and at length becomes about a foot long, the flowerstalks springing from its sinus. The two cotyledons grow equally for the first few days, but one of them is soon arrested while the other grows on in this remarkable manner. *S. Rexii* and *S. biflorus* show this remarkable peculiarity, but also develope a plumule of two or three smaller leaves.

(3.) The Notes on *Anonaceæ*, by George Bentham, indicate the principles adopted in the forthcoming revision of this order for the new *Genera Plantarum*, and characterizes several new species. The rather numerous instances in which the petals are imbricated in æstivation, as in *Magnoliaceæ*, are mentioned, and the æstivation is (perhaps rather too much) used in the division into tribes.

(4.) *Botanical Memoranda*, by George Bentham. In this short paper Mr. Bentham discusses several topics with his well-known ability and good sense. We must demur to his conclusion that the so-called involucre of *Anemone* answers to a single amplexical divided leaf, and would refer to the involucels of two opposite leaves in *A. Virginiana* and *A. Pennsylvanica* in proof of the contrary; nor can we regard the change from alternate to opposite or verticillate leaves as so anomalous or so unusual as in itself to give likelihood to the new hypothesis. The more elaborate ensuing note on the stigmas of *Papavaraceæ* is clear and admirable.

(5.) On *Fissicalyx*, a new genus of *Dalbergieæ* (No. 2223 of Fendler's Venezuelan collection), by the same author.

(6.) Account of the plants collected by Dr. Walker in Greenland and Arctic America during the expedition of Sir Francis M'Clintock in the Yacht 'Fox,' by Dr. J. D. Hooker.

(7.) *Hepaticæ India Orientalis*, by Mr. Mitten; commenced.

Supplement to vol. v.; *Botany*, 1860, contains the *Florida Adenensis*, by Dr. Thomas Anderson, a botanist of excellent promise; 43 pages, with 6 plates. Ninety-four species compose the known phænogamous flora of this arid little peninsula of Aden, belonging to 79 genera and 41 natural orders. Most of these species are scarce in individuals, only a few of the more arid forms predominating; all are more or less peculiar in their habit, and destitute of a bright green color; nearly all are glaucous, whitened, or hoary, many are fleshy, and 16 bear sharp thorns. "All the species have to strive against conditions tending to the entire extinction of vegetable life;" and "the flora appears to be a collection of desert species, selected from widely different natural orders and genera, and all alike contending with the excessive heat and drought." "In so dry a climate, Ferns and other Cryptogamia except Lichenes, are quite unknown." A. G.

Review.—

2. *Life on the Earth, its Origin and Succession;* by John Phillips, M.A., LL.D., F.R.S., late President of the Geological Society of London, Professor of Geology in the University of Oxford. Cambridge and London, Macmillan & Co., 1860, pp. 224, 12mo.—A book with this taking title, especially in these days, is sure of a prompt and wide circulation,—all the more so when the author is an Oxford Professor, and a Rede Lecturer at the sister University. The subject and the author here command attention and respect, and excite a high degree of expectation. We imagine that those readers who take this volume for what it really is, viz.,

an amplification of "the Rede Lecture," delivered in the year 1860, before the University of Cambridge, and as a popular exposition, by an able geologist, of an interesting scientific topic—will not be disappointed.

On the other hand, those who take it,—as the title-page might lead them to do—either for an original speculation upon the Origin and Succession of Life on the Earth, or for a serious and sustained criticism of the particular hypothesis which Mr. Darwin has recently propounded, will hardly have their expectations satisfied. Yet, along with a large amount of very good elementary exposition, some considerations are adduced, and some points are made, which are extremely noteworthy, and which show how well Professor Phillips could have discussed the whole subject, at least in its geological bearings, if he had seriously undertaken it.

The best point, as it strikes us, which Professor Phillips makes against Darwin is drawn from a comparison of fresh-water with marine Mollusca, —the latter of numerous and widely diversified types, and of great change on the whole from age to age; the former of comparatively few types, and much alike all over the world and throughout geological time as far back as they can be traced.

