ON AGASSIZ' VIEWS OF DARWIN'S THEORY OF SPECIES.*

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When the subject of Darwin's Theory of the "Origin of Species" was brought before this Society, a short time back, the paper of M. Agassiz, in the "Annals" of September, 1860, in which he severely criticises the views of Mr. Darwin, was prominently set forth, and characterized as "quite unworthy of so distingushed a naturalist." I then ventured to differ from the writer in this general estimate, and also to demur from the representation given of his fundamental statements; and I agreed to the invitation to defend the remarks of M. Agassiz at some future time. Since then, I have been waiting for an opportunity of laying before you a few observations upon this subject, premising that a more careful perusal of M. Agassiz' criticism has only confirmed me in the opinion I then expressed, viz., that I considered it to be one of the most conclusive and formidable (against Darwin's theory) which had yet appeared.

I know not how I can satisfactorily show the value of M. Agassiz' paper, except by taking up his principal positions *seriatim*, and endeavouring to prove their truth and logical accuracy; and this I shall hope to do, dwelling more particularly upon those points which were specially singled out for objection. Moreover, I shall not enter farther upon a discussion of Mr. Darwin's views than we are necessarily led by the subject-matter of Agassiz' criticism.

* This paper is an answer to certain statements made at the Second Ordinary Meeting of the Society, some of which will be found printed, commencing p. 42 of this volume.

And here let me premise that the criticism in question is one among a very few, which, proceeding from the pen of a profoundly scientific physiologist, and accurate observer, fairly addresses itself to the scientific and physiological aspects of the question—not repeating and corroborating Darwin's own somewhat easily appeased doubts, but attacking it at points which were not hitherto observed to be weak, and arising, as it might be almost said, accidentally, from some considerations relative to the degrees of individuality, and specific differences observed among Acalephs. There does not appear to me to be a word in this paper unworthy of, or inconsistent with, the character and attainments of the author of "An Essay on Classification," a work which, in my humble opinion, is a noble contribution to zoological enquiry, and is characterized by a solidity and accuracy of statement—a conformity with observed phenomena—a chain of logical sequence,—which favourably contrasts with the necessarily imperfect hypothesis of Mr. Darwin—with its gaps—its assumptions—and its demands upon our faith.* And this fact alone renders it, *à priori*, unlikely that the man who wrote the former should be guilty of anything approaching to petulancy or absurdity when reviewing the latter.

It would be presumptuous in the highest degree in me to consider it necessary, under ordinary circumstances, to stand forth as the apologist for one so eminent in every way as Louis Agassiz—one who is an ornament to science, and the pride of the country of his adoption. I only feel called upon to do so on your invitation, and because in the review

* It is, perhaps, necessary to state here, lest I should be misunderstood, that although by no means able to subscribe to the Darwinian hypothesis, I would not wish (as some appear to do) to condemn the whole theory as visionary and mischievous. Neither theory is capable of direct *proof*, and both are so dependent upon knowledge (not only the knowledge of an individual, but also the accumulated knowledge of an age), that I think it would betray an unworthy assumption of wisdom on my part wholly to reject the new one, virtually unheard and untested.

in question he has been charged with dogmatism and intentional obscurity. Did I think that these charges could be substantiated, I would at once lay down my pen; but I firmly believe that a candid enquiry will result differently.

M. Agassiz begins by advocating the idea—"That while species have no material existence, they yet exist as *categories of thought*, in the same way as genera, families, orders, classes, and branches of the animal kingdom;" and again, "that all the natural divisions of the animal kingdom are primarily distinct, founded upon different categories of characters, and that they all exist in the same way,—that is, as *categories of thought embodied in individual living forms*." Now, in all these expressions, I can see nothing obscure nor petulant—but, on the other hand, I *do* see the accurate reasoning of a philosophic mind, and a consistency in the support of a fundamental principle; which principle is in complete antagonism to Darwin's theory; so that there is no possibility of amalgamating the two ideas. Agassiz has, in all his works, maintained the same principle, and no one can reasonably object to his re-stating it on this occasion. But since it is complained that it is obscurely stated, let us examine whether it be not in truth logically and tersely expressed. What is a category of thought? I reply that it means a mental abstraction, in which all the predicates and all the attributes of the idea of species are arranged in an orderly series. Species exist in individuals—all the *similar* individuals existing at one time, embody the idea of a species; the individuals live—they are *objective*—they thrust themselves upon our notice as material beings—but something more than this *mere existence* is arrived at by our reasoning faculties, which *subjectively* infer that these forms represent an *idea*, which we may reasonably conceive was present with the Creator, when it pleased Him to make them, and that idea is *species*. Thus we arrive synthetically through all the characteristics of species,

