Amongst many French physiologists, of whom we may mention Milne-Edwards, the doctrine of the muscular irritability is most in favour, and the evidence afforded in support of it by the experiments of Claude Bernard and others appears to give weight to the theory. They have shown that it is possible by the administration of certain poisons to arrest the action of the nervous system whilst the muscular irritability remains, and, on the other hand, to annihilate the muscular irritability whilst the function of the nerves is persistent. These experiments do not appear to us decisive of the question, for in those cases where the administration of a poison has resulted in suspending one or more of the properties of the nervous system, other faculties may remain, and we do not know, with reference to the heart, how far the function of its own ganglia may or may not be interfered with. For our own part, we are disposed to consider the theory of the discharge of nerve-force from the ganglia of the heart as the true one. That these ganglia are dependent on rhythmical nutrition for the power they rhythmically discharge we entertain no doubt; and this nutrition must take place during the period of repose which the structures enjoy. The nervous force thus generated is discharged at the proper time, and contraction ensues. That there is at all times a reserve of this force which will keep up rhythmical movements for a longer or shorter period when the source of nutrition is cut off, we can easily understand, and in proportion as the heart, as well as other viscera, is more or less influenced by the great nervous centres, so will there be a greater or less dependence of the organ on other sources, except those within its walls, and so shall we find that its rhythmical action will continue when it is removed from the body, for a longer or shorter time.*

Review V.

1. *On the Origin of Species by means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life.* By Charles Darwin, M.A., F.R.S., F.G.S., F.L.S., &c.—*London*, 1859. Post 8vo, pp. 502.

2. *On the Tendency of Varieties to Depart indefinitely from the Original Type.* By Alfred Russel Wallace. From 'Journal of the Proceedings of the Linnæan Society,' July 1st, 1858.

3. *Essays on the Spirit of the Inductive Philosophy, the Unity of Worlds, and the Philosophy of Creation.* By the Rev. Baden Powell, M.A., F.R.S., F.G.S., Savilian Professor of Geometry in the University of Oxford.—*London*, 1855. Post 8vo, pp. 503.

* Since this article was written, we have received an essay on "The Action and Sounds of the Heart," by Dr. Halford. In addition to the facts brought forward in the papers we have referred to, the essay contains some valuable observations on the sounds of the heart as heard in birds. We are disposed to agree with the author that the facts as detailed by him are quite in favour of the valvular theory, but for further information we must refer our readers to the essay itself, which will well repay an attentive perusal.

4. *Introductory Essay to the Flora of New Zealand.* By JOSEPH DALTON HOOKER, M.D., F.R.S., F.L.S., F.G.S., &c., late Botanist to the Antarctic Expedition.—*London*, 1853. 4to, pp. xxxix.

5. *On the Flora of Australia, its Origin, Affinities, and Distribution; being an Introductory Essay to the Flora of Tasmania.* By JOSEPH DALTON HOOKER, M.D., &c. &c.—*London*, 1859. 4to, pp. cxxviii.

THERE is no subject more fertile in suggestive questions, and more capable of exercising the highest powers of the mind in the search for answers to them, than Natural History. There was a time, it is true, when the Botanist confined himself to collecting and drying plants, and arranging them in a herbarium according to such notions of their mutual affinities as he might be able to form from their external characters; their collocation, so long as the Linnæan system was in vogue, being generally about as natural as that of the successive articles in an Encyclopædia arranged according to the alphabetical order of their subjects. And the Zoologist of that epoch was content with filling glass cases with stuffed beasts and birds, putting reptiles and fishes into jars of spirit, and fastening down shells, insects, and star-fishes on the tablets of his museum; knowing little and caring less about their internal structure, and considering every other study but that of their external characters as absolutely profitless. Although this type is now pretty nearly extinct, one meets every now and then with an antiquated specimen of it; and its peculiarities are then brought into marked relief, by the contrast they present with the modes of thought which prevail among the best Naturalists of the present epoch.

The first great step in advance was undoubtedly made by those who showed that no classification of Plants and Animals can have any real value, which is not based on a knowledge of their *internal structure*: hence arose the Natural Method of Botanical arrangement, which, originating with Linnæus (who himself looked upon his artificial system as merely provisional and temporary), has been successively elaborated by Jussieu and Decandolle, Brown and Lindley, and other eminent systematists: and hence arose the 'Règne Animal' of Cuvier, and the 'Histoire Naturelle des Animaux sans Vertèbres of Lamarck,' which have constituted the bases of all subsequent attempts at Zoological systematization down to the present time. But within the last quarter of a century a new idea has been introduced into the Sciences of Classification; that, namely, of *development*. It is no longer regarded as sufficient to ascertain all that can be made out of the organization of the perfected type; for the completest knowledge of this, it is now fully admitted, would often leave us quite in the dark as to the real affinities of the organism. It is necessary to study the progressive stages by which that type has been attained; for often it is only in the earlier of these, before the commencement of aberrations which afterwards tend to obscure and perplex those affinities, that its true relations can be unmistakeably determined.

But the aims of the Philosophic Naturalist are not by any means

confined to the building up a classification of the existing forms of Animal and Vegetable life. He knows well that however complete may be his collections of the Plants and Animals now existing, they only represent but a fragment of the vast scheme of Creation, which has peopled the globe with continually varying forms of life, during that long succession of geological ages which has elapsed since the remains of organized beings were first entombed in the sediments of the ocean-waters which now constitute the oldest of the palæozoic strata. On bringing together all the fragmentary traces which he can collect of the successive Floræ and Faunæ of the great "formations" distinguished by the geologist as marking separate periods, he finds that they fit in so marvellously with the arrangement of the existing groups,—some of them dropping at once (so to speak) into vacant spaces that seem as if purposely left for them, and others being easily accommodated by a rearrangement which makes the new grouping far more symmetrical than the old,—that they all obviously constitute parts of one harmonious system. But this unfortunately never can be fully understood by Man; because the utmost skill of the palæontologist, though it may reconstruct a vetebrate animal from a fragment of a bone, a molluska, crustacean, or an echinoderm from a portion of its testaceous covering, a palm or conifer from a fragment of its woody stem, a fern by its leaves, or a cycad by its fruit, cannot thus reproduce any of that innumerable multitude of forms of Animal and Vegetable life which have "died and left no sign" for want of tissues hard enough to resist decay, and to whose past history, therefore, we can never, in the very nature of things, obtain the slightest clue. Circumscribed as they are, however, not only by this unavoidable restriction, but also by many other limitations necessarily arising out of the conditions under which fossil remains are preserved (to some of which limitations we shall hereafter refer), the researches of Palæontologists have been so successful, as at first to have led some of the more sanguine among them to suppose themselves justified in describing the Fauna of each successive epoch as if they had it all before them, instead of possessing such a fragmentary representation of it as any one would form of the Fauna of the present epoch by bringing together the remains of animals dredged from a small area of the sea-bottom in a dozen or two of different localities. The absurdity of the latter procedure would be scarcely more palpable than is that of the Geologists who attempt to go one step further than facts warrant, and who, not content with cataloguing the species they find in any system of formations, assume that all other types of life were absent when these were in process of deposition.

A more philosophical spirit, however, is now prevalent,—thanks, in great part to the labours of Sir Charles Lyell; and it is coming to be generally felt that the whole fabric of geological doctrine which rapidly grew up during the first third of the present century, needs to be greatly modified to bring it into accordance with the results of those more extended and careful researches in which the second third has been so fruitful. And we do not think that we can better introduce the

subject of the very important inquiry opened out to us by the remarkable treatise at the head of our list, than by noticing some of the considerations most directly bearing upon it, which arise out of the existing aspect of the inquiry into the past history of the earth, and the successive steps by which it came to present not merely its present physical features, but the very peculiar distribution of animal and vegetable forms which people its surface. The problem of the geographical distribution of living beings is in fact the one which just now possesses the very highest interest alike to the Naturalist and the Geologist; for it involves the whole question not only of *what is*, but of *how it came to be so*; bearing, in fact, just the same relation to Botany and Zoology *per se*, that Physical Geology does to Geography. And we consider the opening-up of the new ideas and new objects of inquiry in this direction, which we owe especially to the genius of the late Professor Edward Forbes, as the most important advance which has been made in the philosophy of Natural History previously to the publication of Mr. Darwin's treatise on the Origin of Species.

The unequivocal tendency of this inquiry, so far as it has been yet prosecuted, is to make evident the intimacy of the relation between the present order of things and that which preceded it, and the gradational nature of the changes by which the latter has given place to the former. It may be stated with the highest probability, from the evidence of fossil remains, that a very considerable proportion of those classes of animals now living, whose bones or shells afford means of comparison, *are* the direct descendants of animals that existed before the occurrence of those last great changes which gave to a large part of the surface of the globe its present physical features. All save a few palæontologists are now agreed that even in the earliest of the formations which succeeded the Chalk, a considerable number of shells belonging to existing species present themselves; and that the proportion goes on progressively increasing to the present time. In the case of the Mediterranean Fauna, the very curious result appears deducible from a careful comparison of the present with the former distribution of its mollusks, that all the existing species proper to it have come down from that very remote period when it was a great inland lake, these being found fossil in the successive tertiary deposits of its shores; whilst those whose descent cannot thus be traced are immigrants from the Atlantic, as is indicated not merely by their identity with species characteristic of the Boreal, Celtic, Lusitanian, and West African provinces respectively, but also by the progressive diminution in their proportional abundance as we trace them from the Straits of Gibraltar towards the Levant. The changes in climate which have favoured this intermixture (the extension of a glacial temperature to the south of Europe, for example, having at one period brought the Boreal fauna down to the entrance of the Mediterranean) have caused the extinction of many of the earlier tertiary species of the Mediterranean Province; and thus its present Fauna has come to differ widely from that of the early tertiary period, without affording any

evidence of a "new creation" of species.* Now this modification has obviously been the result of geological changes of the most gradual nature, which have been in continuous operation through the whole of the tertiary period, and which have left their traces (as Professor E. Forbes long since showed) upon the vegetation of Europe, as well as upon the distribution of its Marine Animals.

In like manner it has been recently shown to be an almost inevitable deduction from the present distribution of land animals in the Malay Archipelago, that most important and extensive geological changes have taken place since the islands at present forming that Archipelago were peopled with their existing inhabitants. The two western and eastern halves of that Archipelago, the former containing Sumatra, Java, and Borneo, the latter including Celebes and New Guinea, are separated at their nearest approximation by the Straits of Lombok, which are no more than fifteen miles wide; the fauna of the former is essentially Asiatic, that of the latter essentially Australian; and there is no other intermixture between them than such as a very limited migration across this narrow channel will readily account for. Now the various portions of the Indian province are still connected by a vast submarine plain, which extends over the whole of the Java Sea, the Straits of Malacca, the Gulf of Siam, and the southern part of the China Sea, at a depth of not more than 300 feet, abruptly terminating at its limits in an unfathomable ocean. An elevation of the sea-bottom to this amount, therefore, would nearly double the extent of tropical Asia; and there is every probability that the continent *was* thus extended before that last great elevation of the volcanic range of Java and Sumatra took place, which (according to the general fact first brought into notice by Mr. Darwin, of an alternation of bands of elevation and depression) was coincident with the subsidence that separated those islands from Borneo on the one side, and from the continent of Asia on the other. On the other hand, the great Pacific Continent, of which New Guinea and Australia are doubtless fragments, and which (as Dr. Hooker has rendered probable by botanical considerations) once connected Australia and New Zealand with South America, seems to have extended itself as far westward as the Moluccas; and *its* submergence, producing the limitation and separation of the great islands of the South sea, seems to have taken place before the rise of the tropical Asiatic continent.† There are even indications that the tropical Indian continent extended so near to what is now the coast of Africa, that Bourbon and the Mauritius, perhaps even Madagascar, were outlying portions of it; and if the submergence which formed the bed of the present Indian Ocean should have taken place subsequently to the time when these countries became inhabited by Man, we have a rational explanation of the fact which has perplexed all ethnologists, and which the hypo-

* See the recently published "Natural History of the European Seas," by the late Professor Edward Forbes and Robert Godwin-Austen.

