Evolutionary Advance: Emergent and Resultant.[1]

By Prof. C. LLOYD MORGAN, F.R.S.

THERE seems to be not a little misapprehension as to the position which those who advocate emergent evolution are concerned to defend. Some critics seem to suppose that the contention is: All evolution is by discrete steps, each of which introduces something new; therefore no evolution is by continuous advance with resultant outcome. That is not so. At any rate, I, for one, disclaim intention of saying anything of the sort. It has been my aim to emphasise the claim that what is genuinely new in evolutionary advance is of the emergent type, as distinguished from the resultant type. My claim is: Some evolution is by discrete steps, each of which introduces something new. But stress on emergent factors in evolution does not imