications; and how the need for arranging new data in some order, naturally results in their being thrust into the old classification, unless their incongruity with it is very glaring. A few facts will best prepare the way for criticism.

Up to 1839 it was inferred, from their crystalline character, that the metamorphic rocks of Anglesea were more ancient than any rocks of the adjacent main land; but it has since been shown that they are of the same age with the slates and grits of Carnarvon and Merioneth. Again, slaty cleavage having been first found only in the lowest rocks, was taken as an indication of the highest antiquity: whence resulted serious mistakes; for this mineral characteristic is now known to occur in the Carboniferous system. Once more, certain red conglomerates and grits on the north-west coast of Scotland, long supposed from their lithological

aspect to belong to the Old Red Sandstone series, are now identified with the Lower Silurians. These are a few instances of the small trust to be placed in mineral quality as any evidence of the ages or relative positions of strata. From the recently published third edition of *Siluria*, may be culled numerous facts of like implication. Sir R. Murchison considers it ascertained that the siliceous Stiper stones of Shropshire are the equivalents of the Tremadock slates of North Wales. Judging from their fossils, Bala slate and limestone are of the same age as the Caradoc sandstone, lying forty miles off. In Radnorshire, the formation classed as upper Llandovery rock is described at different spots, as "sandstone or conglomerate," "impure limestone," "hard coarse grits," "siliceous grit"—a considerable variation for so small an area as that of a county. Certain sandy beds on the left bank of the Towy, which Sir R. Murchison had, in his *Silurian System* classed as Caradoc sandstone (evidently from their mineral character), he now finds, from their fossils, belong to the Llandeilo formation. Yet, though *Siluria*, in common with other geological works, supplies numerous proofs that rocks of the same age are often of widely-different composition a few miles off, while rocks of widely-different ages are often of similar composition; and though Sir R. Murchison shows us, as in the case just cited, that he has himself in past times been misled by trusting to lithological evidence; nevertheless, his reasoning all through *Siluria* shows that he still thinks it natural to expect formations of the same age to be chemically similar, even in remote regions. For example, in treating of the Silurian rocks of South Scotland, he says,—"When traversing the tract between Dumfries and Moffat in 1850, it occurred to me that the dull reddish or purple sandstone and schist to the north of the former town, which so resembled the bottom rocks of the Longmynd, Llanberis, and St. David's, would prove to be of the same age;" and further on, he again insists upon the fact that these strata "are absolutely of the same composition as the bottom rocks of the Silurian region." On this unity of mineral character it is, that this Scottish formation is concluded to be contemporaneous with the lowest formations in Wales; for the scanty palæontological evidence suffices neither for proof nor disproof. Now, had there been a decided continuity of similar strata in similar order, between Wales and Scotland, there might have been little to criticize in this conclusion. But seeing that Sir R. Murchison himself admits, that in Westmoreland and Cumberland, some members of the system "assume a lithological aspect different from what they maintain in the Silurian and Welsh region," there seems no reason to expect mineralogical continuity in Scotland; but rather the reverse. Obviously, therefore, the assumption that these Scottish formations are of the same age with the Longmynd of Shropshire, implies the latent belief that certain mineral characters indicate certain eras. Far more striking instances, however, of the influence of this latent belief remain

to be given. Not in such comparatively near districts as the Scottish lowlands only, does Sir R. Murchison expect a repetition of the Longmynd strata; but in the Rheniah provinces, certain "quartzose flagstones and grits, like those of the Longmynd," are seemingly concluded to be of contemporaneous origin, because of their likeness. "Quartzites in roofing-slates with a greenish tinge that reminded us of the lower slates of Cumberland and Westmoreland," are evidently suspected to be of the same age. In Russia, he remarks, that the carboniferous limestones "are overlaid along the western edge of the Ural chain by sandstones and grits, which occupy much the same place in the general series as the millstone grit of England;" and in calling this group, as he does, the "representative of the millstone grit," Sir R. Murchison clearly shows that he thinks likeness of mineral composition some evidence of equivalence in time, even at that great distance. Add to which that, even on the flanks of the Andes and in the United States, such similarities are looked for, and considered as more or less significant of certain ages. Not that Sir R. Murchison contends theoretically for this relation between lithological character and date. For upon the page from which we have just quoted (*Siluria*, p. 387), he says, that "whilst the soft Lower Silurian clays and sands of St. Petersburg have their equivalents in the hard schists and quartz rocks with gold veins in the heart of the Ural mountains, the equally soft red and green Devonian marls of the Valdai Hills are represented on the western flank of that chain by hard, contorted, and fractured limestones." But these, and other such admissions, seem to go for little. While himself asserting that the Potsdam sandstone of North America, the Lingula flags of England, and the alum slates of Scandinavia are of the same period—while fully aware that among the Silurian formations of Wales, there are oolitic strata like those of secondary age; yet is his reasoning more or less coloured by the assumption that formations of like quality probably belong to the same era. Is it not manifest, then, that the exploded hypothesis of Werner continues to influence geological speculation?

"But," it will, perhaps, be said, "though individual strata are not continuous over large areas, yet systems of strata are. Though within a few miles the same bed will gradually pass from clay into sand, or will thin out and disappear, yet the group of strata to which it belongs does not do so; but maintains in remote regions the same relations to other groups."

This is the generally current belief. On this assumption the received geological classifications appear to be framed. The Silurian system, the Devonian system, the Carboniferous system, &c., are set down in our books as groups of formations which everywhere succeed each other in a given order; and are severally everywhere of the same age. Though it may not be asserted that

these successive systems are universal; yet it seems to be tacitly assumed that they are so. In North and South America, in Asia, in Australia, sets of strata are assimilated to one or other of these groups; and their mineral characters and order of superposition are among the reasons assigned for so assimilating them. Though, probably, no competent geologist would contend that the European classification of strata is applicable to all other parts of the globe, yet most, if not all geologists, write as though it were so. We venture to say that among readers of works on geology, nine out of ten carry away the impression that the divisions, Primary, Secondary and Tertiary, are of absolute and uniform application; that these great divisions are separable into subdivisions, each of which is definitely distinguishable from the rest, and is everywhere recognizable by its characters as such or such; and that in all parts of the Earth, these minor systems severally began and ended at the same time. When they meet with the term "carboniferous era," they take for granted that it was an era universally carboniferous —that it was, what Hugh Miller indeed actually describes it, an era when the Earth bore a vegetation more luxuriant than it has ever since done; and were they in any of our colonies to meet with a coal-bed, they would conclude that, as a matter of course, it was of the same age as the English coal-beds.

Now this belief that geologic "systems" are universal, is just as untenable as the other. It is just as absurd when consider *à priori*; and it is equally inconsistent with the facts. Though some series of strata classed together as Oolite may range over a wider district than any one stratum of the series, yet we have but to consider what were the necessary circumstances of their deposit, to see that the horizontal extent of such series, like that of its individual strata, must be limited; and that there is not likely to be anywhere else a series that exactly corresponds either in its characters or in its commencement and termination. For the formation of such a series implies an area of subsidence, in which its component beds were thrown down. Every area of subsidence is necessarily limited; and to suppose that there exist elsewhere groups of beds completely answering to these, is to suppose that, in contemporaneous areas of subsidence, like processes were going on. There is not the least reason to suppose this; but every reason to suppose the reverse. That in contemporaneous areas of subsidence throughout the globe, the conditions would be such as to cause the formation of oolite, or anything like it, is an assumption which no modern geologist would openly make: he would say that the equivalent series of beds found elsewhere would very likely be of dissimilar mineral character. Not only is it, however, that in these contemporaneous areas of subsidence the phenomena going on must be more or less different in kind; but it is that, in no two cases are they likely to agree in their commencements and terminations. The probabilities are greatly against any two separate portions of

the Earth's surface beginning to subside at the same time, and ceasing to subside at the same time—a coincidence which can alone produce corresponding groups of strata. On the contrary, subsidences in different places begin and end with utter irregularity; and hence the groups of strata thrown down in them can but rarely answer to each other as groups. Measured against each other in time, their limits will disagree. They will refuse to fit into any scheme of definite divisions. On turning to the evidence, we find that it daily tends more and more to justify these *à priori* positions. Take, as an example, the Old Red Sandstone system. In the north of England this is represented by a single stratum of conglomerate. In Herefordshire, Worcestershire, and Shropshire, it expands into a series of strata from eight to ten thousand feet thick, made up of conglomerates, red, green, and white sandstones, red, green, and spotted marls, and concretionary limestones. To the south-west, as between Caermarthen and Pembroke, the series of strata exhibits considerable lithological changes; and there is an absence of fossil fishes. On the other side of the Bristol Channel, they display a further change in mineral character and remains. While in South Devon and Cornwall, the equivalent strata, consisting chiefly of slates, schists, and limestones, are so wholly different, that they were for a long time classed as Silurian. When we thus see that in certain directions the whole series of strata thins out, and that its mineral characters as well as its Fauna continually change within moderate distances; does it not become clear that the series of deposits called Old Red Sandstone or Devonian was a local one? And when we find, in other regions, analogous deposits, is it certain—is it even probable—that they severally began and ended at the same time with this? Should it not require overwhelming evidence to make us believe as much?