† See Wallace "On the Zoological Geography of the Malay Archipelago," in Proceedings of the Linnæan Society for Nov. 3rd, 1859.

thesis of migration can scarcely be stretched far enough to account for,—that the languages of Madagascar are not African but Malayo-Polynesian in their fundamental affinities.

Now, there is no reason whatever for the belief that what is true of the later, is otherwise than true of the earlier periods of Geological history. The more we know of the nature of that history, the more obvious does it become that it is one of continuous sequence, not of fits of alternating activity and repose. This was well expressed thirteen years ago by the then President of the Geological Society, Mr. Leonard Horner:

"By whatever names we designate geological periods, there appear to exist no clearly-defined boundaries between them in reference to the whole earth. Such a marked line may be seen in particular localities; but every year's experience, and our more intimate acquaintance with the phenomena exhibited in different countries, and with the distribution, structure, and habits of animals and vegetables, teach us that there is a blending, a gradual and insensible passage from the lowest to the highest sedimentary strata, particularly in respect of fossil remains. The terms we employ to designate formations can only be considered as expressing the general predominance of certain characters to be used provisionally, as a convenient mode of classifying the facts we collect."

And what was thus foreseen by a sagacious reasoner upon the facts then known, has received the fullest confirmation from the results of subsequent researches; which have uniformly tended to show that the supposed boundaries are local, not universal, and that even the widest chasms close together if we trace them far enough.

Again, it may be considered as a legitimate deduction from recent Palæontological inquiry, that it is altogether unphilosophical to attempt to fix the epoch when any particular type of animal or vegetable life first appeared upon the earth. Not one such determination has been found to stand the test of more extended research. It was at one time the orthodox creed that no Mammal was created before the commencement of the Tertiary period, and no Reptile before the middle of the Secondary; simply because no remains of such had been found in the few and limited explorations then made. But we have now abundance of remains of Mammals in the Secondary period, some of them dating back to its commencement; whilst of the existence of Reptiles there is evidence very far back in the Palæozoic. Who now will be bold enough to say that there were no Mammals earlier than the New Red Sandstone, or Reptiles earlier than the Old Red?

Again, the prevalent notion that particular species are to be held as characteristic of particular strata, has been shown to require great modification by the discovery that many are really common to a long series of stratified deposits, not even being limited to the great "formations;" and that the time and order of their appearance are by no means the same in different parts of the globe. Thus, among the Palæozoic species common to Europe and to America, some are found to make their appearance first in Europe, others first in America; so that the order of their succession is reversed in these two regions.

Still more remarkable is the recent discovery of M. Barrande,' that species hitherto considered as peculiar to and characteristic of the newer palæozoic, present themselves in "colonies" (as he not very appropriately terms them) in the midst of those of the older, which afterwards replace them. It is obvious that in all palæontological reasoning, large allowance has to be made for change of geographical distribution. A species or group of species may entirely disappear from one province, in consequence of climatic or other change, and yet may have its existence continued in some other region to which it has retreated. Thus there are many species of shells found fossil in Europe, representing its fauna during the Glacial period, which are now met with alive only in the Arctic seas. On the other hand, of the large Foraminifera which built up the Nummulitic limestone of the Paris basin and of Southern Europe in the early Tertiary period, when the climate seems to have been much warmer than at present, though the greater part may have become extinct, yet some species still exist in the Pacific ocean, and are now building up reefs and islands there, which a geologist of some future epoch, relying too much on the identity of specific forms, might regard as contemporaneous with the great nummulitic formation of Europe. Thus the prevalent idea that there was a new and special creation of species with every one of those marked changes in the physical surface of the globe which has given rise to a distinct "formation," proves to be inconsistent with truth; the difference of Fauna between one formation and another being often, in great part at least, the result of migrations occasioned by alterations in climate or in those other conditions which affect the existence of animals.

Another general fact of great importance in this inquiry, is the constant correspondence which presents itself, alike as to similarity and to difference, between the physical conditions under which consecutive strata were deposited, and the collective aspect of the organic life which is made known to us by the remains they entomb. Every one who possesses but a smattering of geological knowledge well knows that each great "formation," such as the Silurian among the palæozoic, or the Cretaceous among the mesozoic, is really made up of a long series of stratified deposits, which are often very different in mineral characters, but which are for the most part conformable to one another stratigraphically, and of which the fossil Fauna and Flora present the same general features. Yet with this general conformity we encounter a marked change in detail, in passing from the beginning to the end of the series; this sometimes appears abrupt enough in this country to constitute a decided break, such as that which has been supposed to intervene between the Upper and Lower Silurian or the Upper and Lower Chalk; but in other instances a very close conformity is maintained throughout, even where identity ceases, by the successive appearance of what have been termed "representative species." From what has previously been stated, it is obvious that one essential difference between the Upper and Lower Silurian can no longer be maintained; and a careful examination of the Cretaceous series in

localities where it is more complete than in our own country, shows that it may be divided into eight stages, each having a fossil Fauna of its own, which, though peculiar as to its species, yet bears so extremely close a resemblance to that which preceded it, as strongly to suggest, even to such an orthodox believer in the immutability of species as Prof. Pictet, the notion of its derivation from it by direct descent.

Where, on the other hand, there is a marked change in the type of Life between successive deposits,—such, for instance, as that which distinguishes the Devonian formation from the Silurian that preceded it and from the Carboniferous that followed, or the Oolitic from the Triassic and the Cretaceous,—there is always ample evidence of vast intervening changes in the physical conditions under which those deposits were formed. And this evidence seems the most complete (in those areas at least which have been hitherto most carefully examined) in regard to those two great interruptions to the general continuity of the series, which are considered by Geologists to divide the Palæozoic from the Secondary, and the Secondary from the Tertiary. But we feel sure that we speak the conviction of all such Geologists as are not so far wedded to their earlier notions as to be unable fairly to estimate the merits of more recent views, when we say that they look with confidence to future discoveries as likely to bridge over both these chasms; important advances having been made, indeed, within the last few years. Thus the current doctrine has been, that the true palæozoic forms all became extinct with the completion of the Permian formation; and that the formation of the Triassic or New Red Sandstone commenced the Secondary period with a great scantiness of animal and vegetable life, which gradually gave place to the abundance of new forms characterizing the Middle and later Secondary period. But it now appears from the careful study of the remarkable beds belonging to the *Upper* Trias at St. Cassian in the Austrian Alps, that the fossil fauna of that period is really extremely rich; its supposed scantiness being simply due to the fact that in England, France, and Upper Germany the Upper Trias is chiefly represented by beds of fresh or brackish water origin. Now the St. Cassian beds, which are marine, contain a large number both of those Palæozoic forms which had been supposed to have died out long before, and of those Secondary forms which had been regarded as of much later introduction; thus showing the really gradational nature of the transition from one fauna to the other. And, as Sir C. Lyell justly remarks, "we can now no longer doubt that, should we hereafter have an opportunity of studying an equally rich marine fauna of the age of the *Lower* Trias, the great discordance between Palæozoic and Neozoic forms would almost disappear, and the distance in time between the Permian and Triassic eras would be very much lessened in the estimate of every Geologist." So the transition from the Secondary to the Tertiary series appears likely to be made by the great Nummulitic formation of Southern Europe and its associated beds, when these shall have been thoroughly worked out.

We cannot better sum up the results of the inquiries to which we have alluded, than in the words of Prof. Powell:—

"In all those geological periods during which we *can* trace a continuous and gradual succession of formations without marked or violent interruptions, there we invariably find a like slow and gradual change of animated life, proceeding by small modifications of *species*, until at length, comparing the extremes of the series, whole genera may be changed. If, then, in certain other cases, we find apparent *interruptions in the order of species*, apparent breaks in this orderly succession, or between such deposits of so different a character, periods intervening, during which we see that great changes or disturbances were in progress, as we must infer that those changes went on by the regular operation of physical laws, exactly as in the cases in which we *have* uninterrupted evidence,—so, by parity of reason, we must infer that the like gradual and regular changes of species went on during those periods, though all its intermediate links and steps are lost to us, and only the extreme terms are preserved. . . . . *A wide organic difference between two contiguous beds would only mark the longer interval of time between their deposition.*" (pp. 346–349.)

How entirely destitute we are of any title to draw inferences as to what forms of Plants or Animals *did not exist* at any particular epoch, from the apparent absence of their fossil remains, is every now and then made obvious by some unexpected discovery which throws an entirely new light on the history of the period. The researches of thirty-six years—from 1818, when first a lower jaw from the Stonesfield Oolite was pronounced by Cuvier to be Mammalian, to 1845, when the Spalacotherium of Purbeck was described by Owen,—had only disclosed the existence of six species of Mammalia in the whole world from rocks older than the Tertiary. Yet in 1856 and 1857 the careful examination of the thin seam of the Purbeck strata in which the remains of the Spalacotherium were found, brought to light an accumulation of bones of small Mammals, chiefly Marsupial, some insectivorous or predaceous, one purely herbivorous, and others of doubtful affinities, such as clearly shows that there must have been a great abundance and variety of Mammalian life at the period when this bed was deposited. And what makes the lesson the more instructive is the fact, that the Purbeck strata had been previously supposed to have been thoroughly studied by such excellent geologists as Prof. E. Forbes (who worked at them for months consecutively) and by other skilful collectors; that their fossil remains had been separately examined and catalogued by the officers of the Government Survey; and that from the circumstance of their being nearly all of fresh-water origin, yielding insects and fruits, with the stems and roots of trees, it had been anticipated that they would be likely to furnish remains of terrestrial quadrupeds, if any such had existed in that region when these beds were deposited. And yet, though thus interrogated by skilful inquirers, the rocks were silent; until one thin layer of a few inches in thickness—like a single page in a pile of volumes heaped to the height of a mountain,—revealed the memorials of fossil mammalia so numerous and diversified as not merely to surpass those found in all the other secondary rocks put together, but to outnumber those at present known from many a subdivision of the tertiary series.

So, again, the received canon as to the non-existence of Man upon the globe until the completion of the last great changes which gave to its surface its present aspect, has lately been overthrown by the discovery of unquestionable specimens of his handiwork under circumstances which necessitate our carrying back his origin to a period anterior to that at which extensive and important changes of level took place, forming a series of heights through which new river-beds have since had to cut themselves,—a process which, according to all rational probability, must have occupied almost as many thousands of years as Man is commonly supposed to have lived centuries. And though the inference can not yet be regarded as certain, there is a strong probability that the men who shaped the flint implements to which we refer, were contemporaneous with the Mammoth, the Tichorhine Rhinoceros, and other extinct Mammals whose bones have been found associated with these implements in the same gravel deposits or in the contents of the same caves.

Now when due weight is given to these and other considerations of the like tendency, it obviously becomes very difficult to form any rational conception as to the introduction of new types of organic life in any other mode than by *descent with modification* from those previously existing. We know that physical changes of the same order with those which formerly modified the condition of the earth's surface, are still in progress; if new *creations* of species have taken place from time to time even subsequently to the introduction of man, why should they not occur now? Yet would any one be bold enough to affirm that such new creations occur in our own day? It is true that if a collector meets with a form not previously described, he entitles it a "new species;" but by that title he means only a species new to science; and he would not on any account be thought to imply that it has not existed from the beginning of the present order of things.

