CHAPTER XXV.

CONCLUDING REFLEXIONS.

To attempt at this stage any summary of conclusions would be misleading. The first object of this work is not to set forth in the present a doctrine, or to advertise a solution of the problem of Species, but rather to bring together materials that may help others hereafter to proceed with the solution of that problem. A general enumeration of particular conclusions is therefore to be avoided. Indeed, from the scantiness of the evidence, its present value is chiefly in suggestion, and the facts must therefore be themselves still studied in detail. The reader must interpret as he will.

But, as often happens, that which may not shew the right road is enough to shew that the way taken has been wrong, and so is it with this evidence. Upon the accepted view it is held that the Discontinuity of Species has been brought about by a Natural *Selection of particular terms in a continuous series of variations.* Of the difficulties besetting this doctrine enough was said in the introductory pages. These difficulties have oppressed all who have thought upon these matters for themselves, and they have caused some anxiety even to the faithful. And if in face of the difficulties reasonable men have still held on, it has not been that the obstacles were unseen, but rather that they have hoped a way through them would be found.

Now the evidence, of which a sample has been here presented, gives hope that though there be no way through the difficulties, there is still perhaps a way round them. For since all the difficulties grew out of the assumption that the course of Variation is continuous, with evidence that Variation may be discontinuous, for the present at least the course is clear again.

Such evidence as to certain selected forms of variations has, I submit, been given in these chapters, and so far a presumption is created that the Discontinuity of which Species is an expression has its origin not in the environment, nor in any phenomenon of Adaptation, but in the intrinsic nature of organisms themselves, manifested in the original Discontinuity of Variation.

But this evidence serves a double purpose. Though some may doubt whether the variations here detailed are such as go to the building of Specific Differences (a doubt which, it must be granted, does fairly attach to some part of the evidence), yet the existence of sudden and discontinuous Variation, the existence, that is to say, of new forms having from their first beginning more or less of the kind of *perfection* that we associate with normality, is a fact that disposes, once and for all, of the attempt to interpret all perfection and definiteness of form as the work of Selection. The study of Variation leads us into the presence of whole classes of phenomena that are plainly incapable of such interpretation.

The existence of Discontinuity in Variation is therefore a final proof that the accepted hypothesis is inadequate. If the evidence went no further than this the result would be of use, though its use would be rather to destroy than to build up. But besides this negative result there is a positive result too, and the same Discontinuity which in the old structure had no place, may be made the framework round which a new structure may be built.

For if distinct and "perfect" varieties may come into existence discontinuously, may not the Discontinuity of Species have had a similar origin? If we accept the postulate of Common Descent this expectation is hard to resist. In accepting that postulate it was admitted that the definiteness and Discontinuity of Species depends upon the greater permanence or stability of certain terms in the series of Descent. The evidence of Variation suggests that this greater stability depends primarily not on a relation between organism and environment, not, that is to say, on Adaptation, but on the Discontinuity of Variation. It suggests in brief *that the Discontinuity of Species results from the Discontinuity of Variation.*

This suggestion is in a word the one clear and positive indication borne on the face of the facts. Though as yet it is but an indication, there is scarcely a problem in the comparison of structures where it may not be applied with profit.

The magnitude and Discontinuity of Variation depends on many elements. So far as Meristic Variation is concerned, this Discontinuity is primarily associated with and results from the fact that the bodies of living things are mostly made up of repeated parts—of organs or groups of organs, that is to say, which exhibit the property of "unity," or, as it is generally called, "individuality." Upon this phenomenon depends the fact that Meristic Variation in number of parts is often integral, and thus discontinuous.

The second factor that most contributes to the Discontinuity of Variation is Symmetry, manifesting its control in the first place directly, leading often to a result that we recognize as definite and perfect because it is symmetrical.

But besides this direct control that we associate with Symmetry, other effects greatly contributing to the magnitude of Variation can be traced to a factor not clearly to be distinguished from Symmetry itself. For, as has been explained, Symmetry, whether Bilateral or Radial, is only a particular case of that phenomenon of Repetition of Parts so universally characteristic of living bodies; and that resemblance between two counterparts, which we call Bilateral Symmetry, is akin to the resemblance between parts repeated in Series, though, as is shewn by their geometrical relations, the processes of division by which the parts were originally set off, must be in some respects distinct. Bilateral Symmetry of Variation is thus only a special case of the similar and simultaneous Variation of repeated parts.