so comprehensively described by Agassiz in his "Essay on Classification," at the generalization with which we imagine the Creator to have set out. Species, then, is an idea, not an entity; but an idea which sprang from the Eternal Mind.

"As the community of characters," says Agassiz, "among the beings belonging to these different *categories* arises from the intellectual connexion which shows them to be categories of *thought*, they cannot be the result of a *gradual material* differentiation of the objects themselves." I quote this passage because it was imagined, not only that it conveys no sense, but it was even suggested that the author wittingly wrote nonsense. Such a notion is to me incomprehensible; but, farther, I see in it a condensed chain of logical reasoning, which demands close study, and no superficial glance, to appreciate its whole import. The author has just been speaking of the several great plans upon which it has been demonstrated that the animal kingdom has been constructed. It has been shown by the labours chiefly of Owen, Huxley, and others, that no one plan can be constructed to which all animals are reducible. No *invertebrate* animal can be shown to be formed agreeable to the *vertebrate* plan; the molluscan and articulate plan have scarcely anything in common; and the cœlenterata and protozoa, moreover, differ in plan from either, and are probably not even mutually reducible. Here, then, community of *origin*, and community of *characters*, are not synonymous terms. There is *community of characters* in a *class*, but he distinctly asserts that "classes are founded upon different modes of execution of these plans, and, therefore, they embrace representatives which could have no *community of origin*." Moreover, in the sentence I am examining, it must be borne carefully in mind that there is an antithesis between the abstract idea, or category of thought, on the one hand, and the living embodiment, or individual forms, on the other. Bearing this in mind, let us now read the

sentence—"As the community of characters among the beings belonging to these different categories (*i.e.*, great branches of the animal kingdom, each formed upon a different plan) arises from the intellectual connexion which shows them to be categories of thought (in the sense explained above) they (that is, these different categories or abstract plans, embodied in vertebrata, mollusca, articulata, &c.) cannot be the result of a gradual *material* differentiation of the *objects* themselves." Here, the antithesis is between *intellectual*, in the first part of the sentence, and *material*, in the second—between *categories of thought* (or the idea of species) in the first part, and the *objects* (or the embodiment in living forms) in the second.* For Mr. Darwin tells us that the gradual material differentiation of individuals has given rise to all the *great plans* of structure. The whole sentence is pregnant with meaning—not a word is employed which has not a definite and necessary connexion with what precedes or with what follows; and the simple difference between the position of the writer and that of the reader is this, that Agassiz wrote the sentence with a full and thorough appreciation of all the bearings of the subject, every word being fraught with meaning in his mind, whereas, his reader having less grasp of the subject, has, necessarily to learn it by degrees, and by a dint of study of its contextual relations.

A few remarks may here be appropriately introduced upon the subject of these great plans, which appears to me to be one of the last importance.†

* "The leading objection of Mr. Agassiz is likewise of a philosophical [metaphysical] character. It is, that species exist only as *categories of thought*—that, having no material existence, they can have had no material variation, and no material community of origin. Here, the predication is of species in the subjective sense, the inference in the objective sense."—*Prof. Asa Gray, in Atlantic Monthly Magazine, October*, 1860.

† Nothing can prove more certainly the *natural* character of these four distinct plans than the fact that Von Baer and Cuvier, each of them independently arrived at the same conclusions concerning them—Von Baer, through the study of developmental changes, and Cuvier, by means of a close attention to the anatomical structure of animals.