As Dr. Hooker has well remarked:—

"The boldest speculator cannot realize the idea of a highly organized plant or animal starting into life within an area that has been the field of his own exact observation and research; whilst the more cautious advocate hesitates about admitting the origin of the simplest organism under such circumstances, because it compels his subscribing to the doctrine of the 'spontaneous generation' of living beings of every degree of complexity of structure and refinement of organization."

And he adds in a note:

"It is a curious fact (illustrative of a well-known tendency of the mind) that the few writers who have in imagination endeavoured to push the doctrine of special creations to a logical issue, either place the scene of the creative effort in some unknown, distant, or isolated corner of the globe, removed far beyond the ken of scientific observation, or suppose it to have been enacted at a period when the physical conditions of the globe differed both in degree and kind from what now obtains; thus in both cases arguing *ad ignotum ab ignoto*."*

The *extinction* of species is now universally admitted to be a gradual

* Flora of Australia, p. xxvi.

process, depending upon a variety of agencies, of which sometimes one, sometimes another plays the principal part. Many examples of it have occurred during the short period which has elapsed since the interposition of Man has disturbed the previous equilibrium. And no one would now dream of calling in the aid of general destructive catastrophes to account for these successive disappearances, which have been coincident with the successive appearances of new forms at past epochs. On that old doctrine of a succession of convulsions, each of which swept the globe of its living inhabitants, and left it ready to be repeopled afresh, there was no more difficulty in imagining a general renewal of the creative *nisus*, than in conceiving of that by which the first-created forms were introduced. But the hypothesis of occasional and general cataclysms having now given place to an induction based on a far surer foundation of evidence—that, namely, of a continuity of change, more rapid in some regions, less sensible in others, but not less certainly in progress at the present epoch than in times past,—it seems almost necessarily to follow (as has been extremely well urged by Professor Baden Powell) that the succession of forms of Organic Life has been alike gradational, both as to the extinction of the old and the production of the new, and has been determined by causes still operative. If any one should be bold enough to maintain that a production of animals or plants *de novo* does every now and then occur within human experience, he would be fully justified in attributing the introduction of new forms at any antecedent period to a like agency. But if the notion of such new developments in our own period be scouted as unscientific, presumptuous, atheistical, and the like, it is for the advocates of successive creations in past times to show that they deserve any other character. If, on the other hand, it can be shown that the existing forms of Plants and Animals have undergone such modifications within the limits of human experience, as to justify the idea that in a longer succession of ages and under a greater diversity of conditions, those modifications might have been carried to the extent of producing differences such as those by which species and genera are ordinarily distinguished, it is obvious that a legitimate basis is afforded for the inquiry whether this has not to be accepted as a *vera causa* adequate to account for the phenomena of palæontological succession, and whether the hypothesis of successive creations of living beings has really any better foundation than that of a succession of destructive convulsions.

We believe that the time is now fully come for such an inquiry to be taken up and prosecuted to its utmost limits. The subject, it is true, is by no means a new one, and is popularly believed to have been disposed of by the refutation of the fallacies of those who in times past have advocated the doctrine of the transmutation of species. With this doctrine the name of Lamarck is commonly and not unjustly associated; not because it originated with him, but because he first gave it a scientific aspect, and advocated it on the basis of an extensive and profound acquaintance with the Natural History both of Plants and Animals. But to this association no small amount of ridicule and

of misrepresentation is attached, of which we feel called upon to take this opportunity of examining the grounds. In enumerating the causes which tend to produce modifications of animal form and structure, Lamarck unfortunately laid great stress upon the efforts which the being would itself make to execute some new action, as causing the development of an appropriate organ,—the attempt to fly, for example, bringing wings into existence, the swimming of fishes causing the production of fins, and the continual stretching of the giraffe's neck in reaching the food it most liked being the cause of its elongation. Now this absurdity is often quoted as the essential part of Lamarck's theory, whereas it is only an accessory part of it, applicable to the animal kingdom alone, and especially to its higher types. All that Lamarck says of the tendency to vary, which shows itself in Plants and Animals generally, and which is the real basis of his doctrine of transmutation, might be urged by the most philosophic botanist or zoologist of the present time.

Again, the doctrine of transmutation is commonly regarded as atheistical; and Lamarck has been branded as an atheist for upholding it. Yet nothing can be more unfair, as is obvious from his own very explicit statement on the subject:

"Doubtless," he says, "nothing exists but by the will of the sublime Author of all things. But can we assign to him rules in the execution of his will, and fix the method which he has followed? Has not his infinite power been able to create *an order of things* which should successively give existence to all that we behold, as to all that which exists but of which we have no cognizance? Assuredly, whatever may have been His will, the immensity of His power is always the same; and whatever be the manner in which that Supreme Will has been exercised, nothing can detract from its greatness. Reverencing, therefore, the decrees of that Infinite Wisdom, I limit myself within the boundaries of a simple observer of nature. Hence, if I should succeed in clearing up any part of the course which it [Nature] has followed in effecting its operations, I shall say, without fear of deceiving myself, that it has pleased its Author that it should have this faculty or that power."

What can be more truly philosophical, or more truly religious? We cannot suppose that the virulent detractors from the merits of this great man,—who was not merely a Botanist of vast acquirements and did much for the establishment of the Natural System, but who possessed a knowledge of the Invertebrate Animals far surpassing that of Cuvier,—can have read more than those sections of his work which fairly lie open to adverse criticism; the larger part of them, we feel pretty sure, know it only at second hand; and we believe that the day is not far distant when it will be admitted that his great misfortune was in living in advance of his time. Nothing can be more clear and precise than his advocacy of that doctrine of local and gradational change (in opposition to the then current notion of general catastrophes), the establishment of which will carry the name of Sir Charles Lyell down to posterity as that of the great reformer of Geological science. The intimate connexion on which we have been insisting, between the successional modifications of the physical conditions of the

* Philosophie Zoologique, tom. i. p. 56.

globe, and the changes which its living inhabitants have undergone,—a connexion whose intimacy speaks strongly of a causative relation between the two orders of facts,—was discerned by the sagacity of Lamarck, though he had not a tithe of the present evidence on which to rest it. In short, there is scarcely a consideration suggested by the recent progress of Geological inquiry which his far-sightedness had not glimpsed; and the Philosophic Naturalist may still have recourse to his much-abused work for suggestions of the highest value in the prosecution of the inquiries to which his attention must now perforce be given.

It has been unfortunate for the doctrine of "transmutation," that its most prominent advocate in our own day should be an author much more distinguished for the ingenuity of his reasonings and the cleverness of his style of exposition, than for the accuracy of his knowledge of facts. We have ourselves felt called upon to criticise with some severity the shallow assumptions and specious arguments of that brilliant but unsound book 'Vestiges of the Natural History of Creation;' but our criticism was less directed to the fundamental doctrine, than to the grounds on which it was advocated; and we have uniformly done our best to resist the clamour raised by theological prejudice against the book and its author, on the asserted ground of their irreligious character. To us it has always appeared that the question ought to be discussed upon its scientific merits alone, and that the evidence of Creative Design is just as great upon one hypothesis as upon the other. Nobody would think of advancing it as an objection to modern Embryology, that it teaches that the human infant, instead of first coming into existence as a fully-formed though minute *homunculus*, begins life in the condition of the simplest protozoon, and successively acquires those peculiarities of organization which end in constituting him a Man. And we do not suppose that the naturalist who first found out that butterflies and beetles were caterpillars in the earlier stage of their existence, instead of coming out from the egg in the full possession of their insect attributes, was considered on that account less religious than his neighbours. Why, then, should it be regarded as impious to maintain that an analogous development went on during what may be called the life of the world; and that the existing forms of Plants and Animals have originated by genetic descent with modification from those which preceded them, even as the latter did from yet older forms, and so on, back to the beginning of Life on our planet? To deny that such *might* have been the Will of the Creator, is virtually either to deny that His power is constantly exerted in maintaining that regular succession of similar forms, on which the notion of the "permanence of species" is based, or to set limits to the exercise of that power, by asserting that it could not have been exerted in any other mode than that which Man chooses to prescribe.

We could cite passages from the recent writings of many men of high scientific reputation,* which would show that they regard the question of the immutability of species as by no means settled in the

* See, for example, Sir H. Holland's Medical Notes and Reflections, p. 22.

affirmative. And we know, from the many expressions of concurrence in the fundamental principles now advocated by Mr. Darwin, which the publication of his views has called forth, that there was a more general preparedness for their reception than had been supposed; the minds of thoughtful men being open to any suggestion which should furnish a clue that might help us to trace the connexion between existing and antecedent races, and might bring into reconcilement past modification and present fixity.

It is not a little singular that the same solution of this problem should have independently occurred to two Naturalists, Mr. Darwin and Mr. Wallace; each having been apparently led to it by the study of the phenomena of the geographical distribution of animals. In the philosophic mind of Mr. Darwin (there are few men of science in our own country at the present time, who have so justly earned a title to the honourable designation of philosopher), the idea was at first cautiously entertained; it was gradually developed into a systematic form, and subjected to a great variety of tests; and when its author had satisfied himself of its soundness, he applied himself for several years, during such time at least as his feeble health permitted him to labour, to the preparation of a work which should contain not only an exposition of his views, but a full statement of the evidence on which they are based. In the mean time, however, Mr. Wallace transmitted from the East Indian Archipelago, the scene of his zoological labours, a memoir containing a concise exposition of the very same doctrine; and the publication of this memoir in the 'Proceedings of the Linnæan Society' was accompanied by extracts from Mr. Darwin's work, which had been written several years previously. The importance of at once giving to the world a fuller statement of his views than those extracts afforded, was urged upon Mr. Darwin by his friends; and it is to these circumstances that we owe the earlier appearance of a more compendious treatise on the Origin of Species than that which Mr. Darwin had originally planned, and which he still hopes to produce so soon as his health and strength allow him to complete it.

The fundamental positions taken up by Mr. Darwin may be concisely stated as follows:

1. Although it is the general fact that the characters of each type of Plant or Animal are transmitted unchanged from parent to offspring, yet trivial departures from that type are continually presenting themselves, and more important variations every now and then occur.

2. So long as a free intermixture of individuals is kept up, and external circumstances remain unchanged, the larger as well as the smaller variations are usually merged (so to speak) in the general average, and the specific type remains unaltered.

3. If, however, the individuals which are distinguished by any peculiarities of conformation be kept separate from the rest, and be caused to breed together, those peculiarities will be established and perpetuated as the characters of a new race, which will remain distinct from that of the parent stock so long as it is not allowed to breed with

it. It is by such *artificial selection* that all our breeds of domesticated animals have been engendered; the breeder taking advantage of any new peculiarity which he thinks he can turn to useful account, and keeping the animals which present it apart from all others, unless he aims, by "crossing" his breed with some other, to get rid of some undesirable feature, or to introduce some desirable attribute.

4. Although the condition of feral or wild races is so entirely different from that of the races under the influence of Man, as at first sight to exclude the notion that the perpetuation of varieties can be effected by any such selective agency, yet that very difference of conditions brings them under a new set of influences, which will tend to produce an analogous result. The life of all wild animals is a *struggle for existence;* and their relative abundance and power of maintaining their ground is determined far less by their relative fertility, than it is by their power of resisting the agencies constantly at work for their destruction. Those, therefore, which possess the organization that confers the largest amount of such resisting power, will be those that will be likely to survive the longest and to propagate their kind. The more weakly individuals, or those which have some peculiarity of organization which (under the circumstances) places them at a disadvantage, will be earliest removed, their places being filled up by the offspring of the more vigorous.