"If, in either of these cases, the Unionidæ, the Paludinadæ, the Limnæadæ, Planorbes, Physæ, &c., the modern forms are derived from the ancient, we have the full measure of the whole variation,—the differentials of change are all integrated by time, and we behold the sum.—how little! But if not so, if the modern and ancient species have sprung from different branches of a stem still older than either, how much stronger, if possible, is this decisive testimony against the doctrine of indefinite change through time and circumstance! Circumstances have varied, ages have passed away, and yet every generic group exhibits at every step the same essential characters, and many of the little peculiarities, such as eroded beaks, plications on the surface, reflexions of the lip, carinations of the whorls, which cannot be consistent with accumulated tendencies to change, (p. 113). "The discovery of a land-shell allied to if not identical with Pupa, in the interior of a fossil tree (Sigillaria) in the coal-formation of Nova Scotia," (p. 116), is an analogous case.

To enforce the argument we need the statement,—which we wonder Prof. Phillips has not adduced, and which we suppose may be safely ventured upon,—that fluviatile and terrestrial conditions must all along have been more variant and diverse, and therefore more favorable to the eduction and natural selection of variations according to the hypothesis, than marine. Yet while, under the comparatively uniform conditions of the latter, every thing 'doth suffer a sea-change into something rich and strange," the freshwater and terrestrial genera remain almost unaltered. Put in this form, the objection appears to us a formidable one, which we should not know how to answer. Yet, on the whole, there is, as there should be on the theory, a far greater diversity on land than in salt water. The common form of similar objections misapprehends the theory, by assuming that actual variability is something constant and equable—a uniform and measurable force acting always and upon entire species—so that so much time should bring to pass so much change; or else, that the external conditions really produce the variation;—while, also, that *com-*

plex which is condensed and rather boldly personified by the term Natural Selection is no better comprehended by many naturalists than is the Malthusian theory by many political economists.

In view of the fact that a large number of existing species appear (by the evidence of their remains) to have existed from near the beginning of the pleistocene or quaternary epoch, and that the difference between the fauna of that period and the present consists merely in the extinction of a number of species, Mr. Pictet convincingly argues that the actual geological epoch, the present creation, began at the close of the tertiary. Professor Phillips would seem to go farther; for, in view of the similar, though less universal identifications by which "the tertiary series is linked in easy harmony with the actual period," he concludes that; "The present age is in fact a part of the great cænozoic period," (p. 169, 170), under which term, as the preface informs us, he "comprehends not only the eocene, miocene, and pleiocene of Lyell, but the whole series of supra cretaceous deposits." Looking then at "examples of parallel forms of Mammalia now living with some of the tertiary quadrupeds once denizens of the same regions, or regions formerly connected by land," or where, "without this close affinity, a considerable resemblance is found between special tribes now living and others fossil in the same region," as in a part of America "among the Edentata, which though not quite confined to that region, are more plentiful there than elsewhere, and are successors of fossil races also found almost exclusively in that country;" noting also that the marsupial mammals of Australia had marsupial predecessors, our author continues:—

"The peculiarity indeed is of far earlier origin; for it occurs in the eocene deposits of the basin of Paris, in the lacustrine deposits over the upper oolite at Stonesfield, and probably in the Trias of Wurtemberg. In respect of the Stonesfield fossils, this is not the only evidence presented by that curious deposit of similarity of mezozoic life in the north and cænozoic life in the antipodal region of the south. It extends to other groups, both of the land and sea, and almost justifies the notion of some affinity even in the systems of life. For just as at Stonesfield, so in Australia, small insectivorous marsupial mammals are associated with Cycadaceous plants and Ferns; as now in the seas surrounding Australia, Terebratula and Rhynchonella, Trigonia and Cucullæa, consort with Turtles and the Cestracion Sharks, near reefs of coral, and rivers tenanted by Gavialian Crocodiles, so at Stonesfield in the older time, similar animals in similar combination.