The greatness of the observed change from the normal is often largely due to this possibility of simultaneity in Variation, the change thus manifesting itself not in one part only, but in many or all of the members of a series of repeated parts. Instances of such similar and simultaneous Variation of serial parts in animals have now been given. Examples still more marked may be seen abundantly among plants. A variation, for example, in the form or degree of fission of the leaf, slight perhaps by itself, when taken up and repeated in every leaf in its degree, constitutes a definite and conspicuous distinction. Everyone has observed this common fact. Few illustrations of it are more evident than that of the common Hawthorn. In a quickset hedge soon after the leaves begin to unfold almost each separate plant can be recognized even at a distance, and its branches can be traced by their special characters, by the shapes and tints of the leaves, by the angles that they make with the stem, by the manner of unfolding of the buds, and so forth. These variations, sometimes slight in themselves, by their similarity and simultaneity build up a conspicuous result.

The phenomenon of serial resemblance is in fact an expression of the capacity of repeated parts to vary similarly and simultaneously. In proportion as in their variations such parts retain this capacity the relationship is preserved, and in proportion as it is lost, and the parts begin to vary independently, exhibiting differentiation, the relationship is set aside. It will be noticed that to render the converse true we must extend the conception of Serial Homology in special cases to organs not commonly regarded as serially homologous with each other, but which having assumed some common character thereafter may vary together (cp. p. 309).

In the power of independent Variation, members of series once more exhibit the property of "unity" that we have already noticed as appearing in the manner in which the number of the members is changed. The fact that members of series should be capable of varying as "individuals" is paradoxical. Such members, teeth, digits, segments of Arthropods, and the like, are each made up of various tissues endowed with miscellaneous functions and dissimilar in their morphological nature. Nevertheless each group is capable of independent division and of separate Variation. Single digits for instance may thus be independently hypertrophied as a whole, single segments or single appendages or pairs of appendages may be differentiated in some special way, and so forth.

At this point reference may again be made to that extraordinary Discontinuity of Variation appearing in what I have called Homœosis, so strikingly seen in the few Arthropod cases given (p. 146), and so common in flowering plants. In these changes a limb, a floral segment, or some other member, though itself a group of miscellaneous tissues, may suddenly appear in the likeness of some other member of the series, assuming at one step the condition to which the member copied attained presumably by a long course of Evolution.

Many times in the course of this work we have had occasion to consider the modifications in the conception of Homology demanded by the facts of Variation. It is needless to speak further of this matter here, and the reader is referred to pp. 125, 191, 269, 394 and 417, where the subject is discussed in relation to Linear Series of several kinds, and to the facts given in Chapter XVI and at p. 433 bearing on the same questions in their application to Radial Series. The outcome of these considerations shews, as I think, that the attribution of strict individuality to each member of a series of repeated parts leads to absurdity, and that in Variation such individuality may be set aside even in a series of differentiated members. It appears that the number of the series may be increased in several ways not absolutely distinct, that a single member of the series may be represented by two members, that a terminal member may be added to the series, and also that the number of the members may change, no member precisely corresponding in the new total to any one member of the old series: in short, that with numerical change resulting from Meristic Variation there may be a redistribution of differentiation.

But though this is, in my judgment, a fact of great consequence, its relation to the Study of Variation is merely incidental. It is not so much that to enlarge the conception of Homology so as to include the phenomena of Meristic Variation is a direct help, as that to maintain the old view is a hindrance and keeps up an obstacle in the way of any attempt to apprehend the real nature of the phenomena of Division, and hence of Meristic Variation. So long as it is supposed that each member of a series of repeated parts is literally *individual*, it is impossible to form any conception of Division that shall include the facts of Meristic Variation, for in Variation it is found that the members are divisible.

It is an unfortunate thing that the study of Homology has been raised from its proper place. The study of Homologies was at first undertaken as a means of analyzing the structural evidences of relationship, and hence of Evolution. This is its proper work and use; but the pursuit of this search as an aim in itself has led to confusion, and has tended to conceal the fact that there are phenomena to which the strict conception of individual Homology is not applicable.

This exaggerated estimate of the fixity of the relationship of Homology has delayed recognition of the Discontinuity of Meristic Variation, and has fostered the view that numerical Variation must be a gradual process.

This view the evidence shews to be wrong, as it was also improbable.

Brief allusion may be made to three separate points of minor im-importance.

It is perhaps true that, on the whole, series containing large numbers of undifferentiated parts more often shew Meristic Variation than series made up of a few parts much differentiated, but throughout the evidence a good many of the latter class are nevertheless to be seen.

Reference may be made to a point that might with advantage be examined at length. The fact that Meristic Variation may take place suddenly leads to a deduction of some importance bearing on the expectation that the history of development is a representation of the course of Descent. In so far as Descent may occur discontinuously it will, I think, hardly be expected that an indication of the previous term will appear in the ontogeny. For example, if the four-rayed *Tetracrinus* may suddenly vary to both a five-rayed and also to a three-rayed form (see p. 437) it is scarcely likely that either of these should go through a definitely four-rayed stage; and if the origin of the four-rayed form itself from the five-rayed form came similarly as a sudden change, it would not be expected that a five-rayed stage would be found in its ontogeny. Similarly, if a flower with five regular segments arise as a sport from a flower with four, it would not, I suppose, be expected that the fifth segment would arise in the bud later than the other four. I suggest these examples from Radial Series, as in them the question is simpler, but similar reasoning may be applied to many cases of Linear Series also.

It will be noted that the attempt to apply to numerical variations the conception of Variation as an oscillation about *one* mean is not easy, difficulty arising especially in regard to the choice of a unit for the estimation of divergence. In few cases can facts be collected in quantity sufficient even to sketch the outline of such an investigation; but, to judge from the scanty indications available, it seems that in cases of numerical change variations to numbers greater than the normal number, and to numbers less than it are not generally of equal frequency. Probably no one would expect that they should be so.

As was stated in the Introduction, we are concerned here with the manner of origin of variations, not with the manner of their perpetuation. The latter forms properly a distinct subject. We may note however, in passing, how little do the few known facts bearing on this part of the problem accord with those ready-made principles with which we are all familiar. Upon the special fallacy of the belief that great Variation is much rarer in wild than in domesticated animals we have often had occasion to dwell. As was pointed out in the discussion of the evidence on Teeth (p. 266) this belief arises from the fact that domesticated animals are for the most part variable, and that we have every opportunity of observing and preserving their variations. To compare rightly their variability with that of wild animals choice should be made of animals that are also variable though wild. Taken in this way the comparison is fair, and as I have already said, if we examine the variation in the vertebræ of the Sloths, in the teeth of the Anthropoid Apes, in the colour of the Dog-whelks (*Purpura lapillus*), &c., we find a frequency and a range of Variation matched only by the most variable of domesticated animals.

It is needless to call attention to the fact that in hardly any cases even of extreme variations in wild creatures is there evidence that the animal was unhealthy, or ill nourished, or that its economy was in any visible way upset; but in almost every example, save for the variation, the body had the appearance of normal health.

After all that has been said few perhaps will still ask us to believe that the fixity of a character is a measure of its importance to the organism. To try to apply such a doctrine in the open air of Nature leads to absurdity. Let one more case be enough. I go into the fields of the North of Kent in early August and I sweep the Ladybirds off the thistles and nettles of waste places. Hundreds, sometimes thousands, may be taken in a few hours. They are mostly of two species, the small *Coccinella decempunctata* or *variabilis* and the larger *C. septempunctata*. Both are exceedingly common, feeding on Aphides on the same plants in the same places at the same time. The former (*C. decempunctata*) shews an excessive variation both in colours and in pattern of colours, red-brown, yellow-brown, orange, red, yellowish-white and black, in countless shades, mottled or dotted upon each other in various ways. The colours of pigeons or of cattle are scarcely more variable. Yet the colour of the larger *C. septempunctata* is almost absolutely constant, having the same black spots on the same red ground. The slightest difference in the size of the black spots is all the variation to be seen. (It has not even that dark form in which the black spreads over the elytra until only two red spots remain, which is to be seen in *C. bipunctata.*) To be asked to believe that the colour of *C. septempunctata* is constant because it matters to the species, and that the colour of *C. decempunctata* is variable because it does not matter, is to be asked to abrogate reason.

But the significance of the facts does not stop here. When, looking further into the variations of *C. decempunctata* it is found that most of its innumerable shades of variation are capable of being grouped round some eight or ten fairly distinct types, surely an expectation is created in the mind that the distinctness of these forms of varieties, all living [and probably breeding] together, may be of the same nature as the distinctness of Species; and since it is clear that the distinctness of the varieties is not the work of separate Selection we cannot avoid the suspicion that the same may be true of the specific differences too.