5. Thus so long as the external conditions which affect the existence of any race remain unaltered, the characters of that race will not tend to change, from the time when they have once been brought by this process of *natural selection* into the fullest harmony with those conditions; and thus a species may remain permanent for any number of years or ages, simply because no new form has arisen that could surpass the old in the perfection of its adaptation to surrounding conditions.

6. But if those conditions should undergo a change, the harmony previously existing between the constitution of the race and the circumstances under which it exists is disturbed; and that disturbance may be such as to occasion the extinction of the race, unless it shall possess within itself some power of accommodating itself to the change. That accommodation may take place in two ways; either by the direct influence of external conditions in modifying the constitution of the race (as where it is subjected to a change of temperature or of atmospheric pressure); or by the process of *natural selection*, which will no longer tend to maintain the original type, but on the contrary to bring about and establish a modification of it. For if among the aberrant forms that present themselves from time to time, any should occur which are more in harmony with the new conditions of the species than is that of the original type, then the individuals possessing that conformation will have the advantage in the struggle for existence, and will consequently maintain their ground, whilst their less pliable relatives are (as brother Jonathan would phrase it) "improved off the face of the earth." Thus a new race will come to take the place of the old, just as a new breed of domestic animals having superior qualities super-

sedes that from which it was derived; the only important difference between the two cases lying in this, that the *artificial selection* practised by Man has for its purpose to perpetuate only those qualities which he regards as likely to be useful to himself, and which are for the most part such as would be rather disadvantageous than otherwise, if the race had to maintain its existence by its own unaided resources; whilst the process of *natural selection* operates for the good of the race *per se*, and tends to bring it into its highest state of perfection as a self-sustaining and independent aggregate.

7. Thus, then, a species which has presented the aspect of permanence for any length of time, may be caused to undergo a change at any period, and may continue to present that varied form for a long succession of ages, undergoing a further departure from the original type whenever a fresh change in the conditions of its existence shall occur.

8. The question of degree of modification thus comes, in Mr. Darwin's view, to be only one of *time;* and he holds that any amount of change of type is conceivable on the principle of natural selection, if an unlimited lapse of ages be allowed for its operation. On this principle he would trace back all the species of one genus as derived by direct descent with modification from a single prototype; all the genera of one family, in like manner, he regards as having had a common ancestor still more remote; and by parity of reasoning he would derive all the orders of one class, and even all the classes of one sub-kingdom, from the same stock; thus reducing the probable number of primordial forms of animals to some four or five, or even, carrying the same analogy still further, to a single one.

The facts and arguments by which these positions are supported, are set forth in the first five chapters of Mr. Darwin's treatise; wherein are considered (1) the Variation which occurs under Domestication, (2) the Variation which occurs under Natural Conditions, (3) the Struggle for Existence which all living beings, Plants as well as Animals, have to maintain, (4) the operation of Natural Selection, and (5) the Laws of Variation. We shall not offer our readers a detailed analysis of these admirable chapters, because we think it much more fitting that such as desire to make themselves thoroughly acquainted with Mr. Darwin's views should have recourse to his own very lucid and readily accessible exposition of them. And we shall only say that we should strongly suspect either the intellectual capacity or the candour of any man, who should attentively peruse them without being strongly impressed by the cogency of the considerations adduced by Mr. Darwin in support of his fundamental principle of Natural Selection. For ourselves we do not hesitate to say that they appear to us of irresistible force; but the acceptance of the principle by no means involves the acceptance of the conclusions which Mr. Darwin deduces from it; and, as we shall hereafter endeavour to show, the question of the community of origin of the *higher groups*, such as orders, classes, and sub-kingdoms, is one of a very different nature from that of species, genera, and even families.

To those who have been accustomed to look upon *species* as natural types of form definitely marked out by fixed characters which are transmitted without modification or variation from parent to offspring, it is necessary first to prove that a tendency to variation exists, as well in a state of nature, as under the artificial circumstances of cultivation and domestication. Having ourselves made this inquiry a special object of pursuit from the time that our attention was directed to it by Dr. Prichard five-and-twenty years ago, we feel ourselves in a position to affirm without hesitation, that those Naturalists who possess the most comprehensive acquaintance with any department of Zoology or Botany are those who are most disposed to admit the existence of wide variation; the multiplication of species distinguished by minute shades of difference having been the work of that class of men, who think that Natural History essentially consists in collecting and cataloguing.

It is very fortunate for our present purpose that the publication of Dr. Joseph D. Hooker's Introductory Essay on the 'Flora of Australia,' which has been prepared subsequently to the first enunciation of Mr. Darwin's views, enables us to place before our readers the testimony of the man who has probably the highest claim of any living botanist to speak with authority upon this question, so far as the Vegetable Kingdom is concerned. Having received a thorough scientific training from his distinguished father, the former Professor of Botany at Glasgow and present Director of Kew Gardens, Dr. J. D. Hooker has largely profited by the opportunities he has enjoyed of visiting many different countries and of studying the vegetation of various regions; and having been led by his own taste to make the geographical range of species an object of special study, he has had the advantage not only of being able to collate his results with those furnished by the largest and best-named botanical collections in the world, but also of receiving a larger amount of assistance from his fellow-naturalists than falls to the share of most.

The total number of species of Flowering Plants known to exist upon the surface of the globe has been estimated by different botanists, upon data pretty much the same, at 150,000, at 100,000, and at 80,000. In Dr. Hooker's opinion, 50,000 would be much nearer the mark. Among the examples which he has given (in his Introduction to the 'New Zealand Flora') of the fallacious methods on which the higher estimates have been constructed, we find that no fewer than nineteen species have been made of the common *Potato*, and many more of *Solanum nigrum* alone; that the *Pteris aquilina* (the common brake fern) has a different botanical name in almost every country in the world; that the *Vernonia cinerea* of India has given rise to at least fifteen book-species; and that many of the commonest European plants have several names in Europe, others in India, and still others in America,—to say nothing of the host of garden names for themselves, their hybrids and varieties, all of which are catalogued as species in the ordinary works of reference whence such estimates are compiled. The case of *Oxalis corniculata* is peculiarly instructive: of

this, which is one of the most widely diffused and variable plants in the world, no fewer than eight species are made by Cunningham and Richard in their 'New Zealand Flora,' whilst they actually exclude from it the plant whose varieties have given rise to all these.

It is obvious to the intelligent looker-on, that the multiplication of botanical species is the work chiefly of persons who have confined their attention to some local Flora, and who have very little acquaintance with anything beyond; whilst the reduction of the species thus created is subsequently accomplished by the careful comparison of similar plants brought from remote localities, and especially by the comparison of what are obviously varieties in one country with the reputed species of another. Thus in apologizing to the local botanists of New Zealand for the abolition of their eight species of Oxalis, which they affirm to be distinguished by the constancy with which they retain their states under varied conditions, Dr. Hooker says:

"I value such facts very highly, and attach great weight to them, and did these varieties occur only in New Zealand, I should perhaps have withheld so strong an opinion on the subject; but such is not the case, for *O. corniculata* varies as much in numerous other parts of the world; and admitting, as every one must, that varieties are known to retain their characters with more or less of constancy for certain periods, some other evidence is necessary to shake the opinion of the botanist who grounds his views on an examination of the plant from all quarters of the globe."

The following is another example of the same kind, the special value of which for our present purpose consists in the general remarks which are drawn from Dr. Hooker by his reference to it:

"I have been led to dwell upon this point, because I feel sure the New Zealand student will at first find it difficult to agree with me in many cases, as for instance in so protean a Fern as *Lomaria procera*, whose varieties (to an inexperienced eye) are more dissimilar than are other species of the same genus. In this (and in similar cases) he must bear in mind that I have examined many hundred specimens of the plant, gathered in all parts of the south temperate hemisphere; and have found, after a most laborious comparison, that I could not define its characters with sufficient comprehensiveness from a study of its New Zealand phases alone, nor understand the latter without examining those of Australia, South Africa, and South America. The resident may find two varieties of this and of many other plants, retaining their distinctive characters within his own range of observation (for that varieties often do so, and for a very uncertain period, both when wild, and also in gardens, is notorious), and he may perhaps have to travel far beyond his own island to find the link I have found, in the chain of forms that unites the most dissimilar states of *Lomaria procera*; but he can no more argue thence for the specific difference of these, than he can for a specific difference between the aboriginal of New Zealand and himself, because he may not find intermediate forms of his race on the spot. We do not know why varieties should in many cases thus retain their individuality over great areas, and lose them in others; but the fact that they do so proves that no deductions drawn from local observations on widely distributed plants can be considered conclusive. To the amateur these questions are perhaps of very trifling importance, but they are of great moment to the naturalist who regards accurately-defined floras as the means for investigating the great phenomena of vegetation; he has to seek truth amid errors of observation and judgment, and the resulting

chaos of synonymy which has been accumulated by thoughtless aspirants to the questionable honour of being the first to name a species."*

We would fain hope that Dr. Hooker is correct in saying that "the time is happily past when it was considered an honour to be the namer of a plant;" but we fear that there are still too many who are scarcely yet able to appreciate the truth of an excellent remark, which he has unfortunately consigned to a note instead of giving to it the prominence which it deserves, that

"The botanist who has the true interest of science at heart, not only feels that the thrusting of an uncalled-for synonym into the nomenclature of science is an exposure of his own ignorance and deserves censure, but that a wider range of knowledge and a greater depth of study are required, to prove those dissimilar forms to be identical, which any superficial observer can separate by words and a name."

The error of the ordinary species-maker consists in basing his idea of a plant upon the form and aspect which it presents in a small number of specimens collected within a limited area; he makes no allowance for the effects of local peculiarities in temperature, humidity, soil, or exposure, unless he can absolutely trace the cause to the effect; and hence he attaches great importance to habit, stature, colour, hairiness, outline of leaves, period of flowering, &c., all of which characters are recognised by the more experienced botanist as preeminently liable to be affected by external conditions. A truly philosophical systematist like Dr. Hooker, on the other hand, bases his conclusions on the most extensive comparison he can make, not only of dried specimens in herbaria, but of living plants in all latitudes; and thus he comes to acquire a knowledge of the influence of external agencies, not only upon the general phenomena of vegetation, but also upon individual forms. It has been after this fashion that Mr. Bentham has studied the British Flora; with the result of annihilating about a fourth of its reputed species. And the more thoroughly and extensively this method is carried out, the more, it is now obvious, will it tend to simplify botanical science, by reducing the number of really distinct specific types, and clearing out from our systematic treatises the vast mass of rubbish with which they have been crowded by the unscrupulous creativeness of species-mongers.

Dr. J. D. Hooker's investigations early led him to oppose the common practice of fixing upon some one particular form, out of many varieties, as the original type of a species (according to the ordinary hypothesis of "distinct creations"), and considering the rest as derivations from it. Too frequently the term is used merely to characterize that individual of a species which was first cultivated, described, figured, or collected, or that form which happens to be most abundant in the neighbourhood of the writer; and it may of course happen that all the individuals thus referred to may present anomalous or exceptional states of the true type. The only clue we possess to the detection of this, is that which we can derive, in the case of any species of world-wide or "mundane" distribution, from a careful comparison of all its

* New Zealand Flora, pp. xiii. xiv.

variations and from a contrast between these and those of its allies; a sort of medium may thus be eliminated, which may be assumed as an approximation to the original type; but with how little certainty this can be inferred any one may judge for himself, if he endeavours on the same principle to determine what was the original type of the Human species,—whether Adam and Eve were Arabs or Hindoos, Negroes or Mongols, Caucasians or Hottentots, Red Indians or Malays. In regard to plants of limited distribution, especially those restricted to particular islands, the case is different. These are regarded by Dr. Hooker as the remnants of a much larger and more widely diffused Flora, of which a considerable part has been extinguished by subsequent geological changes; and it is of course not only possible but probable that those changes so far modified the condition of the survivors, that their present forms differ considerably from those under which they originally existed. "Practically, then," he says, "the type is a phantom;" that is, in assuming any existing form of a species as the representative of the original, we go far beyond what the facts of the case justify; and our "idea" of a species must necessarily be incomplete, until we have before us *all* the varieties it has presented not only in space but in time.