These great plans of animal structure are not mutually reducible. However animals of different branches may agree in their *external* characters (as I showed at length in a paper read before the Society last session) no comparison can be instituted between their internal structure. No *series of forms* can be constructed, passing insensibly from one great branch to another. The highest forms of one branch are superior to the lowest forms of the branch next above it in organisation, but there is no *community of characters* between the two—each adheres to its own special type or plan. Thus, cephalopods may, in some points, be regarded as intermediate between mollusca and fishes, but the highest cephalopod is superior to the lowest fishes; nevertheless, as Von Baer remarks, "metamorphose a cephalopod as you will, there is no making a fish out of it, except by building up all the parts afresh." Darwin himself recognises this difficulty. Hence he says, in summing up, "I believe that all animals have descended from at most only *four* or *five* progenitors." But at this stage of his argument, the demands of his theory are imperative, and he adds—"Analogy would lead me one step further, namely, to the belief that all animals *and plants* have descended from some *one* prototype;" and arguing from what we must be excused from designating somewhat vague ideas of a *community of composition*, he adds this climax—"Therefore, I should infer from 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."*

* Much stress has been laid, in derivative hypotheses, upon the changes which the organism undergoes in embryo; and, truly, it must diminish our feeling of incredulity in, and repugnance to, the theory of derivation, when we reflect on these changes. *A priori*, it does not seem more incredible that some adult species should have arrived at their present condition by having passed through inferior forms during immense periods of time, than that embryos should (as we know they do) pass through various representative forms of lower types of animal life, previous to arriving at their permanent condition. Embryology shows us

Let me now proceed to the examination of Agassiz' further arguments. I pass over his caustic remarks upon the confusion of ideas implied in the general term, *variability of species;* and I must also necessarily pass by his categorical contradiction of many of Darwin's fundamental statements; but never was a theory more sorely beset than is that of Darwin by the repeated assaults of such a giant in palæontology as Agassiz. Statement after statement, by which the whole theory hangs together, is assailed and impugned—stone after stone of the Darwinian structure trembles before the battering-ram of the champion of species. Out of twelve such reiterated attacks, ten of which are purely palæontological, and stand unchallenged, only one has called for remark, and that one, perhaps, the least important. Nevertheless, believing, as I do, that Agassiz has written no line without an object, I am bound to bring it before the tribunal of criticism. He says—"He (Darwin) would have us believe that animals acquire their instincts gradually, when even those that never see their parents, perform at birth the same acts, in the same way, as their progenitors." Now, this appears at first sight to be such a truism, that it seems unnecessary either for Agassiz to state it, or for me to defend it. But we must not forget that Agassiz writes with especial reference to an argument before us all, viz., Darwin's work on the "Origin of Species." We must, therefore, consider this passage relatively to that work. In chapter 8, we read—"If we suppose any habitual action to become inherited, then the resemblance between what originally was a habit, and an instinct, becomes so close as not to be distinguished;" and again—"Under changed conditions of life, it is at least possible that slight modifications of instinct

that there is no *natural barrier* to development, as long as that development is confined to cognizable gradations. But we have yet to learn that the embryo of a vertebrate animal ever exhibits the articulate or molluscan type; and the primary distinction thus implied casts doubt and difficulty upon the other cases in which the transition *seems* more easy and simple.

might be profitable to a species; and if it can be shown that instincts *do* vary, ever so little, then I can see no difficulty in natural selection preserving, and continually accumulating variations of instinct to any extent that may be profitable." Here, then, Darwin compares instinct to habit, and argues concerning it as he would argue concerning habit. But *instincts* exhibit themselves at the very threshold of life, before it is possible for *habit* to be developed, which presupposes some experience. Hence, how can we "believe (to use the words of Agassiz) that animals acquire their instincts gradually, when even those who never see their parents, perform at birth the same acts, in the same way, as their progenitors?" It will be seen, that in this connexion, the argument is not carping nor superfluous, but forcible and cumulative.

