ART. VIII.—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. 8vo. 1859.

2. *On the Tendency of Varieties to Depart Indefinitely from the Original Type.* By ALFRED RUSSEL WALLACE. (February, 1858.) Proceedings of the Linnæan Society, August, 1858.

3. BUFFON, *Histoire de ses Travaux et de ses Idées.* Par P. FLOURENS, Sec. Perp. de l'Académie des Sciences. 12mo. 1846.

4. *Contributions to the Natural History of the United States.* By M. AGASSIZ. 4to. Vol. I. (I. Essay on Classification.) 1857.

5. *On the Flora of Australia, &c.* By Dr. JOSEPH D. HOOKER, F.R.S. (Introductory Essay.) 4to. 1859.

6. *Essays on the Spirit of the Inductive Philosophy and the Philosophy of Creation.* By the Rev. BADEN POWELL. 12mo. 1855.

7. *Hétérogénie, ou Traité de la Génération Spontanée.* By Professor V. A. POUCHET. 8vo. Paris, 1859.

8. *Recherches sur l'Archetype et les Homologies du Squelette Vertebré.* Par Professor R. OWEN. 8vo. Paris: 1855.

9. *Address to the British Association, Leeds.* By Professor R. OWEN. 8vo. 1858.

10. *Palæontology; or a Systematic Summary of Extinct Animals, &c.* By Professor R. OWEN. 8vo. 1860.

IN the works above cited the question of the origin, succession, and extinction of species is more or less treated of, but most fully and systematically by the accomplished Naturalist who heads the list. Mr. Charles Darwin has long been favourably known, not merely to the Zoological but to the Literary World, by the charming style in which his original observations on a variety of natural phenomena are recorded in the volume assigned to him in the narrative of the circumnavigatory voyage of H.M.S. Beagle, by Capt. (now Admiral) Fitz Roy, F.R.S. Mr. Darwin earned the good opinion of geologists by the happy application of his observations on coral reefs*, made during that voyage, to the explanation of some of the phenomena of the changes of level

* On the Structure and Distribution of Coral Reefs, 8vo. 1842.

of the earth's crust. He took high rank amongst the original explorers of the minute organisation of the invertebrate animals, upon the appearance of his monographs, in the publications by the Ray Society, on the Cirripedia, Sub-classes Lepadidæ (1851), and Balanidæ (1854). Of independent means, he has full command of his time for the prosecution of original research: his tastes have led him to devote himself to Natural History; and those who enjoy his friendship and confidence are aware that the favourite subject of his observations and experiments for some years past has been the nature and origin of the so-called *species* of plants and animals. The octavo volume, of upwards of 500 pages, which made its appearance towards the end of last year, has been received and perused with avidity, not only by the professed naturalist, but by that far wider intellectual class which now takes interest in the higher generalisations of all the sciences. The same pleasing style which marked Mr. Darwin's earliest work, and a certain artistic disposition and sequence of his principal arguments, have more closely recalled the attention of thinking men to the hypothesis of the inconstancy and transmutation of species, than had been done by the writings of previous advocates of similar views. Thus several, and perhaps the majority, of our younger naturalists have been seduced into the acceptance of the homœopathic form of the transmutative hypothesis now presented to them by Mr. Darwin, under the phrase of 'natural selection.'

Dr. Joseph Hooker, in his latest work, above cited, writes:—

'In the Introductory Essay to the New Zealand Flora, I advanced certain general propositions as to the origin of species, which I refrained from endorsing as articles of my own creed; amongst others was the still prevalent doctrine that these are, in the ordinary acceptation of the term, created as such, and are immutable. In the present essay I shall advance the opposite hypothesis, that species are derivative and mutable, and this chiefly because, whatever opinions a naturalist may have adopted with regard to the origin and variation of species, every candid mind must admit that the facts and arguments upon which he has grounded his convictions require revision, since the recent publication by the Linnæan Society of the ingenious and original reasonings and theories of Mr. Darwin and Mr. Wallace.' (P. ii.)

Mr. Darwin claims another convert in an older name of scientific note: in reference to the immutability of species, he tells us, 'I have reason to believe that one great authority, Sir Charles 'Lyell, from further reflection, entertains grave doubts on this 'subject.' For our own part, governed by the motto of the parent society for the promotion of natural knowledge, 'nullius in verba,'

our attention was principally directed, in the first perusal of Mr. Darwin's work, to the direct observations of nature which seemed to be novel and original, and to the additional grounds, based on fact, on which a more lasting superstructure of the theory of the mutability of species might be raised. These observations, therefore, claim our notice before we proceed to discuss the general theory of the work.

No naturalist has devoted more painstaking attention to the structure of the barnacles than Mr. Darwin. In reference to the transitions of organs, and the probability of their conversion from one function to another, he states:—

'Pedunculated cirripedes have two minute folds of skin, called by me the ovigerous frena, which serve, through the means of a sticky secretion, to retain the eggs until they are hatched within the sack. These cirripedes have no branchiæ, the whole surface of the body and sack, including the small frena, serving for respiration. The Balanidæ or sessile cirripedes, on the other hand, have no ovigerous frena, the eggs lying loose at the bottom of the sack, in the well-enclosed shell; but they have large folded branchiæ. Now I think no one will dispute that the ovigerous frena in the one family are strictly homologous with the branchiæ of the other family; indeed, they graduate into each other.' (P. 191.)

That is, a series of modifications are affirmed to have been met with in different species, changing a respiratory into an ovigerous organ. Should this graduation of parts be confirmed, and the respiratory function of the folded membranes in Balanidæ be determined, Mr. Darwin will have contributed both an interesting observation, and a valuable discovery. But neither in the present work, nor in the two volumes published and illustrated at the cost of the Ray Society, are those relations of the folded membranes in the Balanidæ with the heart or vascular system demonstrated, which could alone prove the respiratory function of such membranes.

Mr. Darwin has by no means limited himself to dissections of dead animals, but has devoted much time to observation of the living. Desirous of testing the truth of the assertions respecting the slave-making ants (*Formica sanguinea*), he opened

fourteen nests of that species and found a few slaves in all. Males and fertile females of the slave species (*Formica fusca*) are found only in their proper communities, and have never been observed in the nests of *F. sanguinea*. The slaves are black, and not above half the size of their red masters, so that the contrast in their appearance is very great. When the nest is slightly disturbed, the slaves occasionally come out, and, like their masters, are much agitated and defend

the nest: when the nest is much disturbed and the larvæ and pupæ are exposed, the slaves work energetically with their masters in carrying them away to a place of safety. Hence, it is clear, that the slaves feel quite at home. During the months of June and July, on three successive years, I have watched for many hours several nests in Surrey and Sussex, and never saw a slave either leave or enter a nest. During the present year, however, in the month of July (1859), I came across a community with an unusually large stock of slaves, and I observed a few slaves mingled with their masters leaving the nest, and marching along the same road to a tall Scotch fir-tree, twenty-five yards distant, which they ascended together, probably in search of aphides or cocci. According to Huber, who had ample opportunities for observation, in Switzerland, the slaves habitually work with their masters in making the nest, and they alone open and close the doors in the morning and evening; and, as Huber expressly states, their principal office is to search for aphides. Another day my attention was struck by about a score of the slave-makers haunting the same spot, and evidently not in search of food; they approached and were vigorously repulsed by an independent community of the slave species (*F. fusca*); sometimes as many as three of these ants clinging to the legs of the slave-making *F. sanguinea*. The latter ruthlessly killed their small opponents, and carried their dead bodies as food to their nest, twenty-nine yards distant; but they were prevented from getting any pupæ to rear as slaves. I then dug up a small parcel of the pupæ of *F. fusca* from another nest, and put them down on a bare spot near the place of combat; they were eagerly seized, and carried off by the tyrants, who perhaps fancied that, after all, they had been victorious in their late combat.' (P. 221.)

Many other direct observations on the *F. sanguinea* of England are recounted, and are contrasted with those first recorded by Huber, relative to the slave-holding *F. rufescens* of Switzerland.

'Such are the facts, though they did not need confirmation by me, in regard to the wonderful instinct of making slaves. Let it be observed what a contrast the instinctive habits of *F. sanguinea* present with those of the *F. rufescens*. The latter does not build its own nest, does not determine its own migrations, does not collect food for itself or its young, and cannot even feed itself: it is absolutely dependent on its numerous slaves. *F. sanguinea*, on the other hand, possesses much fewer slaves, and in the early part of the summer extremely few. The masters determine when and where a new nest shall be formed, and when they migrate the masters carry the slaves. Both in Switzerland and England the slaves seem to have the exclusive care of the larvæ, and the masters alone go on slave-making expeditions. In Switzerland the slaves and masters work together, making and bringing materials for the nest: both, but chiefly the slaves, tend, and milk, as it may be called, their aphides; and thus both collect food for the community. In England the masters alone usually leave the nest to collect building materials and food for them-

selves, their slaves, and larvæ. So that the masters in this country receive much less service from their slaves than they do in Switzerland.' (P. 223.)

The instincts of the Bee have received not less attention from Mr. Darwin than those of the Ant; and he has also enriched this interesting part of Natural History by new and original remarks.* Desirous of testing the mechanical hypothesis of the formation of the hexagonal cell, out of an original cylindrical form, by pressure of surrounding cylinders, Mr. Darwin

'separated two combs, and put between them a long, thick, square, (rectangular?) 'strip of wax; the bees instantly began to excavate minute circular pits in it; and as they deepened these little pits they made them wider and wider until they were converted into shallow basins, appearing to the eye perfectly true or parts of a sphere, and of about the diameter of a cell. It was most interesting to me to observe that wherever several bees had begun to excavate these basins near together, they had begun their work at such a distance from each other, that by the time the basins had acquired the above stated width (*i. e.* about the width of an ordinary cell), and were in depth about one-sixth of the diameter of the sphere of which they formed a part, the rims of the basins intersected or broke into each other. As soon as this occurred, the bees ceased to excavate, and began to build up flat walls of wax on the lines of intersection between the basins, so that each hexagonal prism was built upon the festooned edge of a smooth basin, instead of on the straight edges of a three-sided pyramid as in the case of ordinary cells.' (P. 228.)

With regard to the mechanical origin of the bee's cell, Mr. Darwin proceeds to say:—

'In one well-marked instance I put the comb back into the hive, and allowed the bees to go on working for a short time, and again examined the cell, and I found that the rhombic plate had been completed, and had become *perfectly flat:* it was absolutely impossible, from the extreme thinness of the little rhombic plate, that they could have effected this by gnawing away the convex side; and I suspect that the bees in such cases stand in the opposed cells and push and bend the ductile and warm wax (which, as I have tried, is easily done) into its proper intermediate plane, and thus flatten it.

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* In the remarkable volume recently published by Lord Brougham, entitled 'Tracts, mathematical and physical,' which have been produced by his Lordship at various times from the year 1796 to the year 1858, will be found an excellent paper on the Mathematical Structure of Bees' Cells, read before the National Institute of France, by Lord Brougham, in the French language, in May 1858. It is a scientific and literary curiosity.

'From the experiment of the ridge of vermilion wax, we can clearly see that if the bees were to build for themselves a thin wall of wax, they could make their cells of the proper shape, by standing at the proper distance from each other, by excavating at the same rate, and by endeavouring to make equal spherical hollows, but never allowing the spheres to break into each other.' (P. 230.)

Mr. Darwin, while collecting objects of natural history in the rivers of Brazil, was surprised at the similarity of the fresh-water insects, shells, &c., and at the dissimilarity of the surrounding terrestrial beings, compared with the Fauna of Great Britain, and he was led to ponder on this power, as it seemed, in fresh-water productions of ranging widely. He offers many ingenious suggestions to account for the phenomena, and gives, what is of greater value, the following original observation and experiment:—

'Two facts which I have observed—and no doubt many others remain to be observed—throw some light on this subject. When a duck suddenly emerges from a pond covered with duckweed, I have twice seen these little plants adhering to its back; and it has happened to me, in removing a little duckweed from one aquarium to another, that I have quite unintentionally stocked the one with fresh-water shells from the other. But another agency is perhaps more effectual: I suspended a duck's feet, which might represent those of a bird sleeping in a natural pond, in an aquarium, where many ova of fresh-water shells were hatching; and I found that numbers of the extremely minute and just hatched shells crawled on the feet, and clung to them so firmly that when taken out of the water they could not be jarred off, though at a somewhat more advanced age they would voluntarily drop off. These just-hatched molluscs, though aquatic in their nature, survived on the duck's feet, in damp air, from twelve to twenty hours; and in this length of time a duck or heron might fly at least six or seven hundred miles, and would be sure to alight on a pool or rivulet, if blown across sea to an oceanic island or to any other distant point.' (P. 385.)