The still more extended experience acquired by Dr. Hooker since the publication of his 'New Zealand Flora' has only served to confirm and extend the views which he there announced in regard to the variability of the forms assumed by Botanists as specifically distinct.

"The limits of the majority of species," he says ('Flora of Australia,' p. iii.), "are so undefinable that few naturalists are agreed upon them; to a great extent they are matters of opinion, even amongst those persons who believe that species are original and immutable creations; and as our knowledge of the forms and allies of each increases, so do these differences of opinion; the progress of systematic science being, in short, obviously unfavourable to the view that most species are limitable by descriptions or characters, unless large allowances are made for variation."

In the course of a twenty years' study of plants, Dr. Hooker has been engaged in classifying many Floras,—large and small,—insular and continental,—arctic, temperate, and tropical; embracing areas so extensive and varied as fully to justify the assumption that the results derived from these are applicable to the whole vegetable kingdom. In every Flora he finds that whilst some species appear so distinct from one another that most botanists agree as to their limits, their peculiarities being transmitted with little or no change from parent to offspring, and no graduated transition being effected by intermediate forms, there are others which so run into one another as to leave the most practised botanist in a state of perplexity as to their limits, and to prevent him from referring the offspring with any certainty to their parents; so that the entire of such a group has to be regarded as a continuous series of varieties, between the terms of which no hiatus exists suggesting the intercalation of any intermediate variety. The genera *Rubus*, *Rosa*, *Salix*, and *Saxifraga* afford conspicuous examples of these unstable species; whilst *Veronica*, *Campanula*, and *Lobelia* are chiefly composed of comparatively stable forms.

Of these natural groups of varying and unvarying species, some are large and some small; they are also very variously distributed through the classes, orders, and genera of the Vegetable Kingdom; but, as a general rule, the varying species are relatively most numerous in those classes, orders, and genera which are the simplest in structure; increase in complexity of structure being, in Dr. Hooker's opinion, generally accompanied with an increased tendency to permanence in form. Of this principle we could draw some remarkable illustrations from the class of Fungi. Dr. Hooker now agrees with Mr. Darwin (though long disposed to doubt his statement) that the species of large genera are relatively more variable than those of small ones; as if the multiplication of species in the former had been the result of progressive divergence from one primitive type, which possessed an inherent capacity for variation not shared by the latter. And, on the whole, he thinks that herbs are more variable than shrubby plants, and annuals than perennials. The prominent fact, however, is "that this element of mutability pervades the whole Vegetable Kingdom; no class, nor order, nor genus of more than a few species claims absolute exemption; whilst the grand total of unstable forms generally assumed to be species probably exceeds that of the stable.'

The same general doctrines are found by Dr. Hooker to be applicable to all the higher divisions of plants. Some genera and orders are as natural and as limitable by characters, as are some species; others, again, although they contain many very well marked subordinate plans of construction, yet are so connected by intermediate forms with other genera or orders, that it is impossible to limit them naturally. Of the former set, *Orchideæ* and *Gramineæ* are characteristic examples; all the plants belonging to either of these orders being readily referred to each respectively by any competent botanist, notwithstanding that they differ greatly among themselves, not only in habit but in organs of vegetation and reproduction. Of the latter we have examples in *Melanthaceæ* and *Scrophularineæ;* for, although their genera and to a great extent their species also are well-marked and limitable, yet they both contain many groups which are constructed upon very different plans, and are connected with other orders by links of affinity so gradational as to prevent their distinct limitation.

The very important proposition was enunciated by Dr. Hooker in his Introduction to the 'Flora of New Zealand,' that *we are indebted for our means of resolving plants into limitable genera and orders, to the extinction of the forms by which they were originally connected.* This view, which he believes to be now generally accepted even by those who still regard species as the immutable units of the Vegetable Creation, appears to have been suggested to him by the consideration of the effects that must have been produced upon the Flora of the Southern Hemisphere by the submergence of a large proportion of that great Antarctic Continent which seems to have formerly connected New Zealand, South America, Australia, and Van Diemen's Land, as evidenced by a comparison of their respective Floras, and (as Mr.

Wallace has lately shown) by the study of the distribution of their animal inhabitants also.

"No botanist," remarks Dr. Hooker, "can reflect upon the destruction of peculiar species on small islands (such is now going on in St. Helena amongst others), without feeling that, as each disappears, a gap remains which may never be botanically refilled; that not only are those links breaking by which he connects the present flora with the past, but also those by which he binds the different members of the vegetable kingdom one to another."

The zoologist who thoughtfully compares the existing with the extinct forms, in any division of the animal kingdom in which the fossil remains of the latter can be considered as pretty fairly representing the principal types of those which have successively existed in geological time, must see many striking illustrations of this doctrine. Thus if we turn to the existing herbivorous quadrupeds, we find one large series constituting the very natural group of Ruminantia, which seems not only to be complete in itself, but to be very distinctly circumscribed, showing little disposition to pass by gradational links into other groups; whilst in another series the non-ruminant quadrupeds are brought together under the designation of Pachydermata, not so much on account of their agreement in any common characters, as because there seems no other way of disposing of them. The families of which the Elephant, the Tapir, the Rhinoceros, the Hippopotamus, the Pig, the Horse, and the Dugong are respectively the types, differ almost as widely from each other, as regards both their dentition and their extremities, as they do from Ruminants. And yet, when the extinct forms are properly intercalated, these are found to constitute gradational links of affinity of the most remarkable kind, not only among the several families of Pachyderms, but between them and the Ruminants; so that, as Prof. Owen has shown, the whole constitute a series as natural and continuous as that of the Ruminants seem to be now. It is obvious, therefore, that the limitation of the existing Orders of Pachyderms and Ruminants, and the mutual isolation of the families of the former, is due to the extinction of a large proportion of the members of which the entire group was originally constituted.

The inquiry naturally arises, then, whether the *limitation of species*, where it really exists, is not due to a process of a like kind,—namely, *the extinction of intermediate varieties;* and having shown that such a view is sanctioned by a philosophical study of the facts presented to the view of the systematist, Dr. Hooker proceeds to inquire how far it is consistent with the results of physiological research. Our information on this subject is chiefly derived from the behaviour of plants under cultivation; which process affects them, either by suddenly subjecting them to changes in their external conditions which might otherwise have occurred naturally, or by placing them in conditions to which they would never have been exposed in the ordinary course of nature. In the former case, such variations of habit and of conformation are likely to be rapidly induced, as seem to have more gradually developed themselves among wild species growing under varied circumstances; but in the latter the results are widely dif-

ferent, for the plant is eventually either killed, or it undergoes changes in its character that might otherwise have never taken place,—those strongly-marked varieties, without intervening gradations, being produced, which are known to the gardener as "sports." Now the prominent phenomena presented by species under cultivation are analogous in kind and extent to those which have been observed in a state of nature;—a large number apparently remaining permanent and unalterable, while a large number vary indefinitely. With regard to those which are apparently permanent, it is curious to observe that they are not always those which are permanent in a state of nature; and further, that we have no right to conclude that, because they preserve their characters unchanged for a lengthened period, they are necessarily immutable. For it is a remarkable fact that species which have remained apparently unaffected by cultivation for many generations, frequently at length begin to vary; and that, when they have once begun, they are peculiarly prone to vary further.

Now this fact appears to us to have a very marked significance. It can scarcely be questioned that the agencies which at last produce the change in these cases, have really been operative through the whole antecedent period, although their influence has not been ostensibly manifested. And it would seem as if there were some power inherent in the "constitution" of such plants, which causes them to resist such modifying influences and to continue to repeat their ordinary type, until it is borne down by their cumulative action, after which it succumbs altogether. Every medical practitioner must have noticed analogous differences among the subjects of his daily observation. There are some individuals who are unpleasantly affected by every change of temperature or of humidity, by the least indiscretion in diet, by the slightest over-fatigue of body or mind, and yet who are seldom the victims of any serious malady: whilst there are others who seem callous to the most bitter north-easters, and rather enjoy the hot close atmosphere of our most oppressive summer-days, who appear to eat and drink with impunity what would derange any ordinary stomach for a month, and who scarcely know what fatigue means, and who yet, after the resistance of half a life to morbific agencies which seem to roll off them like shot from a bomb-proof, suddenly succumb at last to some fearful attack of disease, from which they never entirely rally, their "constitution" being said to be "broken." This hidden accumulation of the agencies continually operating on organized beings, for a long succession of years in any one individual, or for a long succession of generations in any one species, and their then sudden manifestation in some very marked alteration in the condition of the organism, is a fact about which we think there can be no reasonable doubt. Further, all physiological probability tends to indicate, that the more prolonged the influence, the more decided will be the change in the constitution of the race; just as we see in pathology that any chronic disease is more obstinate and difficult to deal with, in proportion to the length of time during which the individual has been subjected to the causes which have induced it; the most

obstinate of all cases being those in which the tendency to the disease is hereditary, that is, in which the causes have had a persistent operation in a previous generation or succession of generations.

The tendency of all variation of plants under cultivation, as well as of those growing in their natural habitats, is to produce progressively-increasing departures from the original type; "the best marked varieties of a wild species," Dr. Hooker remarks, "occurring on the confines of the area the species inhabits, and the best marked varieties of the cultivated species being those last produced by the gardener." He is not disposed to concur in the common statement "that there is a strong tendency in cultivated, and indeed in all varieties, to revert to the type from which they departed." On the contrary, he says—

"The majority of cultivated vegetables and cereals, such as the cabbage and its numerous progeny, and the varieties of wall-fruit, show when neglected no disposition to assume the characters of the wild states of these plants; they certainly degenerate, and even die if Nature does not supply the conditions which man (by anticipation of her operations, or otherwise) has provided; they become stunted, hard, and woody, and resemble their wild progenitors in so far as all stunted plants resemble wild plants of similar habit; but this is not a reversion to the original type, for most of these cultivated races are not *merely* luxuriant forms of the wild parent. In neglected fields and gardens we see plants of Scotch Kale, Brussels Sprouts, or Kohl-rabë, to be all as unlike their common parent the wild *Brassica oleracea*, as they are unlike one another; so, too, most of our finer kinds of apples, if grown from seed, degenerate and become Crabs, but in so doing they become Crab states of the varieties to which they belong, and do not revert to the original wild Crab-apple. And the same is true to a great extent of cultivated Roses, of many varieties of trees, of the Raspberry, Strawberry, and indeed of most garden plants. It has been held, that by imitating the conditions under which the wild state of a cultivated variety grows, we may induce that variety to revert to its original state; but, except in the false sense of reversion above explained, I doubt if this is supported by evidence. Cabbages grown by the sea-side are not more like wild Cabbages than those grown elsewhere, and if cultivated states disseminate themselves along the coast, they there retain their cultivated form."*

The general fact, then, with regard to the varieties induced by cultivation, is that although, when allowed to run wild, they may so far revert to the type of the original that peculiarities of the latter which had been seemingly obliterated make their appearance again, yet that in doing so they so far retain the special characters they had acquired, as not to lose their claim to be considered as varieties. Hence it is that Botanists are still in doubt as to the parent species of many of our cultivated fruits and cereals; which would not be so if they showed a continued disposition to revert to the wild form. Thus the argument for the permanence of species, that is based upon the asserted tendency of cultivated forms to revert when neglected to the wild type, falls to the ground. As Dr. Hooker justly remarks, what the cultivator does is not necessarily (as some have maintained) to induce a diseased or unnatural condition of constitution, but simply to place the plant under conditions which Nature does not provide *at the same particular place and time.*

* Australian Flora, p. ix.