Yet so strongly is geological speculation swayed by the tendency to regard the phenomena as general instead of local, that even those most on their guard against it seem unable to escape its influence. At page 158 of his *Principles of Geology*, Sir Charles Lyell says:—

> "A group of red marl and red sandstone, containing salt and gypsum, being interposed in England between the Lias and the Coal, all other red marls and sandstones, associated some of them with salt, and others with gypsum, and occurring not only in different parts of Europe, but in North America, Peru, India, the salt deserts of Asia, those of Africa—in a word, in every quarter of the globe, were referred to one and the same period. . . . . It was in vain to urge as an objection the improbability of the hypothesis which implies that all the moving waters on the globe were once simultaneously charged with sediment of a red colour. But the rashness of pretending to identify, in age, all the red sandstones and marls in question, has at length been sufficiently exposed, by the discovery that, even in Europe, they belong decidedly to many different epochs."

Nevertheless, while in this and numerous passages of like implication, Sir C. Lyell protests against the bias here illustrated,

he seems himself not completely free from it. Though he utterly rejects the old hypothesis that all over the Earth the same continuous strata lie upon each other in regular order, like the coats of an onion, he still writes as though geologic "systems" did thus succeed each other. A reader of his "Manual" would certainly suppose him to believe that the Primary epoch ended, and the Secondary epoch commenced, all over the world at the same time—that these terms really correspond to distinct universal eras in Nature. When he assumes, as he does, that the division between Cambrian and Lower Silurian in America, answer chronologically to the division between Cambrian and Lower Silurian in Wales—when he takes for granted that the partings of Lower from Middle Silurian, and of Middle Silurian from Upper, in the one region, are of the same date as the like partings in the other region; does it not seem that he believes geologic "systems" to be universal, in the sense that their lines of separation in all places were contemporaneous? Though he would, doubtless, disown this as an article of faith, is not his thinking unconsciously swayed by it? Must we not say that though the onion-coat hypothesis is dead, its spirit is traceable, under a transcendental form, even in the conclusions of its antagonists?

Let us now consider another leading geological doctrine, to which the cases just referred to introduce us. We mean the doctrine that strata of the same age contain like fossils; and that, therefore, the age and relative position of any stratum may be known by its fossils. While the theory that strata of like mineral characters were everywhere deposited at the same period, has been ostensibly abandoned, there has been accepted the theory that in successive geologic epochs similar plants and animals existed everywhere; and that, therefore, the epoch to which any formation belongs may be known by its contained organic remains. Though, perhaps, no leading geologist would openly commit himself to this belief in its unqualified form, yet it is tacitly assumed in current geological reasoning.

Yet this theory is scarcely more tenable than the other. It cannot be concluded with any certainty that formations in which similar organic remains are found, were of contemporaneous origin; nor can it be safely concluded that strata containing different organic remains are of different ages. This assertion, which to most readers will be startling, is fully admitted by the highest authorities. Sir Charles Lyell confesses that the test of organic remains must be used "under very much the same restrictions as the test of mineral composition." Sir Henry de la Beche, who variously illustrates this truth, gives, as one instance, the great incongruity there must be between the fossils of our carboniferous rocks and those of the marine strata deposited at the same period. But though, in the abstract, the danger of basing positive conclusions upon evi-

dence derived from fossils, is clearly recognized; yet, in the concrete, it is very generally left out of sight. The established conclusions respecting the ages of strata, take but little note of it; and by some geologists it seems altogether ignored. Throughout his *Siluria*, Sir R. Murchison habitually assumes that the same, or kindred species, lived in all parts of the Earth at the same time. In Russia, in Bohemia, in the United States, in South America, strata are classed as belonging to this or that part of the Silurian system, on the strength of the similar fossils contained in them—are concluded to be everywhere contemporaneous if they enclose a proportion of identical or allied forms. In Russia the relative position of a stratum is inferred from the fact that, along with some Wenlock forms, it yields the *Pentamerus oblongus*. Respecting a class of crustaceans found in the Upper Ludlow rock, it is remarked that "large Eurypteri occur in a so-called black greywacke slate at Westmoreland, in Oneida County, New York, which will probably be found to be on the parallel of the Upper Ludlow rock": in which word "probably" we may see both how dominant is this belief of universal distribution of similar creatures at the same period, and how apt this belief is to make its own justification by raising the expectation that the ages are identical when the forms are alike. Not only, however, does Sir R. Murchison interpret the formations of Russia, England, and America after this manner, but also those of the antipodes. Fossils from Victoria Colony, he agrees with the Government surveyor in classing as of Lower Silurian or Llandovery age: that is, he takes for granted, that when certain crustaceans and mollusks were living in Wales, certain similar crustaceans and mollusks were living in Australia. Yet the improbability of this assumption may be readily shown from Sir R. Murchison's own facts. If, as he points out, the crustacean fossils of the uppermost Silurian rocks in Lanarkshire are, "with one doubtful exception," "all distinct from any of the forms known on the same horizon in England;" how can it be fairly presumed that the contemporaneous forms existing on the other side of the Earth were nearly allied to those existing here? Not only, indeed, do Sir R. Murchison's conclusions tacitly assume this doctrine of universal distribution, but he distinctly enunciates it. "The mere presence of a graptolite," he says, "will at once decide that the enclosing rock is Silurian:" and he says this, notwithstanding repeated warnings of the danger of such generalizations. The records of geology furnish numerous cases in which a particular fossil, long considered characteristic of a particular formation, has been afterwards discovered in other formations. Until some twelve years ago, Goniatites had not been found lower than the Devonian rocks; but now, in Bohemia, they have been found in rocks classed as Silurian. Quite recently, the Orthoceras, for years supposed to be a form exclusively palæozoic, has been detected along with mæsozoic Ammonites and Belem-

nites. Yet hosts of such experiences fail to extinguish the assumption, that the age of a stratum may be safely determined by the occurrence in it of a single fossil form. Nay, this assumption survives evidence of even a still more destructive kind. Speaking of the Silurian system in Western Ireland, Sir R. Murchison says, "in the beds near Maam, Professor Nicol and myself collected remains, some of which would be considered Lower, and others Upper, Silurian:" and he then names sundry fossils which, in England, belong to the summit of the Ludlow rocks, or highest Silurian strata; some, "which elsewhere are known only in rocks of Llandovery age," that is, of middle Silurian age; and some, only yet known in Lower Silurian strata, not far above the most ancient fossiliferous beds. Now what do these facts prove? Clearly, they prove that organic forms which in Wales are separated by strata more than twenty thousand feet deep, and therefore seem to belong to periods far remote from each other, were really co-existent. They prove that the mollusks and crinoids held characteristic of early Silurian strata, and supposed to have become extinct long before the mollusks and crinoids of the later Silurian strata came into existence, were really flourishing at the same time with these last; and that these last probably date back to as early a period as the first. They prove that not only the mineral characters of sedimentary formations, but also the collections of organic forms they contain, is, to a great extent, a question of local circumstances. They prove that the fossils met with in any series of strata cannot in the least be taken as representing the whole Flora and Fauna of the period they belong to. In brief, they throw great doubt upon numerous geological generalizations.

Notwithstanding facts like these, and notwithstanding his avowed opinion that the test of organic remains must be used "under very much the same restrictions as the test of mineral composition," Sir Charles Lyell, too, bases positive conclusions on this test; even where the community of fossils is very slight and the distance very great. Having decided that in various places in Europe, middle Eocene strata are distinguished by nummulites; he infers, without any other assigned evidence, that wherever nummulites are found—in Morocco, Algeria, Egypt, in Persia, Scinde, Cutch, Eastern Bengal, and the frontiers of China—the containing formation is middle Eocene. And from this inference he draws the following important corollary:—

"When we have once arrived at the conviction that the nummulitic formation occupies a middle place in the Eocene series, we are struck with the comparatively modern date to which some of the greatest revolutions in the physical geography of Europe, Asia, and northern Africa must be referred. All the mountain chains, such as the Alps, Pyrenees, Carpathians, and Himalayas, into the composition of whose central and loftiest parts the nummulitic strata enter bodily, could have had no existence till after the middle Eocene period."—*Manual*, p. 232.

A still more marked case follows on the next page, where, because a certain bed at Claiborne in Alabama which contains "*four hundred* species of marine shells," includes among them the *Cardita planicosta*, "and *some others* identical with European species, or very nearly allied to them," Sir C. Lyell says it is "highly probable the Claiborne beds agree in age with the central or Bracklesham group of England." When we find contemporaneity supposed on the strength of a community no greater than that which sometimes exists between strata of widely-different ages in the same country, it seems very much as though the above-quoted caution had been forgotten. It appears to be assumed for the occasion that species which had a wide range in space had a narrow range in time; which is the very reverse of the fact. Indeed, it is obvious that the tendency to systematize overrides the evidence, and thrusts Nature into a formula far too rigid to fit her endless variety.

"But," it may be urged, "surely, when in different places the order of superposition, the mineral characters, and the fossils agree, it may be safely concluded that the formations thus corresponding are equivalents in time. If, for example, the United States displays the same succession of Silurian, Devonian, and Carboniferous systems, lithologically similar, and characterized by like fossils, it is a fair inference that these groups of strata were severally deposited in America at the same periods that they were deposited here."

On this position, which seems a very strong one, we have, in the first place, to remark, that the evidence of correspondence is always more or less suspicious. We have already adverted to the several "idols"—if we may use Bacon's metaphor—to which geologists unconsciously sacrifice, when interpreting the structures of unexplored regions. Carrying with them the European classification of strata, and assuming that groups of strata in other parts of the world must answer to some or other of the groups of strata known here, they are necessarily prone to assert parallelism on insufficient evidence. They scarcely entertain the enquiry whether the formations they are examining are not a distinct series, having no European equivalents; but the question is,—with which of the European series shall they be classed?—with which do they most agree?—from which do they differ least? And this being the course of investigation, it is manifest that there is apt to result great laxity of interpretation. How lax the interpretation really is may be readily shown. When strata are discontinuous, as between Europe and America, it is clear that no evidence can be derived from the order of superposition, apart from mineral characters and organic remains; for, unless strata can be continuously traced, mineral characters and organic remains are the only means of classing them as such or such. As to the test of mineral characters, we have seen that it is

almost worthless; and no modern geologist would dare to say it should be relied on. If the entire Old Red Sandstone series in mid-England, differs wholly in lithological aspect from the equivalent series in South Devon, it is clear that similarities of texture and composition can have no weight in assimilating a system of strata in another quarter of the globe to some European system. The test of fossils, therefore, is the only one that remains; and with how little strictness this test is applied, one case will show. Of forty-six species of British Devonian corals, only six occur in America; and this, notwithstanding the wide range in latitude which the Anthozoa are known to have. Similarly of the Mollusca and Crinoidea, it appears that, while there are sundry genera found in America that are found here, there are scarcely any of the same species. And Sir Charles Lyell admits that "the difficulty of deciding on the exact parallelism of the New York subdivisions, as above enumerated, with the members of the European Devonian, is very great, so few are the species in common." Yet it is on the strength of community of fossils that the whole Devonian series of the United States is assumed to be contemporaneous with the whole Devonian series of England. And it is partly on the ground that the Devonian of the United States corresponds in time with our Devonian, that Sir Charles Lyell concludes the superjacent coal-measures of the two countries to be of the same age. Is it not, then, as we said—that the evidence in these cases is very suspicious?

Even supposing, however, that districts some hundreds of miles apart furnished groups of strata that completely agreed in their order of superposition, their mineral characters, and their fossils, we should still have inadequate proof of contemporaneity. For there are conditions, very likely indeed to occur, under which such groups might differ widely in age. If there be a continent of which the strata crop out on the surface obliquely to the line of coast—running, say, west-north-west, while the coast runs east and west—it is clear that each group of strata will crop out upon the beach at a particular part of the coast; that further west the next group of strata will crop out on the beach; and so continuously. As the localization of marine plants and animals is in a considerable degree determined by the nature of the rocks and their detritus, it follows that each part of this coast will have its more or less distinct flora and fauna. What now would result from the action of the waves in the course of a geologic epoch? As the sea made slow inroads upon the land, the place at which each group of strata cropped out upon the beach would gradually move towards the west; its distinctive fish, mollusks, crustaceans, and sea-weeds, migrating with it. Further, the detritus of each of these groups of strata would, as the point of outcrop moved westwards, be deposited over the detritus of the group in advance of it. And if the reader will consider what would be the con-

sequence of these actions carried on for one of those enormous periods required for geological changes, he will see that, corresponding to some eastern stratum, there might exist a stratum far to the west, which, though occupying the same position relatively to other beds, formed of like materials, and containing like fossils, should yet be perhaps a million years later in date.

But the illegitimacy, or at any rate the extreme doubtfulness, of current geological inferences, is best seen when we contemplate the various terrestrial changes now going on, and ask how far such inferences are countenanced by them. If we carry out rigorously the established method of interpreting geological phenomena—that of referring them to causes like those at present in action—we cannot fail to see how improbable are many of the received conclusions.

Along each line of shore that is being worn away by the waves, there are being formed mud, sand, and pebbles, varying in character according to the nature of the strata destroyed. And this detritus, spread over the neighbouring sea-bottom, has, in each locality, a more or less special character. In the English Channel it is not the same as in the Irish Channel; on the east coast of Ireland it is not the same as on the west coast; and so throughout. At the mouth of each great river there is being deposited sediment differing more or less from that of other rivers in colour and quality; forming strata that are here red, there yellow, and in other places brown, grey, or dirty white. Besides which various formations going on in deltas and along shores, there are some much wider and still more contrasted formations. At the bottom of the Ægæan Sea there is accumulating a bed of pteropod shells, which will eventually, no doubt, become a calcareous rock. For some hundreds of thousands of square miles the ocean bed between Great Britain and North America is being covered with a stratum of chalk; and over large areas in the Pacific, there are going on deposits of coralline limestone. Thus throughout the Earth there are at this moment being formed an immense number of strata differing from each other in lithological characters. Name at random any one part of the sea-bottom, and ask whether the deposit there taking place is like the deposit taking place at some distant part of the sea-bottom, and the almost certainly correct answer will be—No. The chances are not in favour of similarity, but very greatly against it.

In the order of superposition of strata there is occurring a like variety. Each region of the Earth's surface has its special history of elevations, subsidences, periods of rest; and this history in no case fits chronologically with the history of any other portion. River deltas are now being thrown down upon formations of quite different ages. While here there has been deposited a series of beds many hundreds of feet thick, the same period

has elsewhere produced but a single bed of fine mud. While in one region the Earth's crust, continuing for a vast epoch above the surface of the ocean, bears record of no changes save those resulting from denudation; in another region the Earth's crust has undergone various changes of level, with their several accompanying groups of stratified detritus. If anything is to be judged from current processes, we must infer, not only that everywhere the succession of sedimentary formations differs more or less from the succession elsewhere, but also that in each place there exists some series of strata to which many other places have no equivalents.