I now pass to a more important part of the subject, namely, the remarks of Agassiz in regard to the assumed connexion between *affinity* and *genealogical relationship;* and, in the first place, I cannot construe his observations in any way so as to make him argue that "similarity between adult animals is but an agreement in a single stage; and if agreement in a single stage be sufficient to prove genealogical relationship—then, since the embryos of very distinct animals are much alike, there must be a close relationship between these very distinct animals." What he *does* say is this—"There is nothing *parallel* between the relations of animals belonging to the same *genus* or the same *family*, and the relations between the *progeny of common ancestors*. In the latter case, we have the result of a physiological law regulating reproduction, and in the former, affinities, which no observation has thus far shown to be in any way connected with reproduction." Here we have an argument, in which the opponent challenges Darwin for *facts* in support of his hypothesis that *affinities* among animals are evidence of *genealo-*

gical relationship. He proceeds—"The most closely allied species of the same genus, *or* the different species of closely allied genera, *or* the different genera of one and the same natural family, embrace representatives which, *at some period or other of their growth,* resemble one another more closely than the nearest blood-relations; and yet we know that they are only *stages of development* of different species, distinct from one another at every period of their life." Here is not a word about similarity between *adult* animals, but the whole argument is based upon *developmental* changes, and the *reductio ad absurdum* is not proven. Thus, proceeds Agassiz, "The *embryo* of our common freshwater turtle (*Chrysemys picta*) and the *embryo* of our snapping turtle (*Chelydra serpentina*) [distinct genera, be it observed] resemble one another far more than the different *species* of Chrysemys [a single genus] in their *adult* state; and yet not a single fact can be adduced to show that any one egg of an animal ever produced an individual of any species but its own." A great and overwhelming fact against the theory of derivation, since it proves that the character of the species impressed upon the germ from the beginning, by hereditary descent, is dominant through all the various changes, analogies, and differentiations through which the embryo passes; never swerving from its undeviating course, except by the force of unwonted disturbing causes, and even then returning by the shortest cut to its original form; so that, as Agassiz elsewhere happily expresses it, "while *individuals* are perishable, they transmit, generation after generation, all that is specific or generic (or in one word *typical*) in them, to the exclusion of every *individual peculiarity,* which passes away with them." How different this from what Darwin's theory would demand of us, which tells us that it is just these *individual peculiarities* which are preserved, and, by their accumulation, *alter* the type.

But the head and front of Agassiz' offence lies in the

following illustration. He says—"A young snake resembles a young turtle, or a young bird, much more than any two species of snakes resemble one another, and yet they go on reproducing their kinds and nothing but their kinds; so that no degree of affinity, however close, can, in the present state of our science, be urged as exhibiting any evidence of community of descent." There is no man living who has more right to speak authoritatively on embryology, particularly upon that of the Reptilia, than Agassiz. For the first four years of *my* existence, *he* dwelt, as a disciple, in the house of Ignatius Döllinger, the master of the great Von Baer, and of Pander, and the father of the science of embryology. His laborious and marvellous work on the "Embryology of the Turtle" (Boston, 1857), which forms a portion of the "Contributions to the Fauna of the United States," is a monument of science and industry, of which any nation may justly be proud. No author has more completely, more thoroughly, or more exhaustingly investigated this difficult branch of physiology than Agassiz; and his assertions on this subject are entitled to the very highest respect. It is conceded that when Agassiz writes of a *young* snake and a *young* bird, in this passage, he refers to an embryo snake and an embryo bird, and, indeed, to a *young* embryo. I can only, however, bring the authority of other eminent physiologists to corroborate the assertion of Agassiz, which, *to non-physiologists*, no doubt, appears somewhat startling. I quote the following passage, therefore, from "Carpenter's Comparative Physiology," p. 628—"All the most important parts of the apparatus of organic life, and even the fundamental portions of that of animal life, are developed upon the same general plan in all vertebrata; and the *special* peculiarities of each class only *gradually* evolve themselves. The conditions under which the alimentary canal, the heart and blood-vessels, the liver, the corpora Woolfiana, the vertebral column, the nervou

centres, and the eye and ear, first present themselves, exhibit no essential difference in the fish, reptile, bird, or mammal." Again, "the history of development," says Von Baer, "is the history of a gradually increasing differentiation of that which was at first homogeneous." The fundamental type, he elsewhere explains, is first developed, and afterwards more and more subordinate characters appear. In these passages is stated, then, the fact, well known to physiologists, which Agassiz has summed up and illustrated in the line in question.

But is the objector and doubter aware that Darwin himself mentions this very fact, quoting Agassiz as an authority. At p. 439 of the "Origin of Species," we read—"It has already been casually remarked that certain organs in the individual, which, when mature, become widely different, and serve for different purposes, are in the embryo exactly alike. The embryos, also, of distinct animals, *within the same class*, are often strikingly similar; a better proof of this cannot be given than *a circumstance mentioned by Agassiz, namely, that having forgotten to ticket the embryo of some vertebrate animal, he cannot now tell whether it be that of a mammal, bird, or reptile.*" Not, however, that this is anything new—for, a dozen years ago, Agassiz wrote—"To deny the reality of natural groups because of their early resemblances would be to take the resemblance for the reality. It would be the same as saying that the frog and the fish are identical, *because at one stage of embryonic life, it is impossible, with the means at our command, to distinguish them.*" And again, in another place—"Hence, the embryos of different animals resemble each other more strongly when examined in the earlier stages of their growth. We have already stated that during almost the whole period of embryonic life, the young fish and the young frog scarcely differ at all; *so it is also with the young snake compared with the embryo bird.*" The truth is, that at a certain period, the embryo of a snake and the embryo of a

bird *are* as much alike as the embryos of two snakes, and affinity *is* thus at fault in indicating relationship, which is, in fact, the argument of Agassiz, and one which cannot be gainsaid. This being the major proposition, the minor is, of course, included in it, namely, the assertion of Agassiz that "an *embryo snake* resembles an *embryo bird*, more than two *adult snakes* (of different species) resemble one another." The differences between embryos should undoubtedly be compared among themselves; but, nevertheless, if such differences are inappreciable in comparison with the distinct specific differences observed in adults, there can be no reason why the argument should not be strengthened by such a comparison.

There is yet one more portion of the paper of M. Agassiz, in which he has, as I conceive, been seriously misunderstood; and, it is a point not inferior in importance to any of those upon which I have already touched. Returning to the subject of *individuality* among Acalephs, with which he commenced the paper, he proceeds to specify the very remarkable modifications which the great "mystery of organic life" exhibits. First, he describes *hereditary individuality* as exhibited in all the higher animals. This is rare in Acalephs, and only exists in the Ciliogrades and some Pulmograde Discophoræ. Second, *derivative*, or *consecutive individuality*, such as occurs in the Nudibranchiata, which, from a single egg, produce more than one individual; this also occurs in such Medusæ as have what is termed an *alternation of generations*. Thirdly, *secondary individuality*, such as is inherent to those individuals arising as *buds* from other individuals, and remaining connected with them (as in the fixed Polyparia); and, fourthly, *complex individuality*, in which such a community acts as a single individual, while each individual member may perform distinct acts of its own. This last occurs as a character of the Siphonophoræ among Acalephs—the Physogrades of De Blainville.

There is thus among Acalephs, great diversity of indivi-

duality; and, moreover, a similar diversity is observed in the specific differences among them; or, in other words, a greater or less degree of polymorphism is remarked. With Ctenophoræ (or Ciliogrades of De Blainville) this polymorphic tendency is at a minimum; for here, not only are the individuals composing the group closely similar, but being all hermaphrodite, there is not even the polymorphism arising from difference of sex. *This*, however, *does* occur in the Pulmograde Discophoræ (to which our naked-eyed Medusæ belong), and sometimes the variations are very striking, as in Aurelia, one of the covered-eyed division, which has received from writers the names, Aurelia lineolata (Peron), A. radiolata (Lamarck) A. granulata, A. rosea, A. surirea, A. purpurata, Medusa purpurata (Penn), and Biblis Aquitaniæ (Lesson), the species indicated being in every case *Aurelia aurita* (Forbes). Deviations from the normal number of parts constitute another source of polymorphism. Next, the cycle of individual differences embraces two distinct types of individuals—the *Medusa* type and the *Hydra* type. *One* of these types may exhibit more or less diversity, there being frequently *two* kinds of Hydra united in one and the same community; or (though more rarely), two kinds of Medusæ, as among the Siphonophoræ (Physogrades). Thus, in the Diphydæ, which appear like pieces of transparent glass, and which were imagined by Cuvier and others to consist each of two distinct animals, always united, although separable with impunity, Professor Huxley shewed (Phil. Trans., 1849) that they consist of two constantly associated, though easily separated forms, slightly attached, but capable, for some time at least, of an independent existence. These two Medusa forms, one (anterior natatory body) including the other (posterior natatory body), may be very similar, as in Diphyes, or very dissimilar, as in Cuboides vitreus. In the latter, the including (anterior) individual is large and cuboid, the included (posterior) individual is small,

tetragonal, and campanulate; whereas in Abyla trigona, the reverse occurs, the including individual being here small, subcuboid, and campanulate; and the included, much larger, oblong, and polygonal.*