"That Nature might supply the conditions at other places and times, may be inferred from the fact that the plant is found to be provided with the means of availing itself of them when provided, while at the same time it retains all its functions, not only unimpaired, but in many cases in a more highly developed state."

It is no answer to such a statement to point to the admitted fact that our cultivated plants are, for the most part, incapable of self-perpetuation; for the tendency of cultivation, in many instances, is to promote the nutrition of the individual at the expense of its reproduction by seed, a state which is so far from being abnormal, that it is the ordinary condition of many wild plants, which scarcely ever multiply in any other way; whilst in other instances in which there is no deficiency in the production of seeds, the obstruction to their fertility results, not from anything unsuitable in the condition of the plant, but from the interference of agencies external to it, such as climate, the voracity of birds, &c., against which the plant cannot maintain itself unless it ripens more seeds than those agencies destroy. Cultivated wheat, for example, will grow and ripen its seed in almost all soils and climates; and as its seeds are produced in great abundance, and can be preserved alive in any quantity, in the same climate, and for many years, it follows that the extinction it would speedily undergo, if not protected by human agency, is due not to the artificial or peculiar condition of the plant itself, which is as sound and unbroken in health and vigour during its life as any wild variety is, but to the number of the enemies that attack its offspring.

For reasons which are fully stated in Mr. Darwin's chapter on Hybridism, there is ground to believe that the mixture of varieties by cross-impregnation tends rather to maintain the continuity of the specific type, than to induce departure from it; its tendency being to contract rather than to enlarge the limits of variation. That some supposed species may have had their origin in hybridization cannot be denied; but the number of these is probably small, the general fact being, that parents differing from each other in characters of sufficient importance to be accounted truly distinct species, do not produce a progeny that is capable *per se* of continuing its race.

Dr. Hooker having thus been led by his extended study of the Vegetable Kingdom to the independent conviction that every individual possesses within itself a greater or less capacity for variation, its power to change ceasing only with its life, is brought face to face with the grand difficulty of this doctrine; which lies in the admitted fact that there are limits to these mutations both as to degree and kind, species being neither visionary nor even arbitrary creations of the naturalist, but being realities, whether only temporarily so or not. Of this difficulty he unreservedly accepts the doctrine of the delimitation of species by *natural selection* (advanced by Mr. Darwin and Mr. Wallace) as a probable solution; and he does not refuse to go along with it even to the full extent of Mr. Darwin's ultimate speculation. After considering the general phenomena of the geographical and palæontological distribution of Plants, he considers himself justified by ascertained facts in the following assumptions:—

"That the principal recognised families of plants which inhabited the globe at and since the Palæozoic period still exist, and therefore have, as families, survived all intervening geological changes. That of these types some have been transferred, or have migrated, from one hemisphere to another. That it is not unreasonable to suppose that further evidence may be forthcoming, which will show that all existing species may have descended genealogically from fewer pre-existing ones; that we owe their different forms to the variation of individuals, and the power of limiting them into genera and species to the destruction of some of these varieties, &c., and the increase of individuals in others. Lastly, that the fact of species being with so much uniformity the ultimate and most definable group (the leaves as it were of the family tree), may possibly be owing to the tendency to vary being checked, partly by the ample opportunities each brood of a variety possess of being fertilized by the pollen of its nearest counterpart, partly by the temporary stability of its surrounding physical conditions, and partly by the superabundance of seeds shed by each individual, those only vegetating which are well suited to existing conditions; an appearance of stability is also, in the case of many perennials, due to the fact that the individuals normally attain a great age, and thus survive many generations of other species, of which generations some present characters foreign to their parents."*

In estimating the relative amount and rate at which different plants vary, Dr. Hooker points out that much error is often committed. Thus it is assumed that annuals are more variable than perennials; but this is chiefly because a brief personal experience enables several generations of annuals to be studied under many varied combinations of physical conditions, whereas the same experience embraces for the most part but a fractional period of the existence of a perennial. It has also been well shown by Mr. Bentham that an appearance of stability is given to many varieties of perennials, through their habitual increase by buds, offsets, &c., which propagate the individual, not the race; of this we have examples in the case of *Rubi* (brambles), which comparatively seldom propagate by seed. A large tract of ground may be peopled by parts of a single individual, whose original peculiarities may thus be widely diffused and firmly established in a particular locality; just as the vast masses of the now-well-known *Anacharis alsinastrum*, which inconveniently obstruct our rivers, canals, and water-courses, and have all been produced by the extension of a single plant imported some years since from Canada, bid fair to perpetuate in this country any departure from the ordinary specific type which the imported plant might chance to present. In like manner it is obvious that among the almost infinitely varied forms of *Diatomaceæ*, which are now such favourite objects of study among British microscopists, and of which some are disposed to make almost as great a multitude of distinct species, many of the differences that present themselves among the individuals collected from separate lakes, pools, or ditches, are due to the fact that any variety which may happen to arise among the offspring of a particular parentage, will tend to be multiplied and perpetuated indefinitely by that duplicative subdivision which among these single-celled organisms represents the budding of higher plants; instead of merging again into the average type, which

* Australian Flora, pp. xxii. xxiii.

it would probably soon do if it were to unite itself with other individuals in generation, instead of keeping up its isolation by self-division. Hence it has come to be admitted among those who have most carefully and extensively studied this group, that neither size, outline, nor distance of striæ affords a sufficient basis for the distinction of species, until it has been ascertained by an extensive comparison of forms brought from different localities in the widest area over which the species can be traced, what are the average characters of the type, and what is their range of variation.

The most important fact in favour of the permanence of species—the only one, in fact, which affords a tangible argument—is that of genetic resemblance. To the tyro in Natural History all similar plants or animals may have had one parent; but all dissimilar plants or animals must have had dissimilar parents. Daily experience demonstrates the first assumption, and seems to justify the second; but it requires years of careful observation to prove that the second is not always true. And, as Dr. Hooker has well pointed out, it is only by such Naturalists as specially devote themselves to this inquiry, that the truth is likely to be eliminated: for the chief aim of the ordinary systematist is either to arrive at an accurate knowledge of the relations of genera and orders, in which case he takes the species (as it were) for granted; or to acquire an intimate knowledge of the particular groups of species presented by local Floras, in studying which he is liable to be misled by the hereditary transmission of the minutest differences within limited areas, and by his want of acquaintance with the intermediate varieties that may present themselves elsewhere. It is because Dr. Hooker has so carefully kept in view the fundamental question of species, whilst engaged in his higher studies, and has shown such a comprehensive mastery of the general phenomena of vegetation when dealing with local Floras, that we attach so high an importance to his *pronunciamento* on this question. His concluding remarks are so excellent that we cannot forbear quoting them in full:

"It has been urged against the theory that existing species have arisen through the variation of pre-existing ones and the destruction of intermediate varieties, that it is a hasty inference from a few facts in the life of a few variable plants, and is therefore unworthy of confidence, if not consideration; but it appears to me that the opposite theory, which demands an independent creative act for each species, is an equally hasty inference from a few negative facts in the life of certain species, of which some generations have proved invariable within our extremely limited experience. These theories, however, must not be judged of solely by the force of the very few absolute facts on which they are based; there are other considerations to be taken into account, and especially the conclusions to which they lead, and their bearing upon collateral biological phenomena, under which points of view the theory of independent creations appears to me to be greatly at a disadvantage; for according to it every fact and every phenomenon regarding the origin and continuance of species, but that of their occasional variation and their extinction by natural causes, and regarding the *rationale* of classification, is swallowed up in the gigantic conceptions of a power intermittently exercised in the development, out of inorganic elements, of organisms the most bulky and complex as well as the most minute and simple; and the consanguinity of each new being

to its pre-existent nearest ally, is a barren fact, of no scientific significance or further importance to the naturalist than that it enables him to classify. The realization of this conception is of course impossible; the boldest speculator cannot realize the idea of a highly organised plant or animal starting into life within an area that has been the field of his own exact observation and research; whilst the more cautious advocate hesitates about admitting the origin of the simplest organism under such circumstances, because it compels his subscribing to the doctrine of the 'spontaneous generation' of living beings of every degree of complexity in structure and refinement of organisation.

"On the other hand, the advocate of creation by variation may have to stretch his imagination to account for such gaps in a homogeneous system as will resolve its members into genera, classes, and orders; but in doing so he is only expounding the principle which both theorists allow to have operated in the resolution of some groups of individuals into varieties: and if, as I have endeavoured to show, all those attributes of organic life which are involved in the study of classification, representation, and distribution, and which are barren facts under the theory of special creations, may receive a rational explanation under another theory, it is to this latter that the naturalist should look for the means of penetrating the mystery which envelopes the history of species, holding himself ready to lay it down when it shall prove as useless for the further advance of science as the long serviceable theory of special creations, founded on genetic resemblance now appears to be."*

Such testimony as this to the value of Mr. Darwin's theory ought to procure for it a fair hearing from the zoologist; and there are not wanting indications that the several divisions of the Animal Kingdom, when studied with the like care, will furnish results of the like character. The question of *what constitutes a species* becomes in fact more and more difficult, in proportion as it is seen that no general rule can be laid down, which shall be alike applicable in all groups. If we adopt the principle formularized by Decandolle,—probably as philosophical a statement of the case as has yet been put forward,—that "we unite under the designation of a species all those individuals which mutually bear to one another so close a resemblance as to allow of our supposing that they *may have* proceeded originally from a single being or a single pair," it is obvious that it opens a further question which nothing but experience can decide, what is the degree of difference which may present itself among the descendants of a common ancestry, or, in other words, what is the *range of variation* of which each specific type is susceptible. It would appear in some cases as if the resemblance of the offspring to each other and to their common parent is so close and uniform, that a difference of a most trivial kind, such as a stripe of colour on the hide of a mammal, or the shape or hue of a tuft of feathers in a bird, or a certain spot on the wing of a butterfly, or a peculiar sculpture of the surface of a shell, is held to justify the assumption of a distinct original parentage for the group of individuals which presents it; notwithstanding that, in many other cases, such slight and superficial differences are justly regarded as of no account whatever, seeing that they occur among individuals which are known to have had a common descent. It is not unfrequently found, indeed, that differences of sufficient constancy and importance to serve

* Australian Flora, pp. xxv. xxvi.

for the separation not merely of species, but even of genera, in one group, are so inconstant and gradational in another, that they cannot be admitted to any other rank than that of individual diversities. Thus the systematist is constantly finding himself at a loss to determine what are the characters of sufficient constancy to serve for the definition of his species, and what extent of range he must assign in each case to individual variation; and this difficulty is generally found to increase, rather than to diminish, with the extent of his researches. For it is generally true that in Zoology, as in Botany, the multiplication of reputed species has been the work of men of very imperfect information, who (to use the appropriate expression of the late Prince of Canino) have described *specimens* rather than *species;* and that a large number of these can be reduced to the rank of varieties merely by a sufficiently extended comparison with each other. And this holds good no less in regard to fossil types than with respect to recent; the unequivocal tendency of recent Palæontological inquiry having been to show that there is a much greater identity in type among the species which occur through a succession of stratified deposits, than those have been willing to admit who have based their conclusions on the trivial differences presented by isolated specimens; and also to indicate that a general modification of any specific type has been consequent on a modification of the physical conditions under which it has existed.