And with respect to the organic bodies imbedded in formations now in progress, the like truth is equally manifest, if not more manifest. Even along the same coast, within very moderate distances, the forms of life differ very considerably; much more on coasts that are at all remote from each other. Again, creatures that are living together near the same shore, do not leave their remains in the same beds of sediment. For instance, at the bottom of the Adriatic, where the prevailing currents cause the deposits to be here of mud, and there of calcareous matter, it is proved that different species of co-existing shells are being buried in these respective formations. On our own coasts we may note that the marine remains found a few miles from shore, in banks where fish congregate, are different from those found close to the shore, where only littoral species flourish. A large proportion of aquatic creatures have structures that do not admit of fossilization; while of the rest the great majority are destroyed, when dead, by the various kinds of scavengers that creep among the rocks and weeds. So that no one deposit near our shores can contain anything like a true representation of the fauna of the surrounding sea; much less of the co-existing faunas of other seas in the same latitude; and still less of the faunas of seas in other latitudes. Were it not that the assertion seems needful, it would be almost absurd to say, that the organic remains now being buried in the Dogger Bank, can tell us next to nothing about the fish, crustaceans, mollusks, and corals that are being buried in the Bay of Bengal. In respect to terrestrial life, the case is even stronger. With still more numerous and greater contrasts between the plants and animals of remote places, there is a still more imperfect registry of them. Schouw marks out on the Earth more than twenty botanical regions; in each of which the group of forms is so far distinct from the rest, that, if it were fossilized, geologists would probably refer it to a separate epoch. Of fauna, the Arctic differs from the Temperate; the Temperate from the Tropical; and the South Temperate from the North Temperate. Nay, in the South Temperate Zone itself, the two regions of South Africa and South America are unlike in their mammals, birds, reptiles, fishes, mollusks, insects. The remains that are being buried at

the bottoms of lakes and estuaries in these several regions, have certainly not that similarity which is usually looked for in those of contemporaneous strata; and the recent forms exhumed in any one of these regions would very untruly represent the present flora and fauna of the Earth. In conformity with the current style of geological reasoning, an exhaustive examination of deposits in the Arctic circle, might be held to prove that though at this period there were sundry mammals existing, there were no reptiles; while the absence of mammals in the deposits of the Galapagos Archipelago, where there are plenty of reptiles, might be held to prove the very reverse. And at the same time, from the formations extending for two thousand miles along the north of Australia, in which are imbedded nothing but corals, mollusks, crustaceans, and fish, along with an occasional cetacean, it might be inferred that there lived in our epoch neither reptiles, nor terrestrial mammals. The mention of Australia, indeed, suggests a conclusive illustration, which, even alone, would amply prove our case. The fauna of this region, both marine and terrestrial, differs widely from any that is found elsewhere. On land all the mammals belong to the lowest, or implacental division; and the insects and testacea are singularly different from those found elsewhere. In the surrounding seas everything is more or less strange; and among the fish there exists a species of shark, which is the only living representative of a genus abundant in early geologic epochs. If now the modern fossiliferous deposits of Australia were to be examined by one ignorant of the existing Australian fauna; and if he were to reason in the usual manner; he would certainly not class these deposits with those of the present time, but with those of some much earlier time. How, then, can we place confidence in the tacit assumption of geologists that formations in remote parts of the Earth are referable to the same period, because the organic remains contained in them display a certain community of character?

"But," it will be replied, "in past eras the same, or similar, organic forms were more widely distributed than now." It may be so; but the evidence adduced by no means proves it. The argument by which this conclusion is reached, runs a risk of being quoted as an example of reasoning in a circle. As already pointed out, between formations in remote regions there is no means of ascertaining equivalence but by fossils. If, then, the contemporaneity of remote formations is concluded from the likeness of their fossils, how can it be said that similar plants and animals were once more widely distributed, because they are found in contemporaneous strata in remote regions? Is not the fallacy manifest? And even supposing there were no such fatal objection as this, the evidence commonly assigned would still be insufficient. For we must bear in mind that the community of organic remains commonly thought sufficient for inferring correspondence in time, is a very imperfect

one. When the compared sedimentary beds are far apart, it is scarcely expected that there will be many species common to the two; it is enough if there be discovered a considerable number of common genera. If now it had been proved that, throughout geologic time, each genus lived but for a short period—a period measured by a single group of strata—something might be inferred. But what if we learn that many of the same genera continued to exist through enormous epochs, measured by several vast systems of strata? When we read that, "among molluscs, the genera *Avicula, Modiola, Terebratula, Lingula,* and *Orbicula,* are found from the Silurian rocks upwards to the present day," must we not infer that if, between the lowest fossiliferous formations and the most recent, there exists this degree of community, there will probably often exist a great degree of community between strata that are far from contemporaneous?

We contend, then, that the reasoning from which it is concluded that similar organic forms were once more widely spread is doubly fallacious; that, consequently, the classifications of foreign strata based on this conclusion are untrustworthy; farther, that judging from the present distribution of life we can scarcely expect to find similar remains in geographically remote strata of the same age; and that where, between the fossils of such geographically remote strata, we do find much similarity, it is probably often due rather to likeness of conditions than to contemporaneity. If from causes and effects, such as we now witness, we reason back to the causes and effects of past epochs, we discover inadequate warrant for sundry of the received doctrines. Seeing as we do that in the Pacific this is a coral-period, that in the North Atlantic it is a chalk-period, and that in the delta of the Mississippi it is a coal-period—seeing also as we do that in one extensive continent this is peculiarly an era of implacental mammals, and that in another extensive continent it is as peculiarly an era of placental mammals; we have good reason to hesitate before accepting as proved, these sweeping generalizations which are based upon a cursory examination of strata occupying but a tenth part of the Earth's surface.

At the outset, this article was to have been a criticism upon the works of Hugh Miller; but it has grown into something much more general. Nevertheless, the remaining two doctrines which we propose to criticize, may be conveniently treated in connection with his name, as that of one who fully committed himself to them. And first, a few words with regard to his position.

That he was a man whose life was one of meritorious achievement, every one knows. That he was a diligent and successful working geologist scarcely needs saying. That with indomitable perseverance he struggled up from obscurity to a place in the world of literature and science, shows him to have been highly endowed in character and intelligence. And that he had a remarkable

power of presenting his facts and arguments in an attractive form, a glance at any of his books will quickly prove. By all means, let us accord him high praise as a man of great activity and sagacity, joined with a large amount of poetry. But while saying thus much, we must add, that his reputation stands by no means so high in the scientific world as in the world at large. Partly from the fact that our Scotch neighbours are in the habit of blowing the trumpet rather loudly before their notabilities—partly because the charming style in which his books are written has gained him a large circle of readers—partly, perhaps, from a praiseworthy sympathy with him as a self-made man; Hugh Miller has met with an amount of applause which, little as we wish to diminish it, must not be allowed to blind the public to his defects as a man of science. The truth is, he was so far committed to a foregone conclusion, that he could not become a truly philosophical geologist. He might aptly be described as a theologian studying geology. The dominant idea with which he wrote may be seen in the titles of his books—*Law versus Miracle*,—*Footprints of the Creator*,—*The Testimony of the Rocks*. Regarding all geological phenomena as evidence for or against certain religious conclusions, it was scarcely possible for him to deal with the facts impartially. His ruling aim was to disprove the Development Hypothesis, to the assumed implications of which he was strongly opposed; and in proportion to the strength of his feeling, was the one-sidedness of his reasoning. He admitted that "God might as certainly have *originated* the species by a law of development, as he *maintains* it by a law of development; the existence of a First Great Cause is as perfectly compatible with the one scheme as with the other." But admitting this, he still considered the hypothesis at variance with Christianity; and therefore combated it. He appears to have overlooked the fact, that the doctrines of geology in general, as held by himself, have been by many rejected on similar grounds; that Captain Hutton's *Chronology of Creation*, and various like books, have been written from this point of view; and that he has himself been repeatedly attacked for his anti-Christian teachings. He seems not to have perceived that, just as his antagonists were wrong in condemning as irreligious, theories which he saw were not irreligious; so might he possibly be wrong in condemning, on like grounds, the Theory of Evolution. He did not bear in mind that, from the time of Galileo downwards, various scientific truths have been fought against because they were thought fatal to religion; which, nevertheless, has not only survived, but has become purer. In brief, he fell short of that highest faith, which knows that all truths must harmonize; and which is, therefore, content trustfully to follow the evidence whithersoever it may lead.

Of course it is impossible to criticize his works without entering upon this great question to which he chiefly devoted himself. The two remaining general doctrines which we have to discuss

bear directly on this question; and, as above said, we now propose to treat them in connection with Hugh Miller's name, because, throughout all his reasonings, he assumes their truth. Let it not be supposed, however, that we shall aim to prove what he has aimed to disprove. While we purpose showing that his arguments against the Development Hypothesis are based on invalid assumptions; we by no means purpose showing that the opposing arguments are based on valid assumptions. On the contrary, we hope to make it apparent, not only that the geological evidence at present obtained is insufficient for either side; but that there seems little probability of sufficient evidence ever being obtained; and that if the question is eventually decided, it must be decided on other than geological data.

The first of the current doctrines to which we have just referred is, that there occur in the records of former life upon our planet, certain great blanks—that though, generally, the succession of fossil forms is tolerably continuous, yet that at two places there occur wide gaps in the series: whence it is inferred that, on at least two occasions, the previously existing inhabitants of the Earth were almost wholly destroyed, and a different class of inhabitants created. Comparing the general life on the Earth to a thread, Hugh Miller says,—

"It is continuous from the present time up to the commencement of the Tertiary period; and then so abrupt a break occurs, that, with the exception of the microscopic diatomaceæ to which I last evening referred, and of one shell and one coral, not a single species crossed the gap. On its farther or remoter side, however, where the Secondary division closes, the intermingling of species again begins, and runs on till the commencement of this great Secondary division; and then, just where the Palæozoic division closes, we find another abrupt break, crossed, if crossed at all,—for there still exists some doubt on the subject,—by but two species of plant."

These breaks are considered to imply actual new creations on the surface of our planet; not only by Hugh Miller, but by geologists in general. And the terms Palæozoic, Mæsozoic, and Cainozoic, are used to indicate these three successive systems of life. It is true that some accept this belief with caution: knowing how geologic research has been all along tending to fill up what were once thought wide breaks. Thus Sir Charles Lyell points out that "the hiatus which exists in Great Britain between the fossils of the Lias and those of the Magnesian Limestone, is supplied in Germany by the rich fauna and flora of the Muschelkalk, Keuper, and Bunter Sandstein, which we know to be of a date precisely intermediate." Again he remarks that "until lately the fossils of the coal-measures were separated from those of the antecedent Silurian group by a very abrupt and decided line of demarcation; but recent discoveries have brought to light in Devonshire, Belgium, the Eifel, and Westphalia, the remains of a fauna of an

intervening period." And once more, "we have also in like manner had some success of late years in diminishing the hiatus which still separates the Cretaceous and Eocene periods in Europe." To which let us add that since Hugh Miller penned the passage above quoted, the second of the great gaps to which he refers has been very considerably narrowed by the discovery of strata containing Palæozoic genera and Mæsozoic genera intermingled. Nevertheless, the actual occurrence of two great revolutions in the Earth's flora and fauna appears still to be thought highly probable, if not certain; and geologic nomenclature habitually assumes it.

Before seeking a solution of these phenomena, let us glance at the several minor causes that produce breaks in the geological succession of organic forms: and first, at the more general ones which modify climate, and, therefore, the distribution of life. Among these may be noted one which, so far as we know, is not named by writers on the subject. We mean that resulting from a certain slow astronomical rhythm, by which the northern and southern hemispheres are alternately subject to greater extremes of temperature. In consequence of the slight ellipticity of its orbit, the Earth's distance from the sun varies to the extent of some 3,000,000 of miles. At present the aphelion occurs at the time of our northern summer; and the perihelion during the summer of the southern hemisphere. In consequence, however, of that slow movement of the Earth's axis which produces the precession of the equinoxes, this state of things is eventually reversed: the Earth is nearest to the sun during the summer of the northern hemisphere, and furthest from it during the southern summer or northern winter. The period required to complete the slow movement producing these changes is nearly 26,000 years; and were there no modifying process, the two hemispheres would severally experience this coincidence of summer with the least distance from the sun, during alternate periods of 13,000 years. But there is also a still slower change in the direction of the axis major of the Earth's orbit; from which it results that the alternation we have described is completed in about 21,000 years. That is to say, if at a given time the Earth is nearest to the sun at our mid-summer, and furthest from the sun at our mid-winter; then, in 10,500 years afterwards, it will be furthest from the sun at our mid-summer, and nearest at our mid-winter. Now the difference between the distances from the sun at the two extremes of this alternation, amounts to one-thirtieth; and hence the difference between the quantities of heat received from the sun on a summer's day under these opposite conditions amounts to one-fifteenth. Estimating this, not with reference to the zero of our thermometers, but with reference to the temperature of the celestial spaces, Sir John Herschel calculates "23° Fahrenheit as the least variation of temperature under such circumstances which can reason-

ably be attributed to the actual variation of the sun's distance." Thus, then, each hemisphere has at a certain epoch, a short summer of extreme heat, followed by a long and very cold winter. Through the slow change in the direction of the Earth's axis, these extremes are gradually mitigated. And at the end of 10,500 years, there is reached the opposite state—a long and moderate summer, with a short and mild winter. At present, in consequence of the great predominance of sea in the southern hemisphere, the extremes to which its astronomical conditions subject it, are greatly ameliorated; while the great proportion of land in the northern hemisphere, tends to exaggerate such contrast as now exists in it between winter and summer: whence it results that the climates of the two hemispheres are not widely unlike. But 10,000 years hence, the northern hemisphere will undergo annual variations of temperature very far greater than now.

In the last edition of his *Outlines of Astronomy*, Sir John Herschel recognizes this as an element in geological enquiries: regarding it as possibly a part-cause of those climatic changes indicated by the records of the Earth's past. That it has had much to do with the great and long-enduring changes of climate of which we have evidence, seems doubtful; but that it must have entailed a rhythmical exaggeration and mitigation of the climates otherwise produced, seems beyond question. And it seems also beyond question, that there must have been a consequent rhythmical change in the distribution of organic life;—a rythmical change to which we here wish to draw attention, as one cause of minor breaks in the succession of fossil remains. Each species of plant and animal has certain limits of heat and cold, within which only it can exist; and these limits in a great degree determine its geographical distribution. It will not spread north of a certain latitude, because it cannot bear a more northern winter, nor south of a certain latitude, because the summer heat is too great; or else in consequence of the effect of temperature upon the humidity of the air, or on the distribution of the organisms it lives upon, it is indirectly restrained from spreading further. But now, what will result from a slow change of climate produced as above described? Supposing the period with which we start is that in which the contrast of seasons is least marked, it is manifest that during the gradual change towards the period of the most violent contrast, each species of plant and animal will slowly change its limits of distribution—will be gradually driven back, here by the winter's increasing cold, and there by the summer's increasing heat—will retire into those localities that are still fit for it. Thus during 10,000 years each species will ebb away from certain regions it was inhabiting; and during the succeeding 10,000 years will flow back again into those regions. From the strata there forming, its remains will disappear; they will be absent from some of the superposed strata;

and will appear in strata higher up. But in what shapes will they re-appear? Subjected during the 21,000 years of their slow recession and their slow return, to changing conditions of life, they are certain to have undergone modifications; and will therefore re-appear with slight differences of constitution and perhaps of form—will be new varieties or species.