Agassiz goes on to argue from all this, as follows—"But notwithstanding the polymorphism among the individuals of one and the same community, genetically connected together, each successive generation reproduces the same kinds of heterogeneous individuals, and *nothing but* individuals linked together in the same way. Surely we have here a much greater diversity of individuals, born one from the other, than is exhibited by the most diversified breeds of our domesticated animals; and yet all these heterogeneous individuals remain true to their species, in one case as in the other, and do not afford the slightest evidence of a transmutation of species."

It is immediately after this that the passage follows, the objection to which has given rise to these remarks—"Would," says Agassiz, "the supporters of the fanciful theories, lately propounded, only extend their studies a little beyond the range of domesticated animals—would they investigate the alternate generations of the Acalephs—the extraordinary modes of development of the Helminth—the reproduction of the Salpæ, &c.,—they would soon learn that there are in the world far more astonishing phenomena, strictly circumscribed between the natural limits of unvarying species, than the slight differences produced by the intervention of man among domesticated animals, and, perhaps, cease to be so confident

* In Prof. Huxley's elaborate Monograph of the Oceanic Hydrozoa (Ray Society, 1859) the Cirrhigrada and Physograda of De Blainville are recast and differently arranged, forming the two families, Calycophoridæ and Physophoridæ. The including and included individuals spoken of in the text are regarded by him as organs of propulsion, and distinguished as proximal and distal nectocalyx. I have retained Cuboides as an illustration, because it does not appear that this is interfered with by Huxley's supposition that it is a Diphyozoöid, derived from Abyla. In Hippopodius, a genus of the Calycophoridæ, the nectocalyces are said to be as many as twelve in number.

as they seem to be, that these differences are trustworthy indications of the variability of species."

I have here fairly stated Agassiz' views—in fact quoted them nearly verbatim, simply adding illustrations; and, I need scarcely remark, in the first place, that these "astonishing phenomena" can, in no respect, be imagined to be *novelties* to M. Agassiz, who, thirteen years ago, published, in conjunction with A. A. Gould, the admirable "Outlines of Comparative Physiology," in which a chapter is devoted to a lucid exposition of these very changes. In the second place, that in Darwin's work on the "Origin of Species," the subject of the "alternate generations of Acalephs, the extraordinary modes of development of the Helminth, and the reproduction of the Salpæ," are altogether ignored, and find no place in the argument, being nowhere, in the remotest manner, alluded to. From this I deduce *three* things:—First, that no man had more cause than Agassiz, by reason of his thorough acquaintance with the subject in all its bearings, to be impressed with the vast importance of these polymorphisms and alternations in any question of biology so comprehensive as the Origin of Species. Secondly, I think we have every right to infer that these most curious and astonishing phenomena (notwithstanding the elaborate notice which he takes of the somewhat analogous phenomena of *neuter insects*) had not been regarded by Darwin in so important a light as to make him consider them a necessary part of his argument, or a possible objection to his theory; and, thirdly, that Agassiz, whose opinion on the question must command the highest respect, by calling attention to the omission, has done nothing more nor less than might reasonably have been looked for from so distinguished a Physiologist.

But I still maintain that the object of the whole reasoning of Agassiz in the passage quoted is simply to draw the reader's attention to the fact of the great extent to which polymorphism

obtains among Acalephs, and that he nowhere states that he regards the metamorphoses undergone by them in the alternations of generations, in the light of *varieties*. He simply includes, and very properly, the cycle of ovum, hydroid, and medusoid, in *one polymorphic species;* and the illustration is, therefore, perfectly just.

But if Agassiz *had* definitely stated his opinion that the transformations of the Medusæ could be regarded as varieties, his doctrine would not have been so heretical but that he would have received the support of many eminent physiologists, and among them of the late illustrious Professor E. Forbes, than whom few had more closely studied the Acalephæ, as his beautful Monograph, published by the Ray Society, amply testifies. At page 82 of that work, he says—"In what light are we to regard the relationship between the Medusa and Polyp? The one is *not the larva* of the other, as is often improperly said, because there is *no metamorphosis* of the one into the other. The first is the parent of the last, and the last of the first, but neither is a *stage* of an *individual* existence, destined to begin life as a Medusa and end it as a Poloyp, and *vicé versâ*.