"What does this teach us? Are we looking upon two partially similar, but really separate creations suited to partially similar conditions in very different periods of time? Or is the life-system of the modern Australian land and sea truly derived in some of its components by descent with modification from the older periods of the world, and preserved to this our day, notwithstanding displacement over half the circumference of the globe, and all the vicissitudes of an immensity of time?" (p. 171, 172).

The author proceeds to answer these questions in the following passages in which his volume culminates:

"Whoever has the courage to adopt the latter view must accept with it the obvious inference, that, in all the countless ages which have rolled away since the branches [?] of Zamia were blown into the lagoon at Stonesfield, the amount of organic change has been small in each group of plants and animals; that a similar amount of change affected the unlike inhabitants of land and sea; that Mollusca and Sharks, and Turtles and Crocodiles, have all been modified by differences of a small description in passing from Oolitic to modern times, while not only hosts of Ammonites and Belemnites have perished in the experiment, but many new forms, as Oliva, Mitra, Triton, Struthiolaria, unknown in the earlier period, have come into view in the latter. But let it be adopted. What follows? These small differences then, accomplished in all that prodigious range of elapsed time, under all that variety of physical changes and removals, these are all the mutations which have been possible under the constant tendency of hereditary descent to perpetuate similar forms with modification.

"One of these genera, that of Trigonia, is known to be in the fossil state rich in species. Supposing them all to have come from one original typical form, the differences which they show in strata of the same system, deposited within the same grand period, and under much similarity of conditions, argue a facility in giving variations: let this operation be supposed to be continued in the interval between the epoch of Stonesfield and that of Australia, and the effects summed by natural selection, the result is the modern *Trigonia*, scarcely differing more in appearance from the fossil species than they differ one from another. But, if not so derived by continual descent, but sprung from separate contemporaneous branches of one stem of life, how should it happen that plants and quadrupeds on land and mollusks in the sea, should in each of these two cases pass with equal advance along the streams of change, moving in one case so fast, in the other so slow? But if the branches sprang at different times and led to these similar results, would this double origin in time, for several similar forms in similar associations, fit with the hypothesis of continual development?"

This is neatly put. But it seems to be founded on the supposition that variation in descent is somehow *caused* by time and change, and goes on by something like equal increments in equal times; whereas, the cause of variation is wholly occult,—the fact is, that some forms remain long invariable or slightly variable under the same conditions in which others vary freely. If Mr. Darwin's theory is bound to explain variation, or to assign a reason for one species varying when another does not, then it utterly fails, for it can do no such thing. If, however, it does not undertake to account for the diversity of species except by regarding them as varieties of earlier origin and wider divergence,—leaving the reason why the progeny is sometimes unlike the parent in one or more particulars as much unexplained as why it is usually like it, but showing how the struggle for life ensures the extinction of crowds of intermediate forms, and now the resulting natural selection may lead certain surviving races farther along the lines of favorable variation,—then it avoids the force of many of the criticisms which have been directed against it.

The criticisms from which, however, it is least able to escape are those which call for lacking intermediate forms between tribes, families, and other great groups, or for some evidence that they ever existed. Here Prof. Phillips as a geologist feels his advantage and urges his point more aptly than some other critics have done.

"The explanation offered in the hypothesis of Mr. Darwin is, that the groups of life which appear to be and really are distinct, in the Cambro-Silurian rocks are not aboriginal forms, but derived from progenitors of far earlier date, belonging to few types or to one, the original form, and the transition forms being known to us. Now they are not unknown to us by any impossibility of being preserved, for the strata of the Cambro-Silurian series are of a kind in which organic remains of great delicacy are often preserved, and indeed such are preserved in these very strata; and by the hypothesis the life-structures which are lost must have only gradually differed in their nature from those which are preserved. It follows, therefore, that the earlier-living progenitors of the Cambro-Silurian series not only lived long before, but must have lived somewhere else. But as in all the known examples of this series of strata, wherever found, we have everywhere animals of the same general type, and nowhere the traces of the earlier progenitors, it is clear that everywhere we are required by the hypothesis to look somewhere else;—which may fairly be interpreted to signify that the hypothesis everywhere fails in the first and most important step. How is it conceivable that the second stage should be everywhere preserved, but the first nowhere?" (p. 214, 215.)