An error more far-reaching and mischievous is the doctrine that a new variation must immediately be swamped, if I may use the term that authors have thought fit to employ. This doctrine would come with more force were it the fact that as a matter of experience the offspring of two varieties, or of variety and normal, does usually present a mean between the characters of its parents. Such a simple result is, I believe, rarely found among the facts of inheritance. It is true that with regard to this part of the problem there is as yet little solid evidence to which we may appeal, but in so far as common knowledge is a guide, the balance of experience is, I believe, the other way. Though it is obvious that there are certain classes of characters that are often evenly blended in the offspring, it is equally certain that there are others that are not.

In all this we are still able only to quote case against case. No one has found general expressions differentiating the two classes of characters, nor is it easy to point to any one character that uniformly follows either rule. Perhaps we are justified in the impression that among characters which blend or may blend evenly, are especially certain quantitative characters, such as stature; while characters depending upon differences of number, or upon qualitative differences, as for example colour, are more often alternative in their inheritance. But even this is very imperfectly true, and as appeared in the case of Earwigs (p. 40) there may be a definite dimorphism in respect of a character which to our eye is simply quantitative. Nevertheless it may be remembered that it is especially by differences of number and by qualitative differences that species are commonly distinguished. Specific differences are less often quantitative only.

But however this may be, whatever may be the meaning of alternative inheritance and the physical facts from which it results, and though it may not be possible to find general expressions to distinguish characters so inherited from characters that may blend, it is quite certain that the distinctness and Discontinuity of many characters is in some unknown way a part of their nature, and is not directly dependent upon Natural Selection at all.

The belief that all distinctness is due to Natural Selection, and the expectation that apart from Natural Selection there would be a general level of confusion, agrees ill with the facts of Variation. We may doubt indeed whether the ideas associated with that flower of speech, "Panmixia," are not as false to the laws of life as the word to the laws of language.

But beyond general impression, in this, the most fascinating part of the whole problem, there is still no guide. The only way in which we may hope to get at the truth is by the organization of systematic experiments in breeding, a class of research that calls perhaps for more patience and more resources than any other form of biological inquiry. Sooner or later such investigation will be undertaken and then we shall begin to know.

Meanwhile, much may be done to further the Study of Variation even by those who have none of the paraphernalia of modern science at command. *Many of the problems of Variation are pre-eminently suited for investigation by simple means.* If we are to get further with these problems it will be done, I take it, chiefly by study of the common forms of life. There is no common shell or butterfly of whose variations something would not be learnt were some hundreds of the same species collected from a few places and statistically examined in respect of some varying character. Any-one can take part in this class of work, though few do.

At the present time those who are in contact with the facts and material necessary for this study care little for the problem, or at least rarely make it the first of their aims, and on the other hand those who care most for the problem have hoped to solve it in another way.

These things attract men of two classes, in tastes and tempera ment distinct, each having little sympathy or even acquaintance with the work of the other. Those of the one class have felt the attraction of the problem. It is the challenge of Nature that calls them to work. But disgusted with the superficiality of "naturalists" they sit down in the laboratory to the solution of the problem, hoping that the closer they look the more truly will they see. For the living things out of doors, they care little. Such work to them is all vague. With the other class it is the living thing that attracts, not the problem. To them the methods of the first school are frigid and narrow. Ignorant of the skill and of the accurate, final knowledge that the other school has bit by bit achieved, achievements that are the real glory of the method, the "naturalists" hear only those theoretical conclusions which the laboratories from time to time ask them to accept. With senses quickened by the range and fresh air of their own work they feel keenly how crude and inadequate are these poor generalities, and for what a small and conventional world they are devised. Dis-appointed with the results they condemn the methods of the others, knowing nothing of their real strength. So it happens that *for them the study of the problems of life and of Species* becomes associated with crudity and meanness of scope. Beginning as naturalists they end as collectors, despairing of the problem, turning for relief to the tangible business of classification, accounting themselves happy if they can keep their species apart, caring little how they became so, and rarely telling us how they may be brought together. Thus each class misses that which in the other is good.

But when once it is seen that, whatever be the truth as to the modes of Evolution, it is by the Study of Variation alone that the problem can be attacked, and that to this study both classes of observation must equally contribute, there is once more a place for both crafts side by side: for though many things spoken of in the course of this work are matters of doubt or of controversy, of this one thing there is no doubt, that if the problem of Species is to be solved at all it must be by the Study of Variation.