To this cause of minor breaks in the succession of organic forms—a cause on which we have dwelt somewhat at length because it has not been taken into account—we must now add sundry others. Besides these periodically-recurring changes of climate, there are the irregular ones produced by altered distributions of land and sea; and these, sometimes less sometimes far greater in amount than the rhythmical changes, must, like them, cause in each region the ebb and flow of species, and consequent breaks, small or large as the case may be, in the palæontological series. Other and more special geological changes must produce other and more local blanks in the succession of fossils. By some inland elevation the natural drainage of a continent is more or less altered; and instead of the sediment it previously brought down to the sea, a great river begins to bring down sediment unfavourable to various plants and animals living in its delta: wherefore these disappear from the locality, perhaps to reappear in a changed form after a long epoch. Elevations or subsidences of shores or sea-bottoms, involving changes in the directions of marine currents, must remove the habitats of many species to which such currents are favourable or unfavourable; and further, this redistribution of currents must alter the places of sedimentary deposits, and so stop the burying of organic remains in some localities, and commence it in others. Had we space, many more such causes of blanks in our palæontological records might be added. But it is needless here to enumerate them. They may be readily gathered from Sir Charles Lyell's *Principles of Geology*.

But now, if these minor revolutions of the Earth's surface produce minor breaks in the series of fossilized remains; is it not manifest that great revolutions will produce great breaks? If a local upheaval or subsidence causes throughout its limited area the absence of some links in the chain of fossil forms; does it not obviously follow that an upheaval or subsidence extending over a large part of the Earth's surface, must cause the absence of a great number of such links throughout a very wide area?

When during a long epoch a continent slowly subsides, and gives place to a far-spreading ocean some miles in depth, at the bottom of which no deposits from rivers or abraided shores can be thrown down; it is clear that when, after some enormous period, this ocean bottom is gradually elevated and becomes the site of new strata, formed by the denudation of adjacent lands—it is clear, we say, that the fossils contained in these new strata are likely to have but little in common with the fossils of the strata

below them. Take, in illustration, the case of the North Atlantic. We have already named the fact that between this country and the United States, the ocean-bottom is being covered with a deposit of chalk—a deposit that has been forming, probably, ever since that great depression of the Earth's crust from which the Atlantic resulted in remote geologic times. This chalk consists of the minute shells of Foraminifera, sprinkled with remains of small Entomostraca, and here and there perhaps a few Pteropod shells: though the sounding lines have not as yet brought up any of these last. Thus, in so far as all higher forms of life are concerned, this new chalk formation must be a blank. At rare intervals, perhaps, a polar bear drifted upon an iceberg, may have its bones scattered over the bed; or a dead decaying whale may similarly leave traces. But such remains must be so rare, that this new chalk formation, if visible, might be examined for a century before any of them were disclosed. And now, if some millions of years hence, the Atlantic bed should be raised, and estuary or shore deposits laid upon it, these deposits would contain remains of a flora and fauna so distinct from everything below them, as to appear like a new creation.

Thus we see that along with continuity of life on the Earth's surface, there not only *may* be, but there *must* be, great gaps in the series of fossils; and that, therefore, these gaps are no evidence against the doctrine of evolution.

One other current assumption remains to be criticized; and it is the one on which, more than on any other, depends the view taken respecting the question of development.

From the beginning of the controversy, the arguments for and against have turned upon the evidence of progression in organic forms found in the ascending series of our sedimentary formations. On the one hand, those who contend that higher organisms have been evolved out of lower, joined with those who, without committing themselves to this doctrine, contend that successively higher organisms have been introduced at successively later periods, appeal for proof to the facts of Palæontology; which, they say, countenance their views. On the other hand, the Uniformitarians, who not only reject the hypothesis of development, but deny that the modern forms of life are higher than the ancient ones, habitually reply to this appeal by saying that the Palæontological evidence is at present very incomplete; that though we have not yet found remains of highly-organized creatures in strata of the greatest antiquity, we must not assume that no such creatures existed when those strata were deposited; and that, probably, geological research will eventually disclose them.

It must be admitted that thus far, the evidence has gone in favour of the latter party. Geological discovery has year after year shown the small value of negative facts. It has been over and over again proved that men's inability to find the remains of

higher organisms in earlier strata was due, not to the absence of such remains, but to incomplete examination. At p. 460 of his *Manual of Elementary Geology*, Sir Charles Lyell gives a list in illustration of this. It appears that in 1709, fishes were not known lower than the Permian system. In 1793 they were found in the subjacent Carboniferous system; in 1828 in the Devonian; in 1840 in the Upper Silurian. Of reptiles, it appears that in 1710 the lowest known were in the Permian; in 1844 they were detected in the Carboniferous; and in 1852 in the Upper Devonian. While of the Mammalia it seems, that though in 1798 none had been discovered below the middle Eocene; in 1818 they were discovered in the Lower Oolite; and in 1847 in the Upper Trias. Since this list was made out, the case of the Uniformitarians has been strengthened by Professor Huxley's demonstration, that the Staganolepis of the uppermost Devonian beds, was a teleosaurian reptile—a reptile of high organization, nearly allied to the existing crocodile: whence it appears, that an animal standing nearly at the head of the second great division of the vertebrata, dates back to the middle of the Palæozoic period. And this, mark, implies, according to the Development Hypothesis, a much earlier existence of highly-organized creatures. For there must have been an enormous antecedent series of vertebrate forms before such a reptile could have been reached by the process of evolution. Compared in their organizations, there is not a greater gap between man and a teleosaur, than between a teleosaur and the amphioxus. And if to advance from a teleosaur to man has required the long interval between the Devonian period and the latest Tertiary period; it follows that, if we assume the rate of progress to have been the same, vertebrate forms, if not indeed reptilian forms, must have existed when the earliest known sedimentary beds were deposited—a conclusion apparently fatal to the doctrines of Lamarck and his followers.

The fact is, however, that both parties set out with an inadmissible postulate. Of the Uniformitarians, not only such writers as Hugh Miller, but also such as Sir Charles Lyell, continually take for granted that we have found the earliest, or something like the earliest, strata. Their antagonists, whether defenders of the Development Hypothesis or simply Progressionists, almost uniformly do the like. Sir R. Murchison, who is a Progressionist, calls the lowest fossiliferous strata "Protozoic." Professor Ansted uses the same term. Whether avowedly or not, all the disputants stand upon this assumption as their common ground.

Yet is this assumption really quite indefensible, as some who make it very well know. Facts may be cited against it which show, not merely that it is a questionable, but a highly improbable one; while the evidence assigned in its favour will not bear criticism.

Because in Bohemia, Great Britain, and portions of North

America, the lowest unmetamorphosed strata yet discovered contain but very slight traces of life, Sir R. Murchison conceives that they were formed while yet few, if any, plants or animals had been created; and, therefore, classes them as "Azoic." His own pages, however, show the illegitimacy of the conclusion that there existed at that period no considerable amount of life. Such traces of life as have been found in the Longmynd rocks, for many years considered unfossiliferous, have been found in some of the lowest beds; and the twenty thousand of feet of superposed strata still yield no organic remains. If now, in so far as signs of life are concerned, these superposed strata throughout a depth of four miles are blank, though the strata over which they lie prove that life had commenced; what becomes of Sir R. Murchison's inference? At page 189 of *Siluria*, a still more conclusive fact will be found. The "Glengariff grits," and other accompanying strata there described, amounting in depth to 13,500 feet, contain no signs of contemporaneous life. Yet Sir R. Murchison refers them to the Devonian period—a period that had a large and varied marine fauna. How then, from the absence of fossils in the Longmynd beds and their equivalents, can we conclude that the Earth was "azoic" when they were formed?

"But," it may be asked, "if living creatures then existed, why do we not find fossiliferous strata of that age, or an earlier age?" One reply is, that the non-existence of such strata is but a negative fact—we have not found them. And considering how little we know even of the two-fifths of the Earth's surface now above the sea, and how absolutely ignorant we are of the three-fifths below the sea, it is rash to say that no such strata exist. But the chief reply is, that these records of the Earth's earlier history have been destroyed by agencies that are ever tending to destroy such records.

It is an established geological doctrine, that all sedimentary strata are liable to be changed, more or less completely, by igneous action. The rocks originally classed as "transition," because they were intermediate in character between the igneous rocks found below them, and the sedimentary strata found above them, are now known to be nothing else than sedimentary strata altered in texture and appearance by the intense heat of adjacent molten matter; and hence are re-named "metamorphic rocks." Moreover, modern researches have shown that these metamorphic rocks are not, as was once supposed, all of the same age; but that they belong to various ages. Not only have primary and secondary strata been transformed by igneous action, but in some places deposits of tertiary origin also; and that even for a quarter of a mile from the point of contact with neighbouring granite. By this process fossils are of course destroyed. "In some cases," says Sir Charles Lyell, "dark limestones, replete with shells and corals, have been turned into white statuary marble, and hard

clays, containing vegetable or other remains, into slates called mica-schist or hornblende-schist; every vestige of the organic bodies having been obliterated." Again, it is fast becoming an acknowledged truth, that igneous rock, of whatever kind, is nothing else than the product of sedimentary strata that have been completely melted. Granite and gneiss, which are of like chemical composition, have been shown, in various cases, to pass one into the other: as at Valorsine, near Mont Blanc, where the two, in contact, are observed to "both undergo a modification of mineral character. The granite still remaining unstratified, becomes charged with green particles; and the talcose gneiss assumes a granitiform structure without losing its stratification." To which add the further fact, that while, fifty years ago, it was thought that all granitic rocks were primitive, or existed before any sedimentary strata, it is now "no easy task to point out a single mass of granite demonstrably more ancient than all the known fossiliferous deposits." In brief, accumulated evidence clearly shows, that by contact with, or proximity to, the molten matter of the Earth's nucleus, all sedimentary strata are liable to be actually melted, or partially fused, or so heated as to agglutinate their particles; and that according to the temperature they have been raised to, and the circumstances under which they cool, they assume the forms of granite, porphyry, gneiss, or rock otherwise altered in texture. Further, it is manifest that though strata of various ages have been thus changed, yet that the most ancient strata have been so changed to the greatest extent: both because they have habitually lain nearer to the centre of igneous agency; and because they have been for a longer period liable to the effects of this agency. Whence follows the obvious corollary, that sedimentary strata passing a certain antiquity, are very unlikely to be found in an unmetamorphosed state; and that strata earlier than these are certain to have been melted up. The truth is, that if, throughout a past of indefinite duration, there had been going on those aqueous and igneous agencies which we see still at work, the state of the Earth's crust would be just what we find it. We have no evidence which puts a limit to the period throughout which this formation and destruction of strata has been going on. For aught the facts prove, it may have been going on for ten times the period measured by our whole series of sedimentary deposits.

Not only, however, is it that in the present appearances of the Earth's crust we find no data for fixing a commencement to these processes—not only is it, that the observed facts permit us to assume such commencement to have been inconceivably remote, as compared even with the vast eras of geology; but it is that we have positive grounds for inferring the inconceivable remoteness of such commencement. Modern geology has established truths which are totally irreconcilable with the belief that the formation and destruction of strata began when the Cambrian rocks were

formed, or at anything like so recent a time. One fact from *Siluria* will suffice. Sir R. Murchison estimates the vertical thickness of Silurian strata in Wales, at from 26,000 to 27,000 feet, or about five miles; and if to this we add the vertical depth of the Cambrian strata on which the Silurians lie conformably, there results on the lowest computation a total depth of some seven miles. Now it is held by geologists, that this vast accumulation of strata must have been deposited in an area of gradual subsidence: to allow of their being thus laid upon each other in regular order, the Earth's crust must have been at that place sinking, either continuously or by very small steps. It is an absolute impossibility, however, that a subsidence of this immense extent could have taken place, unless the Earth's crust had at that time a great thickness. The Earth's molten nucleus tends ever, with enormous force, to assume the form of a regular oblate spheroid. Any depression of its crust below the surface of equilibrium, and any elevation of its crust above that surface, have to withstand immense resistance. It follows inevitably from mechanical principles, that, with a thin crust, nothing but small elevations and subsidences would be possible; and that, conversely, a subsidence of seven miles implies a crust of comparatively great strength, or, in other words, of great thickness. Indeed, if we compare this inferred subsidence in the Silurian period with such elevations and depressions as our existing continents and oceans display, we see no evidence that the Earth's crust was appreciably thinner then than now. What are the manifest implications? If, as geologists generally admit, the Earth's crust has resulted from that slow cooling which is even still going on,—if we see no sign that at the time when the earliest Cambrian strata were formed, this crust was appreciably thinner than now; we are forced to conclude, that the era during which it had acquired that great thickness possessed in the Cambrian period, must have been enormous as compared with the interval between the Cambrian period and our own. But during that incalculable series of epochs, compared with which our geological epochs sink into insignificance, there existed an ocean, tides, winds, waves, rain, rivers. The agencies by which the denudation of continents and filling up of seas have all along been carried on, were as active then as now. Endless successions of strata must have been formed. And when we ask—Where are they? Nature's obvious reply is—They have been destroyed by that igneous action to which so great a part of our oldest-known strata owe their fusion or metamorphosis.

Thus, then, it results, that only the last chapter of the Earth's history has come down to us. The many previous chapters, stretching back to a time immeasurably remote, have been burnt; and with them all the records of life we may presume they contained. The greater part of the evidence which might have served to settle the Development controversy, is for ever lost; and on

neither side can the arguments derived from Geology be conclusive.

"But how happen there to be such evidences of progression as exist?" it may be asked. "How happens it that, in ascending from the most ancient strata to the most recent ones, we *do* find a succession of organic forms, which, however irregularly, carries us from the lower to the higher?" This question seems, at first sight, difficult to answer. Nevertheless, we believe it may be shown that nothing can be safely inferred from this apparent progression. And the illustration which shows this will, we believe, also show how little trust is to be placed in certain geological generalizations that appear to be well established. With this somewhat elaborate illustration, to which we now pass, our criticism may fitly conclude.

Let us suppose that in a region now covered by wide ocean, there begins one of those great and gradual upheavals by which new continents are formed. To be precise, let us say that in the South Pacific, midway between New Zealand and Patagonia, the sea-bottom has been little by little thrust up toward the surface, and is about to emerge. What will be the successive phenomena, geological and biological, which are likely to take place before this emerging sea-bottom has become another Europe or Asia? The first occurrence will be, that such portions of the incipient land as are raised to the level of the waves will be rapidly denuded by them; their soft substance will be torn up by the breakers, carried away by the local currents, and deposited in neighbouring deeper water. Successive small upheavals will bring new and larger areas within reach of the waves; a fresh portion will each time be carried off from the surfaces previously denuded; and further, some of the newly-formed strata, being elevated nearly to the level of the water, will be torn up and redeposited. In the lapse of time, the harder formations of the upraised sea-bottom will be uncovered. These, being less easily destroyed, will remain permanently above the surface; and at their margins there will arise the usual production of beach-sand, and pebbles. While in the slow process of this elevation, going on at the rate of perhaps two or three feet in a century, most of the sedimentary deposits produced will be again and again destroyed and re-formed; there will, in those adjacent areas of subsidence which accompany areas of elevation, be more or less continuous successions of these new strata. And now, what will be the character of these new strata? They will necessarily contain scarcely any traces of life. The central regions of our great oceans are bare of all but the lowest organisms—no fish, no crustaceans save of the very minute degraded types, no molluscs except the humble Pteropods. A new land raised in the midst of this aqueous desert would therefore not only be without terrestrial life, but its shores would have scarcely any marine life: with the exception of Algæ and Zoophytes, the germs of which might pretty early be brought by