The mud adhering to the feet of wading birds may serve to transmit species of aquatic plants far away from their native streams.

'I do not believe (writes Mr. Darwin) that botanists are aware how charged the mud of ponds is with seeds. I have tried several little experiments, but will here give only the most striking case. I took, in February, three table-spoonfuls of mud from three different points, beneath water, on the edge of a little pond. This mud, when dry, weighed only 6¾ ounces. I kept it covered up in my study for six months, pulling up and counting each plant as it grew. The plants were of many kinds, and were altogether 537 in number; and yet the viscid mud was all contained in a breakfast cup! Considering these facts, I think it would be an inexplicable circumstance if

water-birds did not transport the seeds of fresh-water plants to vast distances, and if consequently the range of these plants was not very great. The same agency may have come into play with the eggs of some of the smaller fresh-water animals.' (P. 386.)

Facing the difficulty of the transport of fresh-water or land shell-fish across long tracts of ocean, on the supposition of a transporting bird occasionally resting on, or dipping in, the salt sea, or in the case of such shells adhering to drifted timber, Mr. Darwin made more experiments, and found —

'That several species did in this state withstand uninjured an immersion in sea-water during seven days: one of these shells was the *Helix pomatia*, and after it had again hybernated I put it in sea-water for twenty days, and it perfectly recovered. As this species has a thick calcareous operculum, I removed it, and when it had formed a new membranous one, I immersed it for fourteen days in sea-water, and it recovered and crawled away.' (P. 397.)

Pigeons being monogamous, and proverbial for their constancy, are peculiarly favourable for experiments and practices establishing and propagating varieties. Such varieties consequently have become, under the selective care of man, numerous and extreme. Believing it to be best, in reference to the question of the origin of varieties, to study some special group, Mr. Darwin took up domestic pigeons, associated himself with several eminent pigeon-fanciers, and joined two of the London Pigeon Clubs. He gives descriptions of the leading varieties: and amongst his own observations, the following, perhaps, conveys the newest matter:—

'As the evidence appears to me conclusive, that the several domestic breeds of Pigeon have descended from one wild species,—the Rock-pigeon (*Columba livia*),—I compared young pigeons of various breeds, within twelve hours after being hatched; I carefully measured the proportions (but will not here give details) of the beak, width of mouth, length of nostril and of eyelid, size of feet and length of leg, in the wild stock, in pouters, fantails, runts, barbs, dragons, carriers, and tumblers. Now some of these birds, when mature, differ so extraordinarily in length and form of beak, that they would, I cannot doubt, be ranked in distinct genera, had they been natural productions. But when the nestling birds of these several breeds were placed in a row, though most of them could be distinguished from each other, yet their proportional differences in the above specified several points were incomparably less than in the full-grown birds. Some characteristic points of difference — for instance, that of the width of mouth — could hardly be detected in the young. But there was one remarkable exception to this rule, for the young of the short-faced tumbler differed from the young of the wild rock-pigeon and of the other breeds, in all its proportions, almost exactly as much as in the adult state.' (P. 445.)

These are the most important original observations, recorded in the volume of 1859: they are, in our estimation, its real gems,—few indeed and far apart, and leaving the determination of the origin of species very nearly where the author found it; but a rich mine of such researches is alluded to and promised by Mr. Darwin, in a more voluminous collection of his researches, extending over a period of eighteen years; and to these every naturalist now looks forward with keen interest.

The interdependencies of living beings of different kinds and grades, and the injurious results of their interruption, have long attracted the attention of observant and philosophic naturalists. An undue importance indeed was at one time attached to this principle; it was deemed to be so absolute as that no one species could be permitted to perish without endangering the whole fabric of organisation. So Pope sang:—

> 'From Nature's chain, whatever link you strike,
> Tenth or ten thousandth, breaks the chain alike.'

Manifold subsequent experience has led to a truer appreciation and a more moderate estimate of the importance of the dependence of one living being upon another. Mr. Darwin contributes some striking and ingenious instances of the way in which the principle partially affects the chain, or rather network of life, even to the total obliteration of certain meshes. And truly extinction has made wide rents in the reticulation as now represented by the co-affinities of living species!

'From experiments which I have tried, I have found that the visits of bees, if not indispensable, are at least highly beneficial to the fertilisation of our clovers; but humble bees alone visit the common red clover (*Trifolium pratense*), as other bees cannot reach the nectar. Hence I have very little doubt, that if the whole genus of humble bees became extinct or very rare in England, the heartsease and red clover would become very rare, or wholly disappear. The number of humble-bees in any district depends in a great degree on the number of field-mice, which destroy their combs and nests; and Mr. H. Newman, who has long attended to the habits of humble-bees, believes 'that more than two-thirds of them are thus destroyed all over Eng-'land.' Now the number of mice is largely dependent, as every one knows, on the number of cats; and Mr. Newman says, 'Near villages 'and small towns I have found the nests of humble-bees more numerous 'than elsewhere, which I attribute to the number of cats that destroy 'the mice.' Hence it is quite credible that the presence of a feline animal in large numbers in a district might determine, through the intervention first of mice and then of bees, the frequency of certain flowers in that district!' (P. 73.)

This is very characteristic of the ingenious turn of thought of

our author; the more sober, or perhaps duller, naturalist would, no doubt, appreciate more highly a dry statement of investigations, suggested by the actual extinction of red clover, and tracing that extinction inductively, by the ascertained absence of humble-bees and mice, back to the want of cats in the neighbourhood. For the direct observation, however, (if it should be confirmed), of the exclusive relation of *Bombus terrestris*, as the mechanical fecundator of *Trifolium pratense*, natural history may be indebted to Mr. Darwin. We wish we could cite other instances augmenting this debt from the present work: its chief part, however, is devoted to speculations on the origin of species; and its main object is the advocacy of a view, which we find most clearly expressed in the following passage. Mr. Darwin refers to the multitude of the individuals of every species, which, from one cause or another, perish either before, or soon after attaining maturity.

'Owing to this struggle for life, any variation, however slight and from whatever cause proceeding, if it be in any degree profitable to an individual of any species, in its infinitely complex relations to other organic beings and to external nature, will tend to the preservation of that individual, and will generally be inherited by its offspring. The offspring, also, will thus have a better chance of surviving, for, of the many individuals of any species which are periodically born, but a small number can survive. I have called this principle, by which each slight variation, if useful, is preserved, by the term of Natural Selection, in order to mark its relation to man's power of selection. We have seen that man by selection can certainly produce great results, and can adapt organic beings to his own uses, through the accumulation of slight but useful variations, given to him by the hand of Nature. But Natural Selection, as we shall hereafter see, is a power incessantly ready for action, and is as immeasurably superior to man's feeble efforts, as the works of Nature are to those of Art.' (P. 61.)

The scientific world has looked forward with great interest to the facts which Mr. Darwin might finally deem adequate to the support of his theory on this supreme question in biology, and to the course of inductive original research which might issue in throwing light on 'that mystery of mysteries.' But having now cited the chief, if not the whole, of the original observations adduced by its author in the volume now before us, our disappointment may be conceived. Failing the adequacy of such observations, not merely to carry conviction, but to give a colour to the hypothesis, we were then left to confide in the superior grasp of mind, strength of intellect, clearness and precision of thought and expression, which might raise one man so far above his contemporaries, as to enable

him to discern in the common stock of facts, of coincidences, correlations and analogies in Natural History, deeper and truer conclusions than his fellow-labourers had been able to reach.

These expectations, we must confess, received a check on perusing the first sentence in the book.

'When on board H.M.S. "Beagle," as naturalist, I was much struck with certain facts in the distribution of the inhabitants of South America, and in the geological relations of the present to the past inhabitants of that continent. These facts seemed to me to throw some light on the origin of species — that mystery of mysteries, as it has been called by some of our greatest philosophers.' (P. 1.)

What is there, we asked ourselves, as we closed the volume to ponder on this paragraph,—what can there possibly be in the inhabitants, we suppose he means aboriginal inhabitants, of South America, or in their distribution on that continent, to have suggested to any mind that man might be a transmuted ape, or to throw any light on the origin of the human or other species? Mr. Darwin must be aware of what is commonly understood by an 'uninhabited island;' he may, however, mean by the inhabitants of South America, not the human kind only, whether aboriginal or otherwise, but all the lower animals. Yet again, why are the fresh-water polypes or sponges to be called 'inhabitants' more than the plants? Perhaps what was meant might be, that the distribution and geological relations of the organised beings generally in South America, had suggested transmutational views. They have commonly suggested ideas as to the independent origin of such localized kinds of plants and animals. But what the 'certain facts' were, and what may be the nature of the light which they threw upon the mysterious beginning of species, is not mentioned or further alluded to in the present work.

The origin of species is the question of questions in Zoology; the supreme problem which the most untiring of our original labourers, the clearest zoological thinkers, and the most successful generalisers, have never lost sight of, whilst they have approached it with due reverence. We have a right to expect that the mind proposing to treat of, and assuming to have solved, the problem, should show its equality to the task. The signs of such intellectual power we look for in clearness of expression, and in the absence of all ambiguous or unmeaning terms. Now, the present work is occupied by arguments, beliefs, and speculations on the origin of species, in which, as *it seems to us, the fundamental mistake is committed,* of confounding the questions, of species being the result of a secondary cause or law, and of the nature of that creative law.

Various have been the ideas promulgated respecting its mode of operation; such as the reciprocal action of an impulse from within, and an influence from without, upon the organisation (Demaillet, Lamarck); premature birth of an embryo at a phase of development, so distinct from that of the parent, as, with the power of life and growth, under that abortive phase, to manifest differences equivalent to specific (Vestiges of Creation); the hereditary transmission of what are called 'accidental monstrosities;' the principle of gradual transmutation by 'degeneration' (Buffon) as contrasted with the 'progressional' view.

In reference to the definition of species, Lamarck *, in 1809, cited, as the most exact, that of 'a collection of like (semblables) 'individuals produced by other individuals equally like them '(pareils à eux).' But the progress of discovery, especially, perhaps, in palæontology, led him to affirm that species were not as ancient as Nature herself, nor all of the same antiquity; that this alleged constancy was relative to the circumstances and influences to which every individual was subject, and that as certain individuals, subjected to certain influences, varied so as to constitute races, such variations might and do graduate (s'avancent) towards the assumption of characters which the naturalist would arbitrarily regard, some as varieties, others as species. He comments in almost the words of Mr. Darwin, on the embarrassment and confusion which the different interpretation of the nature and value of such observed differences, in the works of different naturalists, had occasioned.† The true method of surveying the diversities of organisation is from the simple to the compound forms, which course Lamarck affirms to be graduated and regularly progressive, save where local circumstances, and others influencing the mode of life, have occasioned anomalous diversities.

Cuvier had preceded Lamarck in specifying the kinds and degrees of variation, which his own observations and critical judgment of the reports of others led him to admit. 'Although 'organisms produce only bodies similar to themselves, there are 'circumstances which, in the succession of generations, alter to 'a certain point their primitive form.'‡ Here it may be remarked, that the whole question at issue hinges upon the proof of the determination of that limit of variety. Cuvier

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* Philosophie Zoologique, 8vo. 1809, vol. i. p. 54.

† Ib. p. 55.

‡ Cuvier, 'Tableau Élémentaire de l'Histoire Naturelle,' 8vo. 1798, p. 9.

gives no proof that the alteration stops 'at a certain 'point.' It merely appeafs from what follows, that his means of knowing by his own and others' observations had not carried him beyond the point in question, and he was not the man to draw conclusions beyond his premises.

'Less abundant food,' he goes on to say, 'makes the young acquire less size and force. Climate more or less cold, air more or less moist, exposure to light more or less continuous, produce analogous effects; but, above all, the pains bestowed by man on the animal and vegetable productions which he raises for his uses, the consecutive attention with which he restricts them in regard to exercise, or to certain kinds of food, or to influences other than those to which they would be subject in a state of nature, all tend to alter more quickly and sensibly their properties.'

Cuvier admits that the determination by experiment of these variable properties, of the precise causes to which they are due, of the degree of variability and of the powers of the modifying influences, is still very imperfect ('mais ce travail est encore 'très-imparfait.') The most variable properties in organisms are, according to Cuvier, *size* and *colour*.

'The first mainly depends on abundance of food; the second on light and many other causes so obscure that it seems to vary by chance. The length and strength of the hairs are very variable. A villous plant, for example, transported to a moist place, becomes smooth. Beasts lose hair in hot countries, but gain hair in cold. Certain external parts, such as stamens, thorns, digits, teeth, spines, are subject to variations of number both in the more and the less; parts of minor importance, such as barbs of wheat, &c., vary as to their proportions; homologous parts ('des parties de nature analogue') change one into another, *i.e.*, stamens into petals as in double flowers, wings into fins, feet into jaws, and we might add, adhesive into breathing organs [as in the case of the barnacles cited by Mr. Darwin].'

As to the alleged test of the difference between a species and a variety by the *infecundity* of the hybrid of two parents which may differ in a doubtful degree, Cuvier, in reference to this being the case when the parents are of distinct species, and not mere varieties, emphatically affirms, 'Cette assertion ne repose 'sur aucune preuve' (p. 11.); it is at least constant that individuals of the same species, however different, produce together; 'quelque différens qu'ils soient, peuvent toujours pro-'duire ensemble.' But Cuvier warns us not to conclude, when individuals of two different races produce an intermediate and fecund offspring, that they must be of the same species, and that they have not been originally distinct. (P. 13.)

'"The number of varieties, or amount of variation," says Cuvier, "relates to geographical circumstances." At the present day, many

such varieties appear to have been confined around their primitive centre, either by seas which they could neither traverse by swimming or by flight, or by temperatures which they were not able to support, or by mountains which they could not cross, &c.' *

Daily observation, comparison, and reflection, on recent and extinct organisms, pursued from the date of these remarks (1798) to the close of his career (1832) failed to bring the requisite proof, or to impress the mind of Cuvier with any amount of belief worth mentioning, as to the nature of the cause operative in the production of the species of which he was the first to demonstrate the succession.

Lamarck, without contributing additional results from observation and experience, affirms that the changes defined by Cuvier do not 'stop at a certain point,' but progress with the continued operation of the causes producing them. That, moreover, such changes of form and structure induce corresponding changes in actions, and that a change of actions, growing to a habit, becomes another cause of altered structure; that the more frequent employment of certain parts or organs leads to a proportional increase of development of such parts; and that, as the increased exercise of one part is usually accompanied by a corresponding disuse of another part, this very disuse, by inducing a proportional degree of atrophy, becomes another element in the progressive mutation of organic forms.†

These principles seem entitled to be regarded as of the nature of those called 'veræ causæ' by Bacon, and they are agreeable with known powers and properties of animated beings; only observation has not disclosed more than a very limited extent of their operation,—limited both as to the time in which that operation has been watched, and limited consequently as to the amount of the change produced.

When Cuvier affirms that such capacity to vary proceeds only to a certain point, he may mean that it has not been watched and traced beyond such point. Cuvier admits the tendency to hereditary transmission of characters of variation. Neither he nor any other physiologist has demonstrated the organic condition or principle that should operate so as absolutely to prevent the progress of modification of form and structure

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* 'Les variétés de chacune ont dû être d'autant plus fortes et plus nombreuses, que les circonstances des lieux ou de sa nature lui ont permis de s'étendre plus loin; c'est ce qui peut faire croire que les grandes différences que se trouvent parmi les hommes, les chiens, et les autres êtres répandues partout le monde, ne sont que des effets des causes accidentelles, en un mot, des *variétés*.' (P. 14.)

† Philosophie Zoologique, 8vo. 1809, tom. i. chaps. iii. vi. vii.

correlatively with the operation of modifying influences, in successive generations. But those who hastily or prematurely assume an indefinite capacity to deviate from a specific form are as likely to obstruct as to promote the solution of the question.

The principles, based on rigorous and extensive observation, which have been established since the time of Cuvier, and have tended to impress upon the minds of the most exact reasoners in biology the conviction of a constantly operating secondary creational law, are the following:—The law of irrelative or vegetative repetition, referred to at p. 437. of Mr. Darwin's work; the law of unity of plan or relations to an archetype; the analogies of transitory embryonal stages in a higher animal to the matured forms of lower animals; the phenomena of parthenogenesis; a certain parallelism in the laws governing the succession of forms throughout time and space; the progressive departure from type, or from the more generalised to more specialised structures, exemplified in the series of species from their first introduction to the existing forms.* In his last published work † Professor Owen does not hesitate to state 'that perhaps the most important and significant result 'of palæontological research has been the establishment of the 'axiom of *the continuous operation of the ordained becoming of 'living things.*' The italics are the author's. As to his own opinions regarding the nature or mode of that 'continuous 'creative operation,' the Professor is silent. He gives a brief summary of the hypotheses of others, and as briefly touches upon the defects in their inductive bases.‡ Elsewhere he has restricted himself to testing the idea of progressive transmutation by such subjects of Natural History as he might have specially in hand: as, e. g. the characters of the chimpanzee, gorilla, and some other animals.

All who have brought the transmutative speculations to the test of observed facts and ascertained powers in organic life, and have published the results, usually adverse to such speculations, are set down by Mr. Darwin as 'curiously illustrating the blind-'ness of preconceived opinion;' and whosoever may withhold assent to his own or other transmutationists' views, is described

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* The most numerous illustrations of this principle are to be found in Owen's palæontological works and memoirs; but he refrains from announcing it as a general law, probably regarding the induction as being yet incomplete.

† Palæontology, or a Systematic Summary of Extinct Animals, and their Geological Relations, 8vo., 1860, p. 3.; and President's Address to the British Association at Leeds, 1858, p. 3.

‡ Palæontology, p. 404.

as 'really believing that at innumerable periods of the earth's 'history certain elemental atoms suddenly flashed into living 'tissues.' (P. 483.) Which, by the way, is but another notion of the mode of becoming of a species as little in harmony with observation as the hypothesis of natural selection by external influence, or of exceptional birth or development. Nay, Mr. Darwin goes so far as to affirm —

'All the most eminent palæontologists, namely, Cuvier, Owen, Agassiz, Barrande, Falconer, E. Forbes, &c., and all our greatest geologists, as Lyell, Murchison, Sedgwick, &c., have unanimously, often vehemently, maintained the immutability of species.' (P. 310.)

But if by this is meant that they as unanimously reject the evidences of a constantly operative secondary cause or law in the production of the succession of specifically differing organisms, made known by Palæontology, it betrays not only the *confusion of ideas as to the fact and the nature of the law*, but an ignorance or indifference to the matured thoughts and expressions of some of those eminent authorities on this supreme question in Biology.

One of the disciples would seem to be as short-sighted as the master in regard to this distinction.

'It has been urged,' writes Dr. Hooker, '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; but it appears to me that the opposite theory, which demands an independent creative act for each species, is an equally hasty inference.' (*Hooker*, p. xxv.)

Here it is assumed, as by Mr. Darwin, that no other mode of operation of a secondary law in the foundation of a form with distinct specific characters, can have been adopted by the Author of all creative laws than the one which the transmutationists have imagined. Any physiologist who may find the Lamarckian, or *the more diffused and attenuated Darwinian, exposition of the* law inapplicable to a species, such as the gorilla, considered as a step in the transmutative production of man, is forthwith clamoured against as one who swallows up every fact and every phenomenon regarding the origin and continuance of species 'in 'the gigantic conception 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.' Significantly characteristic of the partial view of organic phenomena taken by the transmutationists, and of their

inadequacy to grapple with the working out and discovery of a great natural law, is their incompetency to discern the indications of any other origin of one specific form out of another preceding it, save by their way of gradual change through a series of varieties assumed to have become extinct.

But has the free-swimming medusa, which bursts its way out of the ovicapsule of a campanularia, been developed out of inorganic particles? Or have certain elemental atoms suddenly flashed up into acalephal form? Has the polype-parent of the acalephe necessarily become extinct by virtue of such anomalous birth? May it not, and does it not proceed to propagate its own lower species in regard to form and organisation, notwithstanding its occasional production of another very different and higher kind. Is the fact of one animal giving birth to another not merely specifically, but generically and ordinally, distinct, a solitary one? Has not Cuvier, in a score or more of instances, placed the parent in one class, and the fruitful offspring in another class, of animals? Are the entire series of parthenogenetic phenomena to be of no account in the consideration of the supreme problem of the introduction of fresh specific forms into this planet? Are the transmutationists to monopolise the privilege of conceiving the possibility of the occurrence of unknown phenomena, to be the exclusive propounders of beliefs and surmises, to cry down every kindred barren speculation, and to allow no indulgence in any mere hypothesis save their own? Is it to be endured that every observer who points out a case to which transmutation, under whatever term disguised, is inapplicable, is to be set down by the refuted theorist as a believer in a mode of manufacturing a species which he never did believe in, and which may be inconceivable?

We would ask Mr. Darwin and Dr. Hooker to give some thought to these queries, and if they should see the smallest meaning in them, to reconsider their future awards of the alternative which they may be pleased to grant to a fellow-labourer, hesitating to accept the proposition, either that life commenced under other than actually operating laws, or that 'all 'the beings that ever lived on this earth have descended,' by the way of 'natural selection,' from a hypothetical unique instance of a miraculously created primordial form.

We are aware that Professor Owen and others, who have more especially studied the recently discovered astounding phenomena of generation summed up under the terms Parthenogenesis and Alternation of Generations, have pronounced against those phenomena having, as yet, helped us 'to pene-

'trate the mystery of the origin of different species of animals,' and have affirmed, at least so far as observation has yet extended, that 'the cycle of changes is definitely closed;' that is, that when the ciliated 'monad' has given birth to the 'gregarina,' and this to the 'cercaria,' and the 'cercaria' to the 'distoma,'—that the fertilised egg of the fluke-worm again excludes the progeny under the infusorial or monadic form, and the cycle again recommences.* But circumstances are conceivable,—changes of surrounding influences, the operation of some intermittent law at long intervals, like that of the calculating-machine quoted by the author of 'Vestiges,'—under which the monad might go on splitting up into monads, the gregarina might go on breeding gregarinæ, the cercaria cercariæ, &c., and thus four or five not merely different specific, but different generic, and ordinal forms, zoologically viewed, might all diverge from an antecedent quite distinct form. For how many years, and by how many generations, did the captive polype-progeny of the *Medusa aurita* go on breeding polypes of their species (*Hydra tuba*), without resolving themselves into any higher form, in Sir John Dalyell's aquarium!† The natural phenomena already possessed by science are far from being exhausted on which hypotheses, other than transmutative, of the production of species by law might be based, and on a foundation at least as broad as that which Mr. Darwin has exposed in this Essay.

We do not advocate any of these hypotheses in preference to the one of 'natural selection,' we merely affirm that this at present rests on as purely a conjectural basis. The exceptions to that and earlier forms of transmutationism which rise up in the mind of the working naturalist and original observer, are so many and so strong, as to have left the promulgation and advocacy of the hypothesis, under any modification, at all times to individuals of more imaginative temperament; such as Demaillet in the last century, Lamarck in the first half of the present, Darwin in the second half. The great names to which the steady inductive advance of zoology has been due during those periods, have kept aloof from any hypothesis on the origin of species. One only, in connexion with his palæontological discoveries, with his development of the law of irrelative repetition and of homologies, including the relation of the latter to an archetype, has pronounced in favour of the view of the

* President's Address to the British Association at Leeds, p. 27.

† See the beautiful work entitled 'Rare and Remarkable Animals of Scotland,' 4to. vol. i. 1847, by Sir J. G. Dalyell.

origin of species by a continuously operative creational law; but he, at the same time, has set forth some of the strongest objections or exceptions to the hypothesis of the nature of that law as a progressively and gradually transmutational one.

Mr. Darwin rarely refers to the writings of his predecessors, from whom, rather than from the phenomena of the distribution of the inhabitants of South America, he might be supposed to have derived his ideas as to the origin of species. When he does allude to them, their expositions on the subject are inadequately represented. Every one studying the pages of Lamarck's original chapters (iii. vi. vii., vol. i., and the supplemental chapter of 'additions' to vol. ii. of the 'Philosophie Zoologique'), will see how much weight he gives to inherent constitutional adaptability, to hereditary influences, and to the operation of long lapses of time on successive generations, in the course of transmuting a species. The common notion of Lamarck's philosophy, drawn from the tirades which a too figurative style of illustrating the reciprocal influence of innate tendencies and outward influences have drawn upon the blind philosopher, is incorrect and unjust. Darwin writes:—

'Naturalists continually refer to external conditions, such as climate, food, &c., as the only possible cause of variation. In one very limited sense, as we shall hereafter see, this may be true; but it is preposterous to attribute to mere external conditions, the structure, for instance, of the woodpecker, with its feet, tail, beak, and tongue, so admirably adapted to catch insects under the bark of trees. In the case of the misseltoe, which draws its nourishment from certain trees, which has seeds that must be transported by certain birds, and which has flowers with separate sexes absolutely requiring the agency of certain insects to bring pollen from one flower to the other; it is equally preposterous to account for the structure of this parasite, with its relations to several distinct organic beings, by the effects of external conditions, or of habit, or of the volition of the plant itself.

'The author of the "Vestiges of Creation" would, I presume, say that, after a certain unknown number of generations, some bird had given birth to a woodpecker, and some plant to the misseltoe, and that these had been produced perfect as we now see them; but this assumption seems to me to be no explanation, for it leaves the case of the coadaptations of organic beings to each other and to their physical conditions of life untouched and unexplained.' (P. 3.)

The last cited ingenious writer came to the task of attempting to unravel the 'mystery of mysteries,' when a grand series of embryological researches had brought to light the extreme phases of form that the higher animals passed through in the course of fœtal development, and the striking analogies which transitory embryonal phases of a higher species presented to

series of lower species in their permanent or completely developed state. He also instances the abrupt departure from the specific type manifested by a malformed or monstrous offspring, and called to mind the cases in which such malformations had lived and propagated the deviating structure. The author of 'Vestiges,' therefore, speculates—and we think not more rashly or unlawfully than his critic has done—on other possibilities, other conditions of change, than the Lamarckian ones; as, for example, on the influence of premature birth and of prolonged fœtation in establishing the beginning of a specific form different from that of the parent. And does not the known history of certain varieties, such as that of M. Graux's cachemir-wooled sheep, which began suddenly by malformation, show the feasibility of this view?* 'The whole train of animated beings,' writes the author of 'Vestiges of Creation,' 'are the results *first*, of an 'inherent impulse in the forms of life to advance, in definite times, 'through grades of organisation terminating in the highest 'dicotyledons and mammals; *second*, of external physical circumstances, operating reactively upon the central impulse to 'produce the requisite peculiarities of exterior organisation,—'the adaptation of the natural theologian.' But he, likewise, requires the same additional element which Mr. Darwin so freely invokes. 'The gestation of a single organism is the 'work of but a few days, weeks, or months; but the gestation '(so to speak) of a whole creation is a matter involving enormous 'spaces of time.' . . . . 'Though distinctions admitted as specific are not now, to ordinary observation, superable, time may 'have a power over these.' . . . . 'Geology shows successions 'of forms, and grants enormous spaces of time within which we 'may believe them to have changed from each other by the means 'which we see producing varieties. Brief spaces of time 'admittedly sufficing to produce these so-called varieties, is 'it unreasonable to suppose that large spaces of time would 'effect mutations somewhat more decided, but of the same 'character?'†

Unquestionably not, replies Mr. Darwin:—

'To give an imaginary example from changes in progress on an island: let the organisation of a canine animal which preyed chiefly on rabbits, but sometimes on hares, become slightly plastic; let these same changes cause the number of rabbits very slowly to decrease, and the number of hares to increase; the effect of this would be that

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* Reports of the Juries Exhibition of the Works of All Nations, 8vo., 1852, p. 70.

† Vestiges of Creation, 8vo., 1846, p. 231.

the fox or dog would be driven to try to catch more hares; his organisation, however, being slightly plastic, those individuals with the lightest forms, longest limbs, and best eyesight, let the difference be ever so small, would be slightly favoured, and would tend to live longer, and to survive during that time of the year when food was scarcest; they would also rear more young, which would tend to inherit these slight peculiarities. The less fleet ones would be rigidly destroyed. I can see no more reason to doubt that these causes in a thousand generations would produce a marked effect, and adapt the form of the fox or dog to the catching of hares instead of rabbits, than that greyhounds can be improved by selection and careful breeding.'*

Of course, prosaic minds are apt to bore one by asking for our proofs, and one feels almost provoked, when seduced to the brink of such a draught of forbidden knowledge as the transmutationists offer, to have the Circean cup dashed away by the dry remark of a President of the British Association :—

'Observation of animals in a state of nature is required to show their degree of plasticity, or the extent to which varieties do arise: whereby grounds may be had for judging of the probability of the elastic ligaments and joint-structures of a feline foot, for example, being superinduced upon the more simple structure of the toe with the non-retractile claw, according to the principle of a succession of varieties in time.'†

This very writer has, however, himself suggested an operative cause in the development of organised beings of a different and opposite character to that conceived by 'Vestiges,' to produce the teleological adaptations. Professor Owen has pointed out the numerous instances in the animal kingdom of a principle of structure prevalent throughout the vegetable kingdom, exemplified by the multiplication of organs in one animal performing the same function, and not related to each other by combination of powers for the performance of a higher function. The Invertebrate animals, according to the Professor, afford the most numerous and striking illustrations of the principles which he has generalised as the 'Law of Irrelative Repetition.'

'We perceive,' says he, 'in the fact of the endoskeleton consisting of a succession of segments similarly composed—in the very power of enunciating special, general, and serial homologies—an illustration of that law of vegetative or irrelative

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* 'On the Tendency of Species to form Varieties,' &c., in 'Proceedings of the Linnæan Society,' 1858, p. 49.

† Address, p. 44.

repetition, which is so much more conspicuously manifested by the segments of the exoskeleton of the Invertebrata: as, for example, in the rings of the centipede and worm, and in the more multiplied parts of the skeleton of the Echinoderms. The repetition of similar segments in the spinal column, and of similar elements in a vertebral segment, is analogous to the repetition of similar crystals, as the result of the polarising force in the growth of an inorganic body. Not only does the principle of vegetative repetition prevail more and more as we descend in the scale of animal life, but the forms of the repeated parts of the skeleton approach more and more to geometrical figures; as we see, for example, in the external skeletons of the echini and star-fishes: nay, the calcifying salt assumes the same crystalline figures which characterise it, when deposited and subject to the general polarising force out of the organised body. Here, therefore, we have direct proof of the concurrence of such general all-pervading polarising force, with the adaptive or special organising force, in the development of an animal body.'

In addition, therefore, to the organising principle, however explained, producing the special 'adaptations,' and admitted as the 'second' power in the production of species by 'Vestiges,' Professor Owen states —

'There appears also to be in counter-operation during the building up of such bodies, a general polarising force, to the operation of which the similarity of forms, the repetition of parts, the signs of the unity of organisation may be mainly ascribed; the platonic ἰδέα or specific organising principle would seem,' he adds, 'to be in antagonism with the general polarising force, and to subdue and mould it in subserviency to the exigencies of the resulting specific form.'*

An index of the degree in which the polaric or irrelative repetitive force has operated is given by that character of the animal's organisation which is expressed by the term of 'a more 'generalised structure.' V. Baer pointed out that the structure was 'more generalised,' in the ratio of the proximity of the individual to the starting point of its existence. In proportion as the individual is subject to the action and reaction of surrounding influences, in other words, as it advances in life, does it acquire a more specialised structure — more decided specific and individual characters.† Owen has shown that the more generalised structure is, in a very significant degree, a characteristic of many extinct as compared with recent animals; and it may be readily conceived that specialisation of structure

* Archetype of the Vertebrate Skeleton, 8vo, 1840, p. 171.

† 'The extent to which the resemblance, expressed by the term, "Unity of Organisation," may be traced between the higher and lower organised animals, bears an inverse ratio to their approximation to maturity.' (*Owen, Lectures on Invertebrata,* p. 645.)

would be the result of the progressive modification of any organ applied to a special purpose in the animal economy.

We have cited these attempts to elucidate the nature of the organising forces, to show the prevalent condition of the most advanced physiological minds in regard to the cause of the successive introduction of distinct species of plants and animals. Demaillet invoked the operation of the external influences or conditions of life, with consentaneous volitional efforts, in order to raise species in the scale, as the fish, e. g., into the bird.* Buffon called in the same agency to lower the species, by way of degeneration, as the bear, e. g., into the seal, and this into the whale.† Lamarck added to these outward influences the effects of increased or decreased use or action of parts. The Author of 'Vestiges,' availing himself of the ingenious illustration of a pre-ordained exception, occurring at remote intervals, to the ordinary course, derived by Babbage from the working of his Calculating Engine, threw out the suggestion of a like rare exception in the character of the offspring of a known species, and he cites the results of embryological studies, to show how such 'monster,' either by excess or defect, by arrest or prolongation of development, might be no monster in fact, but one of the preordained exceptions in the long series of natural operations, giving rise to the introduction of a new species. Owen has not failed to apply the more recent discoveries of Parthenogenesis to the same mysterious problem. A polype, e. g., breaks up into a pile of medusæ; 'the indirect or direct 'action of the conditions of life' might tend to harden the integument and change the medusa into a star-fish. But he resists the seduction of possibilities, and governed by the extent of actual observation, says:—'The first acquaintance with these 'marvels excited the hope that we were about to penetrate the 'mystery of the origin of species; but, as far as observation has 'yet extended, the cycle of changes is definitely closed.'‡

Mr. Wallace calls attention to the 'tremendous rate of in-'crease in a few years from a single pair of birds producing two 'young ones each year, and this only four times in their life; 'in fifteen years such pair would have increased to nearly ten 'millions!'§ The passenger-pigeon of the United States exemplifies such rate of increase, where congenial food abounds.

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* Telliamed, ou Entretiens d'un Philosophe Indien avec un Missionaire François, Amsterdam, 8vo., 1748.

† Histoire Naturelle, &c., 4to., tom. xiv. 1766.

‡ Address to the British Association at Leeds, 1858, p. 27.

§ *Proceedings of the Linnæan Society*, 1858, p. 55.

But, as a general rule, the animal population of a country is stationary, being kept down by a periodical deficiency of food and other checks. Hence the struggle for existence; and the successful result of adapted organisation and powers in a well developed variety, which Mr. Darwin generalises as 'Natural Selection,' and which Mr. Wallace * illustrates as follows:—

'An antelope with shorter or weaker legs must necessarily suffer more from the attacks of the feline carnivora; the passenger-pigeon with less powerful wings, would sooner or later be affected in its powers of procuring a regular supply of food.'† If, on the other hand, 'any species should produce a variety having slightly increased powers of preserving existence, that variety must inevitably in time acquire a superiority in numbers.' 'During any change tending to render existence more difficult to a species, tasking its utmost powers to avoid complete extermination, those individuals forming the most feebly organised variety would suffer first; the same causes continuing, the parent species would next suffer, would gradually diminish in numbers, and with a recurrence of similar unfavourable conditions, must soon become extinct. The superior variety would then alone remain, and on a return to favourable circumstances would rapidly increase in numbers and occupy the place of the extinct species and variety. The *variety* would now have replaced the *species*, of which it would be a more perfectly developed and a more highly organised form.' ‡

Buffon regarded varieties as particular alterations of species, as supporting and illustrating a most important principle — the mutability of species themselves. The so-called varieties of a species, species of a genus, genera of a family, &c., were, with him, so many evidences of the progressive amount or degrees of change which had been superinduced by time and generations upon a primordial type of animal. Applying this principle to the two hundred mammalian species of which he had given a history in his great work, he believed himself able to reduce them to a very small number of primitive stocks or families. § Of these he enumerates fifteen: besides which, Buffon specifies certain isolated forms, which represent, as he forcibly and truly expresses it, both species and genus ‖: such are the elephant, rhinoceros, hippopotamus, giraffe, camel, lion, bear, and mole.¶

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* Proceedings of the Linnæan Society (dated from 'Ternate,' February 1858), vol. iii. p. 58.

† Wallace, loc. cit. p. 85. ‡ Ib., p. 58.

§ Histoire Naturelle tom. xiv. p. 338.

‖ 'Quelques espèces isolées, qui, comme celle de l'homme, fassent en même temps espèce et genre.' (Tom. cit., p. 335.)

¶ *Ib., p. 360.*

Palæontology has since revealed the evidences of the true nature and causes of the present seeming isolation of some of these forms.

Such evidences have been mainly operative with the later adopters and diffusers of Buffon's principle in the reduction of the number of primitive sources of existing species, and the contraction of the sphere of direct creative acts. Thus Lamarck* reduces the primordial forms or prototypes of animals to two, viz. the worm (*vers*), and the monad (*infusoires*); the principles which in the course of illimited time operated, on his hypothesis, to produce the present groups of animals led from the vibrio, through the annelids, cirripeds, and molluscs to fishes, and there met the other developmental route by way of rotifers, polypes, radiaries, insects, arachnides, and crustacea. The class of fishes, deriving its several forms from combinations of transmuted squids and crabs, then proceeded through the well-defined vertebrate pattern up to man. With a philosophic consistency, wanting in his latest follower, Lamarck sums up: 'Cette série 'd'animaux commençant par deux branches où se trouvent les 'plus imparfaits, les premiers de chacune de ces branches ne re-'çoivent l'existence que par génération directe ou spontanée.'†

Mr. Darwin, availing himself of the more exact ideas of the affinities and relationships of animal groups obtained by subsequent induction, says: 'I believe that animals have descended 'from at most only four or five progenitors,' [evidently meaning, or answering to, the type-forms of the four or five 'sub-kingdoms' in modern zoology], 'and plants from an equal or lesser 'number.'

But if the means which produce varieties have operated 'through the enormous species of time, within which species 'are changed,'‡ the minor modifications which produce, under our brief scope of observation, so-called varieties, might well amount to differences equivalent to those now separating sub-kingdoms; and, accordingly, 'analogy,' Mr. Darwin logically admits, 'would lead us one step further, namely, to the belief 'that all animals and plants have descended from some one 'prototype;'§ and, summing up the conditions which all living things have in common, this writer infers from that analogy, 'that probably all the organic beings which have ever lived on 'this earth, have descended from some one primordial form, into 'which life was first breathed.'‖

By the latter scriptural phrase, it may be inferred that Mr.

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* Philosophie Zoologique, vol. ii. p. 463. † Ib., p. 463.
‡ Vestiges of Creation, p. 231. § Op. cit., p. 484. ‖ Ib.

Darwin formally recognises, in the so-limited beginning, a direct creative act, something like that supernatural or miraculous one which, in the preceding page, he defines, as 'certain elemental 'atoms which have been commanded suddenly to flash into 'living tissues.' He has, doubtless, framed in his imagination some idea of the common organic prototype; but he refrains from submitting it to criticism. He leaves us to imagine our globe, void, but so advanced as to be under the conditions which render life possible; and he then restricts the Divine power of breathing life into organic form to its minimum of direct operation. All subsequent organisms henceforward result from properties imparted to the organic elements at the moment of their creation, pre-adapting them to the infinity of complications and their morphological results, which now try to the utmost the naturalist's faculties to comprehend and classify. And we admit, with Buckland, that such an aboriginal constitution, 'far from superseding an intelligent agent, would only 'exalt our conceptions of the consummate skill and power, 'that could comprehend such an infinity of future uses, under 'future systems, in the original groundwork of his creation.' We would accordingly assure Professor Owen that he 'may 'conceive the existence of such ministers, personified as Nature, 'without derogation of the Divine power;' and that he, with other inductive naturalists, may confidently advance in the investigation of those 'natural laws or secondary causes, to 'which the orderly succession and progression of organic phe-'nomena have been committed.'* We have no sympathy whatever with Biblical objectors to creation by law, or with the sacerdotal revilers of those who would explain such law. Literal scripturalism in the time of Lactantius, opposed and reviled the demonstrations of the shape of the earth; in the time of Galileo it reviled and persecuted the demonstrations of the movements of the earth; in the time of Dean Cockburn of York, it anathematised the demonstrations of the antiquity of the earth; and the eminent geologist who then personified the alleged anti-scriptural heresy, has been hardly less emphatic than his theological assailant, in his denunciations of some of the upholders of the 'becoming and succession of species by natural law,' or by 'a continuously operating creative force.' What we have here to do, is to express our views of the hypothesis as to the nature and mode of operation of the creative law, which has been promulgated by Messrs. Wallace and Darwin.

The author of the volume 'On the Origin of Species,' starts

* On the Nature of Limbs, p. 86.

from a single supernaturally created form. He does not define it; it may have been beyond his power of conception. It is, however, eminently plastic, is modified by the influence of external circumstances, and propagates such modifications by generation. Where such modified descendants find favourable conditions of existence, there they thrive; where otherwise they perish. In the first state of things, the result is so analogous to that which man brings about, in establishing a breed of domestic animals from a selected stock, that it suggested the phrase of 'Natural 'Selection;' and we are appealed to, or at least 'the young and 'rising naturalists with plastic minds*, are adjured, to believe that the reciprocal influences so defined have operated, through divergence of character and extinction, on the descendants of a common parent, so as to produce all the organic beings that live, or have ever lived, on our planet.

Now we may suppose that the primeval prototype began by producing, in the legal generative way, creatures like itself, or so slightly affected by external influences, as at first to be scarcely distinguishable from their parent. When, as the progeny multiplied and diverged, they came more and more under the influence of 'Natural Selection,' so, through countless ages of this law's operation, they finally rose to man. But, we may ask, could any of the prototype's descendants utterly escape the surrounding influences? To us such immunity, in the illimitable period during which the hypothesis of Natural Selection requires it to have operated, is inconceivable. No living being, therefore, can now manifest the mysterious primeval form to which Darwin restricts the direct creative act; and we may presume that this inevitable consequence of his hypothesis, became to him an insuperable bar to the definition of that form.

But do the facts of actual organic nature square with the Darwinian hypothesis? Are all the recognised organic forms of the present date, so differentiated, so complex, so superior to conceivable primordial simplicity of form and structure, as to testify to the effects of Natural Selection continuously operating through untold time? Unquestionably not. The most numerous living beings now on the globe are precisely those which offer such a simplicity of form and structure, as best agrees, and we take leave to affirm can only agree, with that ideal prototype from which, by any hypothesis of natural law, the series of vegetable and animal life might have diverged.

If by the patient and honest study and comparison of plants

* On the Nature of the Limbs, p. 482.

and animals, under their manifold diversities of matured form, and under every step of development by which such form is attained, any idea may be gained of a hypothetical primitive organism,—if its nature is not to be left wholly to the unregulated fancies of dreamy speculation—we should say that the form and condition of life which are common, at one period of existence, to every known kind and grade of organism, would be the only conceivable form and condition of the one primordial being from which 'Natural Selection' infers that all the organisms which have ever lived on this earth have descended.

Now the form in question is the nucleated cell, having the powers of receiving nutritive matter from without, of assimilating such nutriment, and of propagating its kind by spontaneous fission. These powers are called 'vital,' because as long as they are continued the organism is said to live. The most numerous and most widely diffused of living beings present this primitive grade of structure and vital force, which grade is inferior to that of the truly definable 'plant' or 'animal,' but is a grade represented and passed through by the germ of every, even the highest, class of animals, in the course of embryonic development. The next stages of differentiated or advanced organisation are defined as follows in Professor Owen's last publication:—

'When the organism is rooted, has neither mouth nor stomach, exhales oxygen, and has tissues composed of "cellulose" or of binary or ternary compounds, it is called a "plant." When the organism can move, when it receives the nutritive matter by a mouth, inhales oxygen, and exhales carbonic acid, and developes tissues, the proximate principles of which are quaternary compounds of carbon, hydrogen, oxygen, and nitrogen, it is called an "animal." But the two divisions of organisms called "plants" and "animals" are specialised members of the great natural groups of living things; and there are numerous organisms, mostly of minute size and retaining the form of nucleated cells, which manifest the common organic characters, but without the distinctive superadditions of true plants or animals. Such organisms are called "Protozoa," and include the sponges or *Amorphozoa*, the *Foraminifera* or Rhizopods, the *Polycystineæ*, the *Diatomaceæ*, *Desmidiæ*, *Gregarinæ*, and most of the so-called *Polygastria* of Ehrenberg, or infusorial animalcules of older authors.'*

All these would be interpreted as the earliest evidences of the modifying and species-changing influences, according to the hypothesis of Lamarck. They are the organisms respecting which the first living physiologists hesitate to apply the Harveian axiom *omne vivum ab ovo*, believing the possibility of

* Owen's Palæontology, p. 4.

their spontaneous origin to be by no means experimentally disproved. The prevalence of the essential first step in the production of all higher organisms, viz. through the combined matter of the 'germ-cell' and 'sperm-cell,' has no doubt strongly inclined physiologists to believe impregnation to be an absolute condition of the beginning of all existing organisms. But, as the President of the British Association stated, in his 'Address' at Leeds:—

'In regard to lower living things, analogy is but hazardous ground for conclusions. The single-celled organisms, such as many of the so-called animalcules of infusions, which are at a stage of organisation too low for a definite transfer to either the vegetable or animal kingdoms, offer a field of observation and experiment which may yet issue in giving us a clearer insight into the development of the organic living cell.'—'Whether an independent free-moving and assimilating organism, of a grade of structure similar to, and scarcely higher than, the "germ-cell," may not arise by a collocation of particles, through the operation of a force analogous to that which originally formed the germ-cell in the ovarian stroma, is a question which cannot be answered until every possible care and pains have been applied to its solution.' (P. 28.)

Professor Pouchet believes that he is authorised by the results of his experiments to answer that question in the affirmative. It is one of supreme importance, and which has, hitherto, never received such an amount of painstaking experimental research as it merits; and the best observations, the most carefully conducted and ingeniously devised arrangements for insuring success, are undoubtedly those of the patiently observant Professor of Zoology in the 'Ecole de Médecine,' and 'Ecole 'supérieure des Sciences,' at Rouen.* This, at least, may be affirmed, that the inductive groundwork of his opponents is by no means such as can justify any dogmatic negation of Heterogeny as applicable to the simplest Protozoa.

On the basis, therefore, of analogical probability, it may be inferred:—that the primordial as well as all other forms of organic beings, originate, and have ever originated, from the operation of secondary and continuously operating creative laws: and that the various grades of organisms now in being, from the microscopic monad upwards, indicate the various periods in time at which the first step of the series they respectively terminate began. The monad that by 'natural selection' has ultimately become man, dates from the farthest point in the remote past, upon which our feigners of developmental hypotheses can draw with unlimited

* Pouchet, 'Hétérogénie, ou Traité de la Génération spontanée, 'basé sur des nouvelles Expériences,' 8vo., 1859.

credit: the monad which by its superficial vibratile cilia darted across the field of the microscope we were looking through this morning, is the result of the collocation of particles which, without 'sudden flash,' took place under the operation of the heterogeneous organising force of yesterday.

Accordingly we find that every grade of structure, from the lowest to the highest, from the most simple to the most complex, is now in being,—a result which it is impossible to reconcile with the Darwinian hypothesis of the one and once only created primordial form, the parent of all subsequent living things. The changes which our planet has undergone in the course of geological time have been accompanied by the loss of many minor links which connected together the existing evidences of gradational structure; but the general laws regulating the progress and diversity of organic forms, having been the same throughout all time, so it happens, according to the testimony of the most experienced palæontologists, that—

'Every known fossil belongs to some one or other of the existing classes, and that the organic remains of the most ancient fossiliferous strata do not indicate or suggest that any earlier and different group of beings remains to be discovered, or has been irretrievably lost in the universal metamorphism of the oldest rocks.'*

That forms, recognised as species by their distinctive characters and the power of propagating them, have ceased to exist, and have successively passed away, is a fact now unquestioned; that they have been exterminated by exceptional cataclysmal changes of the earth's surface, as was surmised at the first acceptance of the fact of extinction, has not been proved; that their limitation in time may, in some instances, or in some degree, be due to constitutional changes, accumulating by slow degrees in the long course of generations, is possible: but all the traceable and observed causes of extirpation point either to continuous slowly operating geological changes, or to no greater sudden cause than the apparition of mankind on a limited tract of land not before inhabited. It is now, therefore, generally inferred that the extinction of species, prior to man's existence, has been due to ordinary causes—ordinary in the sense of agreement with the great laws of never-ending mutation of geographical and climatal conditions on the earth's surface. The individuals of species least adapted to bear such influences and incapable of modifying their organisation in harmony therewith, have perished. Extinction, therefore, on this hypothesis, is due to the want of

* Owen's Palæontology, p. 18.

self-adjusting, self-modifying power in the individuals of the species.

In the joint paper on the tendency of varieties to form species by natural means of selection*, one of the authors writes:—

'Any minute variation in structure, habits, or instincts, adapting the individual better to the new conditions, would tell upon its vigour and health. In the struggle it would have a better chance of surviving, and those of its offspring which inherited the variation would also have a better chance. Let this work go on for a thousand generations, and who will pretend to affirm,' asks Mr. Darwin, 'that a new species might not be the result?'

Thereupon is adduced the imaginary example of dogs and rabbits on an island, which we have already cited.

Now this, we take leave to say, is no very profound or recondite surmise; it is just one of those obvious possibilities that might float through the imagination of any speculative naturalist; only, the sober searcher after truth would prefer a blameless silence to sending the proposition forth as explanatory of the origin of species, without its inductive foundation.

In the degeneration-theory of Buffon, man is one of the primitive types,—the created apes and monkeys are derivatives. He might have illustrated it as follows:—

To give an imaginary example from changes in progress on an island: let the organisation of a wild man feeding chiefly on fruits become slightly plastic; let corresponding changes cause the sources of food on the ground very slowly to decrease, and those on the trees to increase: the effect of this would be that the man would try to climb more for food. Suppose also that a tiger or like destructive carnivore should swim over and settle in the island, which happened to be destitute of flints for weapons. The human organisation being slightly plastic, those individuals with the longest and strongest arms, and with the most prehensile use of the great toe, let the difference be ever so small, would be slightly favoured, would survive during that time of the year when food was scarcest on the ground, but ripe and ready on certain trees; they would also rear more young which would tend to inherit these slight peculiarities. The best climbers would escape the tigers, the worst would be rigidly destroyed.

Buffon would have seen no more reason to doubt that these causes, in a thousand generations, would produce a marked effect, and adapt the form of the wild man to obtain fruits rather

* By Darwin and Wallace, 'Proceedings of the Linnæan Society,' August, 1858, p. 45.

than grains, than Darwin now believes that man can be improved by selection and careful interbreeding into a higher, more heroic, more angelic form! The advocate of Buffon's hypothesis might point out that it is on islands, as Borneo and Sumatra, for example, where the orang-utan — the obvious result of such 'degradation by natural selection'—is exclusively found. And is it not there also, and in some other islands of the Malayan Archipelago, where the next step in the scale of 'degeneration' is exhibited in the still longer-armed Ungkas and other tail-less *Hylobates?* And though we call them 'tail-less' yet they have the 'os coccygis;' and this being a terminal appendage of stunted vertebræ, offers the very condition for the manifestation of an occasional developmental variety. If cats, after accidental mutilation or malformation, can propagate a tail-less breed, why may not apes produce a tailed variety, and by natural selection in a long course of ages, degenerate into endless incipient species of 'baboons and 'monkeys'?

But Mr. Darwin, it may be said, repudiates the coarse transmutational conditions and operations of Buffon and Lamarck; or, if there be any parallel between his and Buffon's illustration of the changing of species, at all events such parallels must run in opposite directions. Mr. Darwin starts from a single created prototype, from which it is difficult to conceive he can mean any other course of organic progress than an ascensive one. But of this, in the absence of a definition of the starting point, we cannot be perfectly sure. 'Natural selection' may operate in both directions. The following, for example, would have been cordially welcomed by Buffon as a testimony in favour of his 'dégénération' hypothesis:—

'In North America the black bear was seen by Hearne swimming for hours with widely open mouth, thus catching, like a whale, insects in the water. Even in so extreme a case as this, if the supply of insects were constant, and if better adapted competitors did not already exist in the country, I can see no difficulty in a race of bears being rendered, by natural selection, more and more aquatic in their structure and habits, with larger and larger mouths, till a creature was produced as monstrous as a whale.'*

If the ursine species had not been restricted to northern latitudes, we might have surmised this to have been one of the facts connected with 'the distribution of the inhabitants of South 'America,' which seemed to Mr. Darwin, when naturalist on board H.M.S. Beagle, 'to throw some light on the origin of

* Darwin, p. 184. (1st edition.)

species.'* But the close resemblance of the style, and of the tone and frame of mind which could see no difficulty in the adequacy of the above-cited circumstances of 'external con-'ditions, of habit, of volition,' to change a bear into a whale, to those exemplified in the 'Philosophie Zoologique,' point strongly to the writings of Lamarck as the true suggestor of Mr. Darwin's views of animated nature. We look, however, in vain for any instance of hypothetical transmutation in Lamarck so gross as the one above cited; we must descend to older illustrators of the favourite idea, to find an equivalent case of the bear in pursuit of water-insects, and we find one in the following:—

'Car il peut arriver, comme nous sçavons qu'en effet il arrive assez souvent, que les poissons ailés et volans chassant ou étant chassés dans la mer, emportés du desir de la proie ou de la crainte de la mort, ou bien poussés peut-être à quelques pas du rivage par des vagues qu'excitoit une tempête, soient tombés dans des roseaux ou dans des herbages, d'où ensuite il ne leur fut pas possible de reprendre vers la mer l'essor qui les en avoit tirés, et qu'en cet état ils ayent contracté une plus grande faculté de voler. Alors leurs nageoires n'étant plus baignées des eaux de la mer, se fendirent et se déjettèrent par la sécheresse. Tandis qu'ils trouvèrent dans les roseaux et les herbages dans lesquels ils étoient tombés, quelques alimens pour se soutenir, les tuyaux de leurs nageoires, séparés les uns des autres, se prolongèrent et se revêtirent de barbes; ou, pour parler plus juste, les membranes qui auparavant les avoient tenus collés les uns aux autres se métamorphosèrent. La barbe formée de ces pellicules déjetées s'allongea elle-même; la peau de ces animaux se revêtit insensiblement d'un duvet de la même couleur dont elle étoit peinte, et ce duvet grandit. Les petits ailerons qu'ils avoient sous le ventre, et qui comme leurs nageoires, leur avoient aidé à se promener dans la mer, devinrent des pieds, et leur servirent à marcher sur la terre. Il se fit encore d'autres petits changemens dans leur figure. Le bec et le col des uns s'allongèrent; ceux des autres se racourçirent: il en fut de même du reste du corps. Cependant la conformité de la première figure subsiste dans le total; et elle est et sera toujours aisée à reconnoître.

'Examinez en effet toutes les espèces de poules, grosses et petites, même celles des Indes, celles qui sont huppées, ou celles qui ne le sont pas; celles dont les plumes sont à rebours telles qu'on en voit à Damiette, c'est-à-dire, dont le plumage est couché de la queue à la tête; vous trouverez dans la mer des espèces toutes semblables, écailleuses ou sans écailles. Toutes les espèces de perroquets dont les plumages sont si divers, les oiseaux les plus rares et les plus singulièrement marquetés sont conformes à des poissons peints, comme eux, de noir, de brun, de gris, de jaune, de verd, de rouge, de violet, de couleur

* Darwin, p. 1. (1st edition.)

d'or et d'azur; et cela précisément dans les mêmes parties où les plumages de ces mêmes oiseaux sont diversifiés d'une manière si bizarre.'*

Demaillet, it must be admitted, enters more fully into the details of the operation of 'natural selection,' in changing the fish into the bird; and it is, perhaps, from this very 'naïveté' in the exposition of his theory, that its weakness has been made so obvious to later zoologists and comparative anatomists. Mr. Darwin rarely shows a fair front to these searching tests; *the facts of the manner of transmutation, as they might have* presented themselves to his fancy, are not stated with the 'abandon' of the old French philosopher. Vague and general as is the illustration based upon Hearne's remark, it is made still more vague in a later reprint of the volume 'On the Origin of Species.' It now reads, 'In North America the 'black bear was seen by Hearne swimming for hours with 'widely opened mouth, thus catching, almost like a whale, 'insects in the water.' (Ed. 1860, p. 184.)

'Individuals, it is said, of every species, in a state of nature 'annually perish,' and 'the survivors will be, for the most part, 'those of the strongest constitutions and the best adapted to 'provide for themselves and offspring, under the circumstances 'in which they exist.' Now, let us test the applicability of this postulate to the gradual mutation of a specific form by some instance in Natural History eminently favourable for the assumed results. In many species nature has superadded to general health and strength particular weapons and combative instincts, which, as, e. g., in the deer-tribe, insure to the strongest, to the longest-winded, the largest-antlered, and the sharpest-snagged stags, the choice of the hinds and the chief share in the propagation of the next generation. In such peculiarly gifted species we have the most favourable conditions for testing one of the conclusions drawn by Messrs. Darwin and Wallace from this universally recognised 'struggle for 'the preservation of life and kind.' If the offspring inheriting the advantages of their parents, did in their turn, however slightly and gradually, increase those advantages and give birth to a still more favoured progeny, with repetition of the result to

* 'Telliamed, ou Entretiens d'un Philosophe Indien avec un Mis-'sionaire François, *sur la Diminution de la Mer*,' &c., 8vo. Amsterdam, 1768. (An edition in two volumes of this original and suggestive work, was printed, with the life of the author (Demaillet), at the Hague, in 1755. The passage quoted will be found at p. 166, tom. ii. of this edition.)

the degree required by 'natural selection,'—then, according to the rate of modification experimentally proved in *pigeons*, we ought to find evidence of progressive increase in the combative qualities of antlers in those deer that for centuries have been under observation in our parks, and still more so in those that have fought and bred from the earliest historical times in the mountain wilds of Scotland. The element of 'natural selection' above illustrated, either is, or is not, a law of nature. If it be one, the results should be forthcoming; more especially in those exceptional cases in which nature herself has superadded structures, as it were expressly to illustrate the consequences of such 'general struggle for the life of the individual and the con-'tinuance of the race.'* The antlers of deer are expressly given to the male, and permitted to him, in fighting trim, only at the combative sexual season; they fall and are renewed annually; they belong moreover to the most plastic and variable parts or appendages of the quadruped. Is it then a fact that the fallow-deer propagated under these influences in Windsor Forest, since the reign of William Rufus, now manifest in the superior condition of the antlers, as weapons, that amount and kind of change which the successions of generations under the influence of 'natural selection' ought to have produced? Do the crowned antlers of the red deer of the nineteenth century surpass those of the turbaries and submerged forest-lands which date back long before the beginning of our English History? Does the variability of the artificially bred pigeon or of the cultivated cabbage outweigh, in a philosophical consideration of the origin of species, those obstinate evidences of persistence of specific types and of inherent limitation of change of character, however closely the seat of such characters may be connected with the 'best chance of taking care of self and of begetting offspring?' If certain bounds to the variability of specific characters be a law in nature, we then can see why the successive progeny of the best antlered deer, proved to be best by wager of battle, should never have exceeded the specific limit assigned to such best possible antlers under that law of limitation. If unlimited variability by 'natural selection' be a law, we ought to see some degree of its operation in the peculiarly favourable test-instance just quoted.

That the variability of an organism to a certain extent is a constant and certain condition of life we admit, otherwise there

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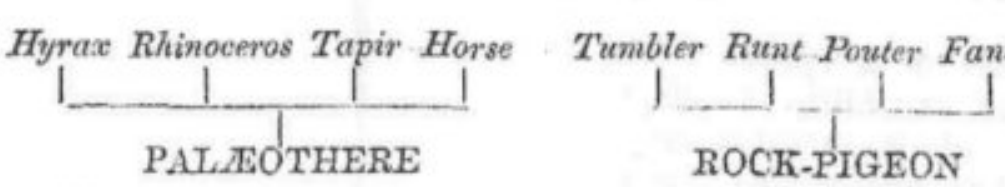
* 'Individual males have had, in successive generations, some slight advantage over other males in their weapons, and have transmitted these advantages to their male offspring.' (*Darwin*, p. 89.)

would be no distinguishable individuals of a species. The forester, by the operation of this law of variability, is able to distinguish his individual oaks, the shepherd his particular sheep, the teacher his several scholars. This true and proved law of variability is, in fact, the essential condition of individuality itself. We have searched in vain, from Demaillet to Darwin, for the evidence or the proof, that it is only necessary for one individual to vary, be it ever so little, in order to the conclusion that the variability is progressive and unlimited, so as, in the course of generations, to change the species, the genus, the order, or the class. We have no objection to this result of 'natural selection' in the abstract; but we desire to have reason for our faith. What we do object to is, that science should be compromised through the assumption of its true character by mere hypotheses, the logical consequences of which are of such deep importance.

The powers, aspirations, and missions of man are such as to raise the study of his origin and nature, inevitably and by the very necessity of the case, from the mere physiological to the psychological stage of scientific operations. Every step in the progress of this study has tended to obliterate the technical barriers by which logicians have sought to separate the inquiries relating to the several parts of man's nature. The considerations involved in the attempt to disclose the origin of the worm are inadequate to the requirements of the higher problem of the origin of man; and it may be that the conditions of that problem are beyond our present powers of acquiring certain knowledge.

To him, indeed, who may deem himself devoid of soul and as the brute that perisheth, any speculation, pointing, with the smallest feasibility, to an intelligible notion of the way of coming in of a lower organised species, may be sufficient, and he need concern himself no further about his own relations to a Creator. But when the members of the Royal Institution of Great Britain are taught by their evening lecturer that such a limited or inadequate view and treatment of the great problem exemplifies that application of science to which England owes her greatness, we take leave to remind the managers that it more truly parallels the abuse of science to which a neighbouring nation, some seventy years since, owed its temporary degradation. By their fruits may the promoters of true and false philosophy be known. We gazed with amazement at the audacity of the dispenser of the hour's intellectual amusement, who, availing himself of the technical ignorance of the majority of his auditors, sought to blind them as to the frail foundations of 'natural selection' by such illustrations as the subjoined:—



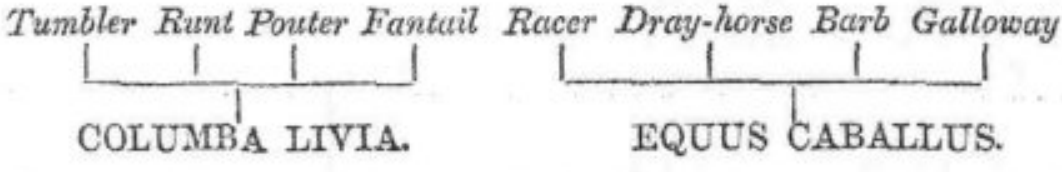

The above diagrams were set before an intelligent audience by a professor, in whom they naturally repose confidence as to facts specially belonging to his science, as parallel instances of departure from type: the one illustrating the extent and directions in which varieties diverge from a type form, in long course of time, by 'natural selection;' the other showing the correlative examples of such divergence, in a short course of time, through human selection. He told them that, in the latter series, the skeleton varied in regard to the number of vertebræ; but did not remark that it was in the variable region of the tail, on which no ornithologist ever depended for a specific character, neither did he state that the alleged difference in the number of dorsal vertebræ* was one that is merely simulated by a greater or less extent of the process of anchylosis over a region of the spinal column in which every vertebra was originally distinct. With regard to the parallel diagram, no allusion was made to such differences in the relative position of the cranial bones as the following:—viz., that in the palæotherium, as in the tapir, the maxillary bones intervene between and separate the nasal bones from the intermaxillary bones; whilst in the horse, as in the hyrax, the nasal and intermaxillary bones are united as far as their extremities; that, consequently, the external nostril is bounded by four bones in the horse, but by six in its implied progenitor; that there is as marked a difference in the conformation of the orbit, which is encircled by the union of the malar with the frontal bone in the horse, but is left widely open or incomplete, by the want of such union in the same two cranial bones of the palæothere. The advocate of the 'natural selection' view exaggerated resemblances and glossed over discrepancies of structure. The resemblance of the Palæothere to its four hypothetical descendants, in respect of their more generalised or more specialised structures, was flippantly affirmed to be as that of a father to his four sons!† Nothing was said to give his hearers a notion of the important difference between the horse and palæothere in the structure and implantation of the whole dental system. Yet the horse resembles

* Darwin, p. 22.

† Professor Huxley's Lecture 'On Species and Races and their 'Origin,' Friday, February 10th, 1860, Journal of the Royal Institution of Great Britain.

the elephant in having a long mass of complexly interblended dental substances deeply implanted in a large simple socket; whilst the palæothere differs from both in having a short mass or crown of differently disposed dental substances implanted by several long fangs in a correspondingly complex socket. To the competent anatomist a score of such anatomical differences would be present to the memory in contrasting the two alleged parallel series of differences from selection natural and human; to which differences in the palæontological series nothing comparable in essential value has been pointed out in the varieties of *Columba livia.* The competent palæontologist, moreover, would detect the superficial character of the knowledge that would interpose the tapir in any series leading from palæotherium: he would point to the eocene lophiodon as the true ancestor of the tapir on the derivative hypothesis.

Neither zoology nor physiology as yet, however, possesses a single fact to support the idea that six incisor and two canine teeth, as in the palæothere, could be blended or changed, by progressive transmutation, into the pair of large scalpriform teeth that projects from the fore part of the lower jaw in the hyrax or scriptural coney. The genuine cultivator of science and true representative of the minds on which the glory and greatness of nations depend, would feel bound to illustrate any series of observed varieties of a species by a true parallel. The hoofed mammals which afford this parallel with the diverging series of pigeons, are the following: —




Here the differences in regard to size, colour, development of tegumentary appendages, number of caudal vertebræ, length or stuntedness of muzzle, relative length of limb to body, &c., are closely analogous with the diversities which Mr. Darwin has dwelt upon in the first chapter of his work. And not only are the subjects of the above diagrams morphologically but physiologically alike; not merely are the differences of form and structure similar and equivalent, but the powers of procreation are the same. 'The hybrids or mongrels from between all the 'domestic breeds of pigeons are perfectly fertile;'* so, likewise, are the hybrids or mongrels from between all the domestic breeds of the horse. Now, as this is not the case with the hy-

* Darwin, p. 26.

brid between any variety of the horse and of the ass, it may be inferred that the physiological distinction would be, at least, as great, or more insuperable, between the horse and the tapir, or the rhinoceros, or the palæotherium. The infertility, or very rare fertility, of the solipedous mule, even when paired with a true horse or ass, and the absolute infertility of such hybrids *inter se*, are facts so notorious, that the professorial advocate of 'natural selection' was compelled to admit that his alleged parallel broke down at the physiological test,—the most important element of the comparison.

It is assumed by Mr. Darwin that variations, useful in some way to each being, occur naturally in the course of thousands of generations (p. 80.), that such variations are reproduced in the offspring, and, if in harmony with external circumstances, may be heightened in still further modified descendants of the species. The transmission and exaggeration of a variety, step by step, in the generative series, essential to the theory of 'natural selection,' implies the fertility of the individuals constituting the several steps of the series of transmutation. But numerous instances, familiar to every zoologist, suggest an objection which seems fatal to the theory, since they show extreme peculiarities of structure and instinct in individuals that cannot transmit them, because they are doomed to perpetual sterility.

The most numerous and important members of the hive, which collect the pollen on their peculiarly expanded thighs, and the honey in their peculiarly valvular crop or 'honey-bag,' and which, in the construction of cells of a shape adapted to contain the greatest possible quantity of honey with least possible consumption of wax, have practically solved a recondite mathematical problem, are the neuters, or females with abortive sexual organs,—'non-breeding females' of our great physiologist Hunter. From the hypothetical protoplastic progenitor of all animal species, what an enormous series of 'slight modifications of 'structure and instinct' must have rolled, snow-ball like, along the articulate line of departure, to have accumulated, according to 'natural selection,' in the *Apis mellifica*, which in the days of Moses exercised as now their structures and instincts in the 'land flowing with milk and honey!'

So also in the family of ants, the neuters or sterile females form, in certain species, two, or even three castes,—soldiers, workers, nurses. In *Cryptocerus* the workers of one caste 'carry 'a wonderful sort of shield on their heads;' in the Mexican genus, *Myrmecocystus*, the workers of one caste are fed by the workers of another caste, and have an enormously expanded ab-

domen where a sort of honey is secreted and stored, which, like domestic cattle, they supply to the rest of the community. Mr. Darwin, with one of his usual happy illustrations, compares the workers of the 'driver ant' (*Anomma*), to a 'set of workmen building a house, of whom many were five feet four inches high, and many sixteen feet high; but we must suppose that the larger workmen had heads four instead of three times as big as those of the smaller men, and jaws nearly five times as big;' in short, the most grotesque and extravagant scene in a pantomime is realised in the industrial community of a West African ant.

Yet all these instances of exaggerated peculiarities of structure and instinct are manifested in individuals which never could have transmitted them.

No zoologist, perhaps, is better acquainted with these fatal exceptions to his principle of the organisation of species by hereditary transmission of variation-characters, than Mr. Darwin. He could not, with any pretension to free and candid discussion, pass over the chief instances which have checked the natural disposition of all zoologists to obtain inductively an intelligible idea of the most mysterious phenomena of their science. But the barrier at which Cuvier hesitated, Mr. Darwin rushes through, and thus he disposes of the difficulty:—

'We have even slight differences in the horns of different breeds of cattle in relation to an artificially imperfect state of the male sex; for oxen of certain breeds have longer horns than in other breeds, in comparison with the horns of the bulls or cows of the same breeds. Hence I can see no real difficulty in any character having become correlated with the sterile condition of certain members of insect-communities: the difficulty lies in understanding how such correlated modifications of structure could have been slowly accumulated by natural selection.

'I have such faith in the powers of selection, that I do not doubt that a breed of cattle, always yielding oxen with extraordinarily long horns, could be slowly formed by carefully watching which individual bulls and cows, when matched, produce oxen with the longest horns; and yet no one ox could ever have propagated its kind. Thus I believe it has been with social insects: a slight modification of structure, or instinct, correlated with the sterile condition of certain members of the community, has been advantageous to the community: consequently the fertile males and females of the same community flourished, and transmitted to their fertile offspring a tendency to produce sterile members having the same modification.' (P. 238.)

It is a notorious and constant fact, that the castrate bovine has longer horns than either the perfect male or female. The progressively elongating result in the case of the oxen, about

which our theorist does not doubt, has not been proved experimentally. It is capable of proof or disproof. In scientific questions of far less import than the origin of animal species, involving our own, small value, if any, is attached to suppositious cases.

It is, doubtless, by no means necessary that we should sow a seed of the very cauliflower we eat in order to get more cauliflowers; seed of other individuals of the same stock will suffice. So the bee-keeper feels satisfied that the progeny of the impregnated young queen will exercise all the wonderful instincts which result in the production of wax and honey, as effectively as the virgin-sisters of the queen-mother, who were destroyed in the preceding winter. And our readers may well wonder what all this has to do with the explanation of the acquisition of the adaptive structures and instincts of neuter bees, by homœopathic doses of Lamarckian transmutation, accumulating through a long series of hereditary transmissions? We cannot reply; we can only quote, with no less amazement, our author: —

'This difficulty, though appearing insuperable, is lessened, or, as I believe, disappears, when it is remembered that selection may be applied to the family, as well as to the individual, and may thus gain the desired end. Thus, a well-flavoured vegetable is cooked, and the individual is destroyed; but the horticulturist sows seed of the same stock, and confidently expects to get nearly the same variety; breeders of cattle wish the flesh and fat to be well marbled together; the animal has been slaughtered, but the breeder goes with confidence to the same family.' (P. 237.)

Now every step in the production of the breed or family of cattle may have been observed and recorded; and many of the incidents of the transmutative journey of the edible variety of cabbage from the wild stock may be similarly known; but this is just the knowledge that we desiderate in regard to the creation of the honey-bee by the way of 'natural selection;' and, instead of satisfying our craving with the mature fruit of inductive research, Mr. Darwin offers us the intellectual husks above quoted, endorsed by his firm belief in their nutritive sufficiency!

To more intelligible propositions in support of his hypothesis, we marginally noted, as we perused them, the difficulties or exceptions which rose in our mind. We have still room for a few of these illustrations of the groundwork of 'natural 'selection.'

'From looking at species as only strongly-marked and well-defined varieties, I was led to anticipate that the species of the larger genera in each country would oftener present varieties, than the species of the smaller genera. To test the truth of this anticipation I have arranged the plants of twelve countries, and the coleopterous insects of two

districts, into two nearly equal masses, the species of the larger genera on one side, and those of the smaller genera on the other side, and it has invariably proved to be the case that a larger proportion of the species on the side of the larger genera present varieties, than on the side of the smaller genera.' (P. 55.)

The elephant is, however, a small genus, indeed one of the smallest in the sense of the number of species composing it, which are indeed but two, the *Elephas Indicus* and *Elephas Africanus* of Cuvier. But the range of variety in both African and Indian kinds is by no means inconsiderable. Livingstone adds instances in the elephants of the Zambesi, and the terms 'Dauntelah,' 'Mooknah,' &c., applied by the Indian and Singhalese entrappers of the wild proboscidians to the different varieties that are captured, still more exemplify this tendency to vary in individuals of a 'small genus.' Another exception to Mr. Darwin's rule as strongly and quickly suggests itself in the genus *Pithecus*. Naturalists seem unwilling to admit more species than the Bornean Pongo (*Pithecus Wurmbii*, seu *Satyrus* of Wurmb), and the smaller orang (*P. morio*), since established by Owen. But the varieties in regard to the cranial crests, to colour, to relative length of arms, appears by a memoir from the pen of the latter naturalist*, to be both numerous and well marked. On the other hand, the species of the antelope genus have not hitherto presented any notable varieties to the observation of naturalists; and yet the genus, in respect to the number of these species, is one of the largest in the mammalian class. There may be, of course, a difference in different classes of organisms in this respect. Plants and invertebrates may better exemplify Mr. Darwin's proposition than fishes, reptiles, or quadrupeds. But an hypothesis applied to all living things can only be sustained by laws and rules of a like generality of application.

Mr. Darwin's argument for a common origin of all the varieties of dovecot pigeon, leads him to affirm, 'all recent experience shows that it is most difficult to get any wild animal to breed freely under domestication.' (P. 24.) But the recent experience at the Zoological Gardens of London tells a different story. Three young individuals, two males and one female, of those most strange exotic quadrupeds, the giraffe, were transported from their African wilderness to the menagerie in Regent's Park in 1836. No sooner had they attained the proper age in 1838 than they bred; and there has been no other interval in the re-

* 'Characters of the skull of the male *Pithecus morio*, with remarks on the varieties of *Pithecus satyrus*,' by Professor Owen. Zoological Transactions, vol. iv. p. 163. 1856.

petition of the act than that which the phenomena of a fifteen months' gestation and seven months' suckling necessarily interpose. 'Nine fawns have been produced without any casualty.'* A pair of the largest and wildest of antelopes (the Eland, *A. Oreas*) is transported from the boundless sunny plains of South Africa to the confinement of a park in cloudy and rainy Lancashire; they breed freely there, and become the parents of elands now widely distributed over Great Britain, and promising in another century to be as common in our parks as fallow deer.† What conditions might seem more adverse to health and procreative power than such as are exemplified by the contrast of the den and the pond appropriated to the hippopotamus in the Jardin des Plantes, with that noble river where these most uncouth of African wild beasts disported themselves prior to their capture? Before two years elapse after the arrival of the young male and female, they produce a fine offspring.

Such are the signs of defective information which contribute, almost at each chapter, to check our confidence in the teachings and advocacy of the hypothesis of 'Natural Selection.' But, as we have before been led to remark, most of Mr. Darwin's statements elude, by their vagueness and incompleteness, the test of Natural History facts. Thus he says: —

'I think it highly probable that our domestic dogs have 'descended from several wild species.' It may be so; but what are the species here referred to? Are they known, or named, or can they be defined? If so, why are they not indicated, so that the naturalist might have some means of judging of the degree of probability, or value of the surmise, and of its bearing on the hypothesis?

'Isolation, also,' says Mr. Darwin, 'is an important element in 'the process of natural selection.' But how can one select if a thing be 'isolated'? Even using the word in the sense of a confined area, Mr. Darwin admits that the conditions of life 'throughout such area, will tend to modify all the individuals of 'a species in the same manner, in relation to the same conditions.' (P. 104.) No evidence, however, is given of a species having ever been created in that way; but granting the hypothetical influence and transmutation, there is no selection here. The author adds, 'Although I do not doubt that isolation is of con-'siderable importance in the production of new species, on the 'whole, I am inclined to believe, that largeness of area is of 'more importance in the production of species capable of spread-'ing widely.' (P. 105.)

* Edinb. Review, January, 1860, p. 179. † Ib., pp. 167–9.

Now, on such a question as the origin of species, and in an express, formal, scientific treatise on the subject, the expression of a belief, where one looks for a demonstration, is simply provoking. We are not concerned in the author's beliefs or inclinations to believe. *Belief is a state of mind short of actual knowledge.* It is a state which may govern action, when based upon a tacit admission of the mind's incompetency to prove a proposition, coupled with submissive acceptance of an authoritative dogma, or worship of a favourite idol of the mind. We readily concede, and it needs, indeed, no ghost to reveal the fact, that the wider the area in which a species may be produced, the more widely it will spread. But we fail to discern its import in respect of the great question at issue.

We have read and studied with care most of the monographs conveying the results of close investigations of particular groups of animals, but have not found, what Darwin asserts to be the fact, at least as regards all those investigators of particular groups of animals and plants whose treatises he has read, viz., that their authors 'are one and all firmly convinced that each of 'the well-marked forms or species was at the first independently 'created.' Our experience has been that the monographers referred to have rarely committed themselves to any conjectural hypothesis whatever, upon the origin of the species which they have closely studied.

Darwin appeals from the 'experienced naturalists whose 'minds are stocked with a multitude of facts' which he assumes to have been 'viewed from a point of view opposite to his own,' to the 'few naturalists endowed with much flexibility of mind,' for a favourable reception of his hypothesis. We must confess that the minds to whose conclusions we incline to bow belong to that truth-loving, truth-seeking, truth-imparting class, which Robert Brown *, Bojanus †, Rudolphi, Cuvier ‡, Ehrenberg §, Herold‖, Kölliker ¶, and Siebold, worthily exemplify. The rightly and sagaciously generalising intellect is associated with the power of endurance of continuous and laborious research, exemplarily manifested in such monographs as we have quoted below. Their authors are the men who trouble the intellectual world little with their beliefs, but enrich

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* Prodromus Floræ Novæ Hollandiæ.
† Anatome Testudinis Europææ.
‡ Mémoires pour servir à l'Anatomie des Mollusques.
§ Die Infusionsthierchen, als vollkommene Organismen.
‖ Disquisitiones de Animalium vertebris carentium, &c.
¶ Entwickelungsgeschichte des Cephalopoden.

it greatly with their proofs. If close and long-continued research, sustained by the determination to get accurate results, blunted, as Mr. Darwin seems to imply, the far-seeing discovering faculty, then are we driven to this paradox, viz., that the elucidation of the higher problems, nay the highest, in Biology, is to be sought for or expected in the lucubrations of those naturalists whose minds are not weighted or troubled with more than a discursive and superficial knowledge of nature.

Lasting and fruitful conclusions have, indeed, hitherto been based only on the possession of knowledge; now we are called upon to accept an hypothesis on the plea of want of knowledge. The geological record, it is averred, is so imperfect! But what human record is not? Especially must the record of past organisms be much less perfect than of present ones. We freely admit it. But when Mr. Darwin, in reference to the absence of the intermediate fossil forms required by his hypothesis — and only the zootomical zoologist can approximatively appreciate their immense numbers — the countless hosts of transitional links which, on 'natural selection,' must certainly have existed at one period or another of the world's history — when Mr. Darwin exclaims what may be, or what may not be, the forms yet forthcoming out of the graveyards of strata, we would reply, that our only ground for prophesying of what may come, is by the analogy of what has come to light. We may expect, e. g., a chambered-shell from a secondary rock; but not the evidence of a creature linking on the cuttle-fish to the lump-fish.

Mr. Darwin asks, 'How is it that varieties, which I have 'called incipient species, become ultimately good and distinct 'species?' To which we rejoin with the question:—Do they become good and distinct species? Is there any one instance proved by observed facts of such transmutation? We have searched the volume in vain for such. When we see the intervals that divide most species from their nearest congeners, in the recent and especially the fossil series, we either doubt the fact of progressive conversion, or, as Mr. Darwin remarks in his letter to Dr. Asa Gray*, one's 'imagination must fill up very wide 'blanks.'

The last ichthyosaurus, by which the genus disappears in the chalk, is hardly distinguishable specifically from the first ichthyosaurus, which abruptly introduces that strange form of sea-lizard in the lias. The oldest Pterodactyle is as thorough and complete a one as the latest. No contrast can be more remarkable,

* Proceedings of the Linnæan Society, 1858, p. 61.

nor, we believe, more instructive, than the abundance of evidence of the various species of ichthyosaurus throughout the marine strata of the oolitic and cretaceous periods, and the utter blank in reference to any form calculated to enlighten us as to whence the ichthyosaurus came, or what it graduated into, before or after those periods. The Enaliosauria of the secondary seas were superseded by the Cetacea of the tertiary ones.

Professor Agassiz affirms:—

'Between two successive geological periods, changes have taken place among plants and animals. But none of those primordial forms of life which naturalists call species, are known to have changed during any of these periods. It cannot be denied that the species of different successive periods are supposed by some naturalists to derive their distinguishing features from changes which have taken place in those of preceding ages, but this is a mere supposition, supported neither by physiological nor by geological evidence; and the assumption that animals and plants may change in a similar manner during one and the same period is equally gratuitous.'*

Cuvier adduced the evidence of the birds and beasts which had been preserved in the tombs of Egypt, to prove that no change in their specific characters had taken place during the thousands of years—two, three, or five—which had elapsed, *according to the monumental evidence*, since the individuals of those species were the subjects of the mummifier's skill.

Professor Agassiz adduces evidence to show that there are animals of species now living which have been for a much longer period inhabitants of our globe.

'It has been possible,' he writes, 'to trace the formation and growth of our coral reefs, especially in Florida, with sufficient precision to ascertain that it must take about eight thousand years for one of those coral walls to rise from its foundation to the level of the surface of the ocean. There are around the southernmost extremity of Florida alone, four such reefs, concentric with one another, which can be shown to have grown up one after the other. This gives for the beginning of the first of these reefs an age of over thirty thousand years; and yet the corals by which they were all built up are the same identical species in all of them. These facts, then, furnish as direct evidence as we can obtain in any branch of physical inquiry, that some, at least, of the species of animals now existing, have been in existence over thirty thousand years, and have not undergone the slightest change during the whole of that period.' †

To this, of course, the transmutationists reply that a still

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* Contributions to Natural History: Essay on Classification, p. 51.

† Ibid., p. 53.

longer period of time might do what thirty thousand years have not done.

Professor Baden Powell, for example, affirms:—'Though each 'species may have possessed its peculiarities unchanged for a 'lapse of time, the fact that when long periods are considered, all 'those of our earlier period are replaced by new ones at a later 'period, proves that species change in the end, provided a suffi-'ciently long time is granted.' But here lies the fallacy: it merely proves that species are changed, it gives us no evidence as to the mode of change; transmutation, gradual or abrupt, is in this case mere assumption. We have no objection on any score to the change; we have the greatest desire to know how it is brought about. Owen has long stated his belief that some pre-ordained law or secondary cause is operative in bringing about the change; but our knowledge of such law, if such exists, can only be acquired on the prescribed terms. We, therefore, regard the painstaking and minute comparisons by Cuvier of the osteological and every other character that could be tested in the mummified ibis, cat, or crocodile, with those of the species living in his time; and the equally philosophical investigations of the polypes operating at an interval of 30,000 years in the building up of coral reefs, by the profound palæontologist of Neuchatel, as of far higher value in reference to the inductive determination *of the question of the origin of species* than the speculations of Demaillet, Buffon, Lamarck, 'Vestiges,' Baden Powell, or Darwin.

The essential element in the complex idea of species, as it has been variously framed and defined by naturalists, viz., the blood-relationship between all the individuals of such species, is annihilated on the hypothesis of 'natural selection.' According to this view a genus, a family, an order, a class, a sub-kingdom,—the individuals severally representing these grades of difference or relationship, — now differ from individuals of the same species only in degree: the species, like every other group, is a mere creature of the brain; it is no longer from nature. With the present evidence from form, structure, and procreative phenomena, of the truth of the opposite proposition, that 'classifica-'tion is the task of science, but species the work of nature,' we believe that this aphorism will endure; we are certain that it has not yet been refuted; and we repeat in the words of Linnæus, '*Classis et Ordo* est sapientiæ, *Species* naturæ opus.'