In the case of Aurelia—

a. The Medusa produces eggs;
b. The eggs produce Infusoria;
c. The Infusoria fix, and become hydroid Polyps;
d. The hydroid Polyps produce Medusæ by gemmation.

With such facts, unquestioned facts, before us, it seems to me that we have no choice between theories, and we must admit the idea of *alternation of generations* to be true."

In point of fact, however, the phenomena of alternate generations, or "the production of dissimilar individuals among sexual animals, by a non-sexual process" (Allen Thomson) are far more astonishing than the transformations undergone by insects. For there is only the most superficial analogy between

the *alternation of generations* of Medusæ and Salpæ, and the *metamorphoses* of insects. In the latter there is a distinct change from one stage into another, readily traceable; in the former, the animals "remain different through their whole life, so that their relationship does not appear until a succeeding generation. The son does not resemble the father, but the grandfather; and in some cases the resemblance reappears only at the fourth or fifth generation (as in Distoma) or even later" (as in Aphis, at the ninth.) Thus in the case of the Acalephs—the little animal, which on leaving the egg, has the form of an infusory, passes in succession through the phases of Scyphistoma, Strobila and Ephyræ, so called, because, before these changes were understood, they were imagined to be different *genera*, and were named accordingly. "But the remarkable point in these metamorphoses is, that what was at first a single individual, is thus transformed by transverse division into a number of entirely distinct animals, which is not the case in ordinary metamorphoses. Moreover the upper segment [of the strobila] does not follow the others in their development. Its office seems to be accomplished as soon as the other segments begin to be independent; being intended merely to favour their development, by securing and preparing the substances necessary to their growth." Hence they are called Medusa polypiform *nurses*. "There is [then] this essential difference between the metamorphoses of the caterpillar, and alternate reproduction, that, in the former case, the same individual passes through all the phases of development; whereas, in the latter, the individual disappears, and makes way for another, which carries out what its predecessors had begun. It would give a correct idea of this difference to suppose that the tadpole, instead of being itself transformed into a frog, should die, having first brought forth young frogs; or that the chrysalis should, in the same way, produce young butterflies. In either case the young would still belong

to the same species, but the cycle of development, instead of being accomplished in a single individual, would involve two or more acts of generation."*

I must, however, bring the subject to a close; not for want of material, for it is almost inexhaustible, but because I have already exceeded the limits I had anticipated. I trust I have fulfilled my pledge of justifying the criticism of M. Agassiz from the charges which have been brought against it; and I believe I may safely leave the matter in the hands of those who have listened to my vindication.

At the conclusion of the paper, the PRESIDENT said he would not invite discussion at that late hour (10.5). He desired to express the great interest and pleasure which he was sure had been felt by all who had listened to the many instructive matters which had been brought before them in the paper which had just been read by Dr. Collingwood, who, nevertheless, he thought, had altogether failed in showing the injustice of his strictures on the criticism of M. Agassiz. He would confine his remarks to two points, and, after all that had been said, he considered the illustration of the embryo snake and the embryo bird was an appeal to ignorance and not to science; and that M. Agassiz' petulant and offensive assumption of want of information on the part of Mr. Darwin and his supporters, on a subject unquestionably familiar to them, justified his censure of M. Agassiz' review as unworthy of so distinguished a philosopher. He (the Rev. H. H. Higgins) was not a supporter of Mr. Darwin's theory, but he deeply regretted the uncandid manner in which it had been in many quarters attacked.

Dr. COLLINGWOOD observed that calling the similarities among embryos "an appeal to ignorance" was simply begging the question. He did not consider it necessary to repeat his arguments, but he would cheerfully abide by what he had written, and was quite content that it should stand as the record of his defence of Agassiz.

* The passages in inverted commas I have quoted from Dr. Wright's edition of Agassiz and Gould's "Outlines of Comparative Physiology," London, 1851. I have considered myself justified in doing this, for these reasons; first, because although a work of Agassiz himself, two other names are associated with his in it;—secondly, because the statements are the most succinct and definite I have been able to meet with;—and thirdly, because I believe they are still accepted as the correct and philosophical view of the subject of *alternation of generations*.