drift sea-weed; its shores would be tenantless. And thus the first formed strata, like the Cambrian of our geologists, would be practically "azoic"— would contain only such occasional traces of fucoids, &c., as are found in the most ancient rocks of Great Britain and Ireland. Imagine now that the processes we have briefly indicated continue—that the emerging land, spite of denudation, becomes wider in extent, and fringed by higher and more varied shores; and imagine, further, that there still go on those ocean currents which, at long intervals, convey from far distant shores immigrant forms of life. What will result? The mere lapse of time will of course favour the introduction of such new forms: admitting, as it must, of those combinations of fit conditions, which, under the law of probabilities, can occur only at very distant intervals. Add to this, that the increasing area of the emerging land implies the increasing length of its coast; from which there follows a longer line of contact with the streams and waves that bring drifting masses, and, therefore, a greater chance that germs of fresh life will be stranded. And once more, the comparatively varied character of the shores, involving increased variety of physical conditions, will furnish suitable habitats for more numerous species. So that as the elevation proceeds, there are three separate causes conspiring to introduce additional marine plants and animals. To what classes will the increasing fauna be for a long period confined? Of course, to classes of which individuals, or their germs, are most liable to be carried far away from their native shores by floating sea-weed or drift-wood; to classes which are also least likely to perish in transit, or from change of climate; and to those which can best find a subsistence around coasts comparatively bare of life. Evidently then, corals, annelids, inferior molluscs, and crustaceans, &c., will chiefly constitute the early fauna. The large predatory members of these classes will be later in establishing themselves: not only because the creatures they prey upon must precede them; but also because, being more complex, they, or their ova, must be less likely to survive the journey, and the change of conditions. Thus the strata deposited next after those which we concluded would be almost "azoic," would contain the remains of invertebrata, allied to those found near the shores of Australia and South America. Of such invertebrate remains, the lower beds would furnish comparatively few genera, and these of relatively low type; while in the upper beds the number of genera would be greater, and the type higher: just as among the fossils of our Silurian system. As this great geologic change slowly progressed through its long history of earthquakes, volcanic disturbances, minor upheavals and subsidences,—as the extent of land became still greater, its coast-line still longer and more varied, and the neighbouring sea more thickly inhabited by the inferior forms of life; there would result a fit habitat for the lowest division of the vertebrata. In order of time, fish would naturally come after the invertebrata;

both as being less likely to have their ova transported across the uninhabited waste of waters, and as requiring for their subsistence a pre-existing fauna. They might be expected to make their appearance along with the predaceous crustaceans; as they do in the uppermost Silurian rocks. And here, too, let us remark, that as during this long epoch we have been describing, the sea would have made great inroads on those parts of the continent that had remained stationary, and would probably in some places have worked its way inland till it reached masses of igneous or metamorphic rocks, there might, in course of time, arise by the decomposition and denudation of such rocks, local deposits coloured with oxide of iron, like our Old Red Sandstone; and in these deposits might be buried the remains of the fish then peopling the neighbouring sea.

The upheaval still going on, and the continent in the vast stretch of geologic epochs becoming gradually larger, spite of denudations and local sinkings, let us now ask what would be the state of its surface? For a long period its desert of naked rocks and pebbles would bear traces only of the humblest forms of vegetable life, such as we find in grey and orange patches on our own rugged mountain sides; for these alone could flourish on such a surface, and their spores would be the most readily transported. When, by the decay of such protophytes, and that decomposition of rock effected by them, there had resulted a fit habitat for mosses, these, of which the germs might readily be conveyed in drifted trees, would begin to spread. A soil having been eventually thus produced, it would become possible for plants of higher organization to find root-hold; and as in the way we have described the continent grew larger, and had more multiplied relations with winds and waters, such higher plants might be expected ultimately to have their seeds transferred from the nearest lands. After something like a flora had thus colonized the surface, it would become possible for insects to exist; and of terrestrial creatures insects would manifestly be the first to find their way from elsewhere. As, however, land life, both vegetable and animal, is much less likely than marine life to survive the accidents of transport from distant shores; it is clear that long after the sea surrounding this new continent had acquired a varied flora and fauna, the continent itself would still be comparatively bare; and thus that the early strata, like our Silurians, would afford no traces of terrestrial life. By the time that something like the size of Europe had been reached, we may fairly suppose a luxuriant vegetation to have been acquired. Under what circumstances are we likely to find this vegetation fossilized? A large continent implies large rivers. Large rivers are liable to have deltas. Deltas, as we know from extant cases, possess a rank vegetation, and afford the conditions needful for preserving it in the shape of coal-beds. Observe, then, that while in the

early history of such a continent a carboniferous period could not occur, the occurrence of a carboniferous period would become probable after long continued upheavals had uncovered a large area. As in our own sedimentary series, coal-beds would make their appearance only after there had been enormous accumulations of earlier strata charged with marine fossils.

Let us ask next in what order the higher forms of animal life would make their appearance. We have seen how, in the succession of marine forms, there would necessarily be something like a progress from the lower to the higher; bringing us in the end to predaceous molluscs, crustaceans, and fish. We have now to consider what are likely to succeed fish. Clearly, after marine creatures, those which would have the greatest chance of surviving the voyage would be amphibious reptiles: both because they are more tenacious of life than higher animals, and because they would be less completely out of their element. Especially might we expect to be conveyed, such amphibious reptiles as can live in both fresh and salt water, like alligators; and such as are drifted out of the mouths of great rivers on floating trees, as Humboldt says the Orinoco alligators are. Thus a creature like the teleosaur of the upper Devonian rocks, is about the most likely immigrant next after the creatures that are strictly marine. It is further manifest, that reptiles; of other kinds would be among the earliest vertebrata to people the new continent. If we consider what will occur on one of those natural rafts of trees, soil, and matted vegetable matter, sometimes swept out to sea by such currents as the Mississippi, with a miscellaneous living cargo upon it; we shall see that while the active, hot-blooded, highly-organized creatures will soon die of starvation and exposure, the inert, cold-blooded ones, which can go long without food, will live perhaps for weeks; and so, out of the chances from time to time occurring during long periods, reptiles will be the first to get safely landed on foreign shores: as indeed they are even now known sometimes to be. The transport of mammalia being comparatively precarious, must, in the order of probability, be longer postponed; and is indeed unlikely to occur until by the enlargement of the new continent the distance of its shores from adjacent lands has been greatly diminished, or the formation of intervening islands had increased the chances of survival. Assuming, however, that the facilities of immigration had become adequate; which would be the first mammals to arrive and live? Not large herbivores; for they would be soon drowned if by any accident carried out to sea. Not the carnivora; for these would lack appropriate food, even if they outlived the voyage. Small quadrupeds frequenting trees, and feeding on insects, would be those most likely both to be drifted away from their native lands and to find fit food in a new one. Such insectivorous mammals as those found in the Trias and the Stonesfield slate, are those

which might naturally be looked for as the pioneers of the higher vertebrata. And if we suppose the facilities of communication to be again increased, either by the further shallowing of the intervening sea and the consequent multiplication of islands, or by an actual junction of the new continent with an old one through continued upheavals; we should finally have an influx of the larger and more perfect mammals.

Thus, then, it is manifest that the successive sedimentary deposits formed while this new continent was undergoing gradual elevation, would seem to furnish clear evidence of a general progress in the forms of life. That lands thus raised up in the midst of a wide ocean would first give origin to unfossiliferous strata; next to strata containing only the lowest marine forms; next to strata containing higher marine forms, ascending finally to fish; and that the strata above these would contain reptiles, then small mammals, then great mammals; seems to us to be demonstrable from the known laws of organic life. And if the succession of fossils presented by the strata of this supposed new continent, would thus simulate the succession presented by our own sedimentary series; must we not say that our own sedimentary series very possibly records nothing more than the phenomena accompanying one of these great upheavals? We think this must be considered not only possible, but highly probable: harmonizing as it does with the unavoidable conclusion before pointed out, that geological changes must have been going on for a period immeasurably greater than that of which we have records. And if the probability of this conclusion be admitted, it must be admitted that the facts of Palæontology can never suffice either to prove or disprove the Development Hypothesis; but that the most they can do is, to show whether the last few pages of the Earth's biological history are or are not in harmony with this hypothesis—whether the existing flora and fauna can or can not be affiliated upon the flora and fauna of the most recent geologic times.