To the Physiologist and Pathologist, the question of hereditary transmission is one of which it is almost impossible to exaggerate the importance; for, as Sir H. Holland has well remarked in one of his suggestive essays, "in considering the hereditary tendency to disease, whether arising from structural or less obvious causes, it is needful to regard it in connexion with, or even as part and effect of, that great general principle, through which varieties of species have been spread over the globe, with obvious marks of wise and beneficent design." The inquiry is one far too wide to be even entered upon in the limited space that now remains to us; but we shall take an early opportunity of bringing it under the attention of our readers, confining ourselves for the present to a notice of some of the chief points of interest discussed by Mr. Darwin in the chapters of his book to which we have not yet alluded.

In his Sixth Chapter he grapples with some of the difficulties which will naturally present themselves to the minds of those who shall attempt to follow him in the application of the doctrine of Natural Selection to anything like the extent to which he is himself disposed to carry it. Thus it may reasonably be inquired why, if species have descended from other species by insensibly fine gradations, do we not see everywhere innumerable transitional forms? Why is not all nature in confusion, instead of presenting, as we see, an assemblage of species for the most part well defined? Of his answers to these questions, marked by his usual penetration and logical consistency, we are enabled to present a summary in his own words:

"I believe that species come to be tolerably well-defined objects, and do not at any one period present an inextricable chaos of varying and intermediate links:—

"Firstly, because new varieties are very slowly formed, for variation is a very slow process, and natural selection can do nothing until favourable variations chance to occur, and until a place in the natural polity of the country can be better filled by some modification of some one or more of its inhabitants; and such new places will depend on slow changes of climate, or on the occasional immigration of new inhabitants, and probably in a still more important degree on some of the old inhabitants becoming slowly modified with the new forms thus produced, and the old ones acting and reacting on each other. So that, in any one region and at any one time, we ought only to see a few species presenting slight modifications of structure in some degree permanent; and this assuredly we do see.

"Secondly, areas now continuous must often have existed within the recent period in isolated portions, in which many forms, more especially amongst the classes which unite for each birth and wander much, may have separately been rendered sufficiently distinct to rank as representative species. In this case, intermediate varieties between the several representative species and their common parent, must formerly have existed in each broken portion of the land; but these links will have been supplanted and exterminated during the process of natural selection, so that they will no longer exist in a living state.

"Thirdly, when two or more varieties have been formed in different portions of a strictly continuous area, intermediate varieties will, it is probable, at first have been formed in the intermediate zones, but they will generally have had a short duration. For these intermediate varieties will, from reasons already assigned (namely, from what we know of the actual distribution of closely allied or representative species, and likewise of acknowledged varieties), exist in the intermediate zones in lesser numbers than the varieties which they tend to connect. From this cause alone the intermediate varieties will be liable to accidental extermination; and during the process of further modification through natural selection, they will almost certainly be beaten and supplanted by the forms which they connect; for these, from existing in greater numbers, will, in the aggregate, present more variation, and thus be further improved through natural selection, and gain further advantages.

"Lastly, looking not to one time, but to all time, if my theory be true, numberless intermediate varieties, linking most closely all the species of the same group together, must assuredly have existed; but the very process of natural selection constantly tends, as has been so often remarked, to exterminate the parent forms and the intermediate links. Consequently, evidence of their former existence could be found only amongst fossil remains, which are preserved, as we shall in a future chapter attempt to show, in an extremely imperfect and intermittent record." (pp. 177-179.)

We are somewhat surprised that Mr. Darwin has not had recourse for illustration to the phenomena presented by the Races of Man, which, until fused by intermixture, present such remarkable constancy and speciality of characters over their respective areas, as to have led many Ethnologists to regard them as distinct species, yet which those who have studied them most candidly, intelligently, and profoundly, are now disposed to refer to a common origin. The doctrine of Natural Selection could scarcely, we think, be more satisfactorily tested, than by applying it to the process by which the Earth has become peopled with races so diversified, yet each presenting features of such marked adaptation to the peculiar conditions of its existence, as to necessitate, in the opinion of some, the idea of its special creation with reference to these.

Another objection which naturally occurs to the inquirer is that

arising out of the peculiar modifications of structure presented by some animals (as, for example, the bat or the whale), and the wonderful elaboration of particular organs, such as the eye, of which we hardly as yet fully understand the inimitable perfection. Mr. Darwin freely admits the cogency of this objection; but he urges not unfairly that if large bodies of facts, otherwise inexplicable, can be explained on the theory of descent, we ought not to hesitate in going further, and to admit—what cannot be affirmed to be logically impossible—that any conceivable degree of perfection may be acquired through Natural Selection. "Reason," he says, "ought to conquer Imagination;" and the Astronomer who affirms that his reason can gauge such profundity of space, and the Geologist who trusts in its ability to penetrate such unfathomable depths of time, as defy the power of the Imagination, ought to be the last to say that because they cannot conceive how an eye could thus be brought into existence, therefore Mr. Darwin's theory is false.

Some, again, will find in the special Instincts of animals,—such as those which lead the bee to construct cells which have practically anticipated the discoveries of profound mathematicians,—a grave objection to the doctrine of Natural Selection. This subject is specially discussed by Mr. Darwin in his Seventh Chapter, which contains some very curious and novel information on the subject of the architecture of bees, together with an examination of the difficulties presented on his theory by the existence of neuter insects, the parasitic instincts of birds and insects, the slave-making instincts of ants, &c. His mode of disposing of these difficulties is very ingenious; but to us it appears unnecessarily elaborate. For what are called instincts are simply in our apprehension the expressions of the habitual modes of operation of the particular organization; and any modification or further development of the organization will necessarily involve a corresponding modification of its habitual mode of action. We see this unmistakeably in Plants, the direction of whose tendrils or rootlets is often so marvellously adapted to the purposes which these have respectively to serve, as to have forced on the minds of many observers the notion that it must be guided by some degree of consciousness. We may see it clearly, too, in watching the development of Animals and even of Man; the special instincts of whose different periods of life are obviously related to the functional activity of particular organs, the alimentative dominating for the most part in the earlier stages, and the sexual when the generative system has come with adult age into energetic operation. Hence, if the principle of Natural Selection can account for the production of the organized structure, we see no difficulty in extending it to any of the actions to which it is subservient; all these being but the expressions (so to speak) of the capacity of that structure.

It will be more pertinently asked, however, in what manner, on Mr. Darwin's theory of the Origin of Species, we can account for *species*, when crossed, being sterile and producing sterile offspring, whereas, when *varieties* are crossed, their fertility is unimpaired? This

subject is elaborately treated in Mr. Darwin's Eighth Chapter, which is obviously the result of a very careful and conscientious examination of the facts in regard to hybridism, that have been accumulated by the labours of those two admirable observers Kolreuter and Gärtner, as well as of many others collected by himself; his general conclusion being that no such precise law can be admitted, as that according to which it has been proposed to distinguish species from varieties on the ground of their sterility or fertility when brought into sexual connexion. For although it is doubtless a general rule that the hybrid offspring of parents whose specific diversity (according to the received views) is unquestionable, are *per se* incapable of engendering a mixed race, yet their sterility is by no means uniformly absolute, being in fact a question of degree. So, again, although it is doubtless a general fact that the crossing of forms only slightly different tends to increase the vigour and fertility of the offspring, yet it also appears that wider differences, though still within the admitted range of variation, tend to diminish and even to check fertility. It is well known that in grafting trees, the capacity of one species or variety to unite with another bears a general relation to their mutual conformity in constitution, though sometimes modified by unknown differences in their vegetative systems; and looking to the fact that the reproductive system is more readily affected than any other by causes which influence the vital conditions of any race, it does not seem strange that the greater or less readiness of one species to breed with another should be rather dependent on unknown differences in their reproductive systems, than on any special limitation of their capacity with a view to prevent their crossing and blending in nature. The sterility of hybrids, which have their reproductive systems imperfect, and which have had this system and their whole organization disturbed by being compounded of two distinct species, seems closely allied to that sterility which so frequently affects pure species when their natural conditions of life have been disturbed. There has doubtless been much vicious reasoning in regard to this matter; plants and animals which breed freely together having been assumed on that account to be identical in species; and those which are not thus fertile being assumed to be diverse. And it is a somewhat significant fact, that the two very careful experimentalists already alluded to have arrived in several instances at diametrically opposite conclusions, when they have come to apply this test in practice. We are strongly disposed, therefore, to agree with Mr. Darwin, that the phenomena of Hybridism, when carefully examined, *do not* support the received view that there is a fundamental distinction between species and varieties; but are at least as well explained upon the idea that sterility is a question of degree,—that degree depending upon the amount of divergence which separates the constitutional conditions of the two forms brought together.

It may be urged, however, as an objection to Mr. Darwin's views, that varieties produced by artificial selection, though differing so strongly from each other that they would be unhesitatingly ranked by any one ignorant of their origin as belonging to dissimilar species

or even (as in the case of Pigeons) to dissimilar genera, are capable of breeding freely with each other; no instance being known of varieties thus engendered ever coming to be kept from mixture by reason of their sterility when crossed. The explanation of this fact, however, does not appear to us at all difficult on Mr. Darwin's view. Artificial selection is generally practised rather with a view of intensifying and perpetuating some particular feature, which may have but very little relation to the general constitutional character. Moreover, in no instance in which any great diversity of conformation has been engendered, has this been maintained (so far as we know) for any long succession of generations; consequently it has not yet been fixed (so to speak) as the attribute of the race, which commonly shows a continual tendency to reversion to the ordinary type, and is only kept up in its speciality by elimination of all the individuals that do not fully come up to the desired type. The ordinary influence of external causes, on the other hand, tends sooner (as we see in medical practice) to modify the constitutional state, than to affect the bodily conformation; and it is easy to understand from the evidence we daily have of their operation, that their prolonged agency might very considerably affect the capacity of one race to propagate with another, even though that action had not manifested itself in any very striking diversity of external characters.

In his Ninth and Tenth Chapters, Mr. Darwin applies himself to the consideration of the objections which may be raised against his views, on the ground of the entire absence of Palæontological evidence as to the past existence of any such finely graduated organic chain,—consisting of a series of intermediate links between the first progenitors of each principal type, and the diversified forms under which it has subsequently presented itself,—as his theory requires. The force of this, which he admits to be the most obvious and the gravest objection which can be urged against it, he thinks is entirely removed by a fair appreciation of *the extreme imperfection of the geological record.* To all such as have been prepared for the reception of his arguments by the mastery of Sir Charles Lyell's admirable reasonings upon the subject, in that grand work on the 'Principles of Geology,' which we fully agree with Mr. Darwin in believing that "the future historian will recognise as having produced a revolution in natural science," we feel sure that Mr. Darwin's deductions must approve themselves as necessary corollaries from Sir C. Lyell's demonstrations. To those, on the other hand, who have been accustomed to look at the revelations of Geology as giving us a succession of tolerably complete pictures of the former condition of the Earth, instead of an assemblage of isolated sketches or studies which show us rather how little than how much we know, or are ever likely to know, about its past history and its living inhabitants, Mr. Darwin's protest against the drawing of any inferences unfavourable to his theory from the paucity of the remains of the almost infinite number of generations which must have succeeded one another in the long roll of years, will seem more like the special pleading of an advocate than (what we are firmly convinced

that it is) the candid representation of profound scientific truth. We are fully satisfied that he does not in the least exaggerate the imperfection of the geological record; and think it the less necessary to add anything in confirmation of his views, because we have already attempted to show, in our introductory remarks, how many indications are afforded by modern geological inquiry that the production of new species has been effected by constant and progressive rather than by intermittent action. Many of the great leading facts in Palæontology certainly harmonize most remarkably with the doctrine of continuous descent with modification through natural selection.