So, also, of what follows:

"Are we sure that varieties which are given by nature in successive generations can be *summed up in one direction* by the variable preponderant of a number of *concomitant variable conditions* of life? Can we remove 'natural selection' from the large synonomy of 'chance' except by giving to one of the variable conditions of which it is the sum, direction, definite value, or effect. Is it not the one acknowledged possession of every species, an inherent tendency to propagate its like? Would not the effect of this one constant among any number of variables without law be to preserve the characters of the species forever?" But what, we ask, is this inherent tendency of the species to propagate its like but the summing up in one direction of the tendency of each generation of individuals to propagate its like? Is not the occasional appearance of an offspring unlike the progenitor as much a natural fact as the contrary? And does not each initial variety, once originated, also have an inherent tendency to propagate its particular like, which, when it prevails for some generations, fixes a new "constant" which would equally tend "to preserve the characters of the 'variety' forever?"

"And," continues our author, "if 'natural selection' were regarded as giving direction to these variables, in combination with that constant tendency, what would be the final result but that which has always been recognized, viz: a species varying within limits which are to be sought out by experience. But finally, if natural selection be thus gifted with the power of continually acting for the good of its subject, encouraging it, or rather compelling it to continual advancement,—how is this beneficient personification to be separated from an ever-watchful providence,—

which once brought into view sheds a new light over the whole picture of causes and effects?" (p. 215, 216).

We answer, nohow, except by indicating to some extent the mode or way in which this Providence may operate.

The more we can recognize or clearly conceive the mode, the better; but, whether we can express the results of observation in the form of general laws or not, we are equally convinced that "what would be regarded as remarkable inventions if they were due to human minds and hands," "cannot be removed from the list of intelligent adaptations because they are frequent in nature, and are of higher perfection and greater beauty than any work of man;" and that "no one will ever be [or rather, reasonably can be] satisfied with laws which had no Author, works which had no Maker, co-ordinations which had no Designer."

And our author does simple justice to Lamarck in giving him the benefit of his own averment that "by Nature we are to understand a certain order of causes and effects constituted by the will of the Supreme Author of all things." The points against Darwin's theory made or suggested in the present volume, with so much acuteness, are all the more telling for the entire fairness and excellent spirit in which they are made. This is far more than can be said of the following essay, viz:

3. *Species not Transmutable nor the Result of Secondary Causes; being a Critical Examination of Mr. Darwin's Work*, &c; by C. R. Bree, Esq., M.D., F.L.S., &c.—London, Groombridge & Sons.—A favorable notice in the Athenæum of Dr. Bree's volume led us to suppose that it might be a contribution of some importance in the discussion of the nice questions which the publication of Mr. Darwin's book has raised. But this expectation has not been fulfilled on perusal. The author's intentions are praiseworthy, and his zeal in a good cause exuberant. But we cannot entertain a great respect for the reasoning of a writer who, on the one hand sees design and adaptation in the distribution of sunshine and rain, and the succession of the seasons, while on the other he insists that *because* "all the parts of a creature act harmoniously and co-ordinately one with another," necessitating the inference "that they were preordained to act collectively for the animal," therefore "they could not have been produced by [through] variation, natural selection, divergence of form," or indeed through any secondary causes whatever. We are unable exactly to comprehend how one who sees design and adaptation realized in the inorganic world through what are called secondary causes, is entitled to declare that the establishment of the doctrine of the succession of species,—each marked with more special if not stronger evidences of design than anything in inorganic nature,—through secondary causes, would "destroy every vestige of a shadow of belief in a watchful Providence and adaptive creation, and strike deeply and irrecoverably at the root both of natural and revealed religion." In our opinion such defenders of the faith play unwittingly into the hands of its most dangerous adversaries.

A. G.