Dr. EDWARDS hoped the President would not leave the chair without affording him the opportunity of expressing the gratification he had received from Dr. Collingwood's able exposition of M. Agassiz' views, and he moved that the cordial thanks of the society be presented to him; for he felt sure that all present must have been pleased with the philosophical tone and highly instructive character of his paper, which, he considered, relieved M. Agassiz from the charge of wilful obscurity, and, at the same time, was exceedingly impartial on the general subject.

The Rev. C. H. BURTON seconded the motion, and confessed that he could not agree with the President's remarks, for he considered that Dr. Collingwood had successfully justified M. Agassiz.

SIXTH ORDINARY MEETING.

ROYAL INSTITUTION, 7th January, 1861.

The Rev. H. H. HIGGINS, M.A., PRESIDENT, in the Chair.

Mr. DAVID ANDERSON, of Egremont, was elected a member of the Society.

The PRESIDENT called attention to the magnificent illustrated works recently presented to the Royal Institution by the Emperor of the French, and the late King of Prussia, and which, having been handsomely bound, are now placed in the Library. Among the various works presented by the Emperor of the French was the beautiful one on the Catacombs of Rome; and the work of Lepsius on Egypt, from the late King of Prussia, was a very valuable addition. The town of Liverpool was indebted to Mr. T. C. Archer for these splendid donations.

The PRESIDENT also referred to Dr. Wallich's recently published "Notes on the Presence of Animal Life at Vast

Depths in the Sea," in which it is stated that in sounding not quite midway between Capes Farewell and Rockall, in 1,260 fathoms, a number of starfishes, belonging to the genus *Ophiocoma*, came up adherent to the lowest fifty fathoms of the deep sea line employed. Such facts as these were extremely interesting, because they are quite at variance with our existing ideas respecting the depth at which animal life can exist in the sea.

Mr. RICHARD BROOKE, F.S.A., then read a paper on—

"THE PROGRESS OF ART, SCIENCE, AND USEFUL INVENTIONS, SINCE THE MIDDLE AGES,"

in which he showed by an elaborate reference to the history of inventions, that most of our important and wonderful discoveries are of comparatively recent date.

SEVENTH ORDINARY MEETING.

ROYAL INSTITUTION, 21st January, 1861.

The Rev. H. H. HIGGINS, M.A., PRESIDENT, in the Chair.

The Rev. J. MACNAUGHT, M.A., and Mr. H. B. ROBERTS, were elected members of the Society.

Dr. COLLINGWOOD drew the attention of the meeting to the Reports of the Microscopical Section of the Literary and Philosophical Society of Manchester, in which it was stated that envelopes had been supplied by the society for distribution among captains of vessels to enable them to preserve soundings, and other similar materials, for microscopic examination; and, he further added, that directions for the

preservation of minute objects of natural history had been lately drawn up by Mr. William Weightman, with the object of calling the attention of captains to their collection under favourable circumstances.

The PRESIDENT referred to a lately published work by Professor Phillips, entitled "Life on the Earth, its Origin and Succession," which he strongly recommended to the notice of the members of the Society; and read a passage from it, which placed the difficulties of the developmental hypothesis in a very just and strong light.

Dr. COLLINGWOOD exhibited and remarked upon the valuable work just published by the Ray Society, on British Spiders, by Mr. John Blackwall. This work is copiously illustrated with beautifully coloured plates, and is the first of two volumes upon this hitherto neglected subject.

The Rev. J. ROBBERDS drew attention to a published paper by Mr. P. H. Holland, read before the Society of Arts, on the means of preventing coal-pit accidents, in which the writer urged the desirableness of compelling colliery-proprietors, by Act of Parliament, to insure the life of every miner employed, so as both to secure some provision for his family in case of accident, and also to make it the interest of the proprietors to enforce every known precaution against accidents, in order to reduce the rate of insurance. The writer calculated that an advance of only one penny per ton on the 66,900,000 tons of coal annually raised, would be sufficient to insure the life of every miner to the amount of £200.

The following paper was then read:—