"We can thus understand how it is that new species come in slowly and successively; how species of different classes do not necessarily change together, or at the same rate, or in the same degree; yet in the long run that all undergo modification to some extent. The extinction of old forms is the almost invariable consequence of the production of new forms. We can understand why when a species has once disappeared it never reappears. Groups of species increase in numbers slowly, and endure for unequal periods of time; for the process of modification is necessarily slow, and depends upon many complex contingencies. The dominant species of the larger dominant groups tend to leave many modified descendants, and thus new sub-groups and groups are formed. As these are formed, the species of the less vigorous groups, from their inferiority inherited from a common progenitor, tend to become extinct together, and to leave no modified offspring on the face of the Earth. But the utter extinction of the whole group of species may often be a very slow process, from the survival of a few descendants, lingering in protected and isolated situations. When a group has once wholly disappeared, it does *not reappear; for the link of generations has been broken.*

"We can understand how the spreading of the dominant forms of life, which are those that oftenest vary, will in the long run tend to people the world with allied, but modified, descendants; and these will generally succeed in taking the places of those groups of species which are their inferiors in the struggle for existence. Hence, after long intervals of time, the productions of the world will appear to have changed simultaneously.

"We can understand how it is that all the forms of life, ancient and recent, make together one grand system; for all are connected by generation. We can understand from the continued tendency to divergence of character, why the more ancient a form is, the more it generally differs from those now living. Why ancient and extinct forms often tend to fill up gaps between existing forms, sometimes blending two groups previously classed as distinct into one; but more commonly only bringing them a little closer together. The more ancient a form is, the more often, apparently, it displays characters in some degree intermediate between groups now distinct; for the more ancient a form is, *the more nearly it will be related to*, and consequently resemble, the common progenitor of groups since become widely divergent. Extinct forms are seldom directly intermediate between existing forms; but are intermediate only by a long and circuitous course through many extinct and very different forms. We can clearly see why the organic remains of closely consecutive formations are more closely allied to each other, than are those of remote formations; for the forms are more closely linked together by generation: we can clearly see why the remains of an intermediate formation are intermediate in character." (pp. 343-5.)

The Tenth and Eleventh Chapters are devoted to the subject of Geographical Distribution, which, as we have already endeavoured to

show, has lately come to present an aspect altogether new. These seem to us to be among the most interesting and satisfactory in the whole work; the clue which Mr. Darwin's theory affords, taken in connexion with the strong probability that considerable changes of level have occurred since the Earth has been peopled by its existing inhabitants, leading us towards a much better rationale of the remarkable phenomena presented by the present Geographical distribution of Plants and Animals, than any which has been previously offered. The great leading facts seem, as Mr. Darwin points out, to be explicable on the theory of migration (generally of the more dominant forms of life) together with subsequent modification and the multiplication of new forms. We can thus comprehend the high importance of barriers, whether of land or water, which separate our several zoological and botanical provinces. We can thus understand the localization of subgenera, genera, and families; and how it is that under different latitudes, for instance in South America, the inhabitants of the plains and mountains, of the forests, marshes, and deserts, are in so mysterious a manner linked together by affinity, and are likewise linked to the extinct beings which formerly inhabited the same continent. So again it can be explained why oceanic islands should have few inhabitants, but of these a large proportion should be endemic or peculiar; and why whole groups of organisms, as batrachians and terrestrial mammals, should be absent from such, whilst the most isolated islands possess their own peculiar species of aërial mammals or bats. As the late Professor Edward Forbes used to insist, there is a striking parallelism in the laws of life throughout time and over space; the laws governing the succession of forms in past times being nearly the same with those governing at the present time the differences in different areas. On Mr. Darwin's theory these relations are intelligible; for whether we look to the forms of life which have changed during successive ages within the same quarter of the world, or to those which have changed after having migrated into distant quarters, in both cases the forms within each class have been connected by the same bond of ordinary generation; and the more nearly any two forms are united in blood, the nearer will they generally stand to each other in time and in space; in both cases the laws of variation have been the same, and modifications have been accumulated by the same power of natural selection.

The argument is brought to a close in the Thirteenth Chapter, which treats of the evidence afforded in favour of the author's views by the Mutual Affinities admitted by all Naturalists to exist among Organized beings, by Morphology, Embryology, and the existence of Rudimentary organs. Mr. Darwin urges with great force that the nature of the relationship by which all living and extinct beings are united by complex, radiating, and circuitous lines of affinities into one grand system, —the rules followed and the difficulties encountered by naturalists in their classifications,—the value set on characters, if constant and prevalent, whether of high functional importance, or of the most trifling importance, or (as in the case of rudimentary organs) of no importance,

—the wide opposition in value between analogical or adaptive characters, and characters of true affinity,—and other such rules,—all naturally follow on the view of the common parentage of those forms which are considered by naturalists as allied, together with their modification through natural selection, with its contingencies of extinction and divergence of character. Thus, in his view, all true Classification is genealogical; and community of descent is the hidden bond which naturalists have been unconsciously seeking, rather than some unknown plan of creation, or the enunciation of general propositions, and the mere collocation and separation of objects more or less alike.

On this same view, it is urged by Mr. Darwin, all the great facts in Morphology become intelligible; that resemblance in general plan of organization which is expressed by the term "unity of type," acquiring a meaning and value of which it was before utterly destitute. Nothing, as Prof. Owen has fully admitted, can be more hopeless than to attempt to explain this fundamental similarity of pattern in members of the same class, by utility, or the doctrine of final causes. On the ordinary view of the independent creation of each animal and plant, we can only say that *so it is*, or that it has so pleased the Creator to construct it. A like explanation, he argues, may be given to the great facts of Embryology; such as the very general, but not universal, difference in structure between the embryo and the adult; the early likeness of parts in the same individual embryo, which afterwards become very unlike and serve for diverse purposes; the general, but not universal, resemblance of embryos of different species belonging to the same class; the absence of any close relation in the structure of the embryo to the conditions of its existence, except when it becomes active and has to provide for itself instead of being nurtured by its parent; and the apparent retrogradation which sometimes takes place from the embryonic type as the animal approaches maturity. It was long ago shown by Von Baer that all development proceeds from the general to the special; and, as we have already pointed out, this principle has come to exert a most important influence in classification, the real place of a group being often determined rather by reference to its embryonic than to its adult type. Now, the causes of modification which originate varieties of structure will for the most part act through the constitution of the parents; and the analogy of hereditary diseases appearing late in life would seem to indicate that such causes may not manifest their effect upon the offspring, until it has attained an age approaching that at which they acted on the parent. Hence it can be understood how it is that the differences in the races of domestic animals should be much less marked when they have just come into the world, than they subsequently become. Mr. Darwin found, for example, that the newly-hatched young of different breeds of pigeons, differing so much in their adult state that they would have been ranked among different genera had they been natural productions, so imperfectly presented their peculiar characters as not to be always distinguishable. Cases occur every now and then, however, in which the departure

from the ordinary type has shown itself at a much earlier period in the life of the offspring; and if this be perpetuated by breeding (as in the case of the "ancon" conformation of the legs, or the silky wool of the Mauchamp breed of sheep), it shows itself at a correspondingly early period in the subsequent descendants. If, then, the embryo be looked upon as a picture, more or less obscured, of the common parent form of each great class of animals, the successive modifications which it undergoes in its further development repeat in their essential features the modifications which the type has undergone in antecedent ages under the operation of natural selection.

On the same view the presence of Rudimentary Organs comes to acquire a meaning of which it is otherwise destitute; and this, to our minds, is one of the strongest arguments in favour of Mr. Darwin's theory. The same reasoning power (as he truly says) which tells us plainly that most parts and organs are exquisitely adapted for certain purposes, tells us with equal plainness that these rudimentary or atrophied organs are imperfect and useless. Such organs are generally said to have been created "for the sake of symmetry," or in order "to complete the scheme of nature;" and Mr. Paget has thrown out the idea that they serve to withdraw matter from the blood, which could not be retained in it without injury. Now, on the principle just now stated, at whatever period of life an organ thrown out of use by changed habits consequently becomes atrophied, and this condition is perpetuated by Natural Selection, the principle of inheritance at corresponding ages will tend to reduce the organ at the like period in the life of the offspring, and consequently will seldom affect or reduce it in the embryo; and hence will arise the greater relative size of rudimentary organs in the embryo, and the subsequent proportional reduction by arrest of their development. It is remarkable how much value systematists have been led to attach to the presence of rudimentary organs as indications of natural affinity; and this becomes readily intelligible on the genealogical view of classification, just as, to use Mr. Darwin's apposite simile, letters which are retained in the spelling of a word, though they have become useless in classification, often afford a clue in the search for its derivation. "Nature may be said to have taken pains to reveal, by rudimentary organs and by homologous structures, her scheme of modification, which it seems that we wilfully will not understand."

Of the Recapitulation and Conclusion contained in the closing Chapter, we need say no more than that it is a masterly summing-up of the author's argument, a worthy finale to what we do not hesitate to designate a wonderful book. From the remarks we have made as we have proceeded in our exposition of Mr. Darwin's views, our readers will perceive that we are strongly convinced of their fundamental truthfulness; and we cannot see any legitimate escape from the logical conclusion, that the process of Natural Selection has had a similar agency in multiplying the number of divergencies of type amongst the races of plants and animals living in a state of Nature, that the process of Artificial Selection has exerted on those subject to the

influence of Man. But when we once go beyond the limits of our actual experience, the question as to the extent of this change is one as to which we have no data whatever for any positive conclusion, and are left altogether to the guidance of probabilities. Supposing, for the sake of argument, that we concede to Mr. Darwin that all Birds have descended from one common stock,—and we cannot see that there is any essential improbability in such an idea, so small are the divergencies from a common type presented by any members of that group,—yet it by no means thence follows that Birds and Reptiles, or Birds and Mammals, should have had a common ancestry. The very imperfection of the Geological record, on which he so pointedly dwells, takes away all power of denial that Birds may have been placed on the Earth as early as any form of organic life whatever. And to us it seems far more likely that this has been the case with regard to each of the great types marked out by decided structural and physiological peculiarities, than that these have been derived from any still more remote ancestor by the process of Natural Selection. So, too, there seems to us so much in the psychical capacity of Man, however degraded, to separate him from the nearest of the Mammalian class, that we can far more easily believe him to have originated by a distinct creation, than suppose him to have had a common ancestry with the Chimpanzee, and to have been separated from it by a series of progressive modifications.

We think it very important to the fair reception of Mr. Darwin's primary views, that they should be considered quite apart from the ultimate conclusions to which they tend in his own mind, but which others may see adequate reason for doubting or rejecting. It is among their highest merits that the mere provisional reception of them, as a step (it may be) to something still better, will give a new interest to the philosophical pursuit of Natural History, and will bring into mutual reconcilement the two great ideas of opposing schools,—the morphological notion of Unity of Type,—and the teleological notion of Conditions of Existence.

Review VI.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XLII. *London*, 1859. 8vo. pp. 461.

The present volume of the 'Medico-Chirurgical Transactions' is inferior to none of its predecessors in interest or intrinsic value. It contains twenty-five original communications, about one-half of which are medical and the other half surgical. These communications are illustrated by nine lithographic plates, five of which are coloured, by fourteen diagrams, and three woodcuts. We proceed, as usual, to lay before our readers a brief abstract of the contents of the volume:

I. *A Case of Excision of the Head of the Humerus, with its Results.* By John Birkett, F.R.C.S.—This case is mainly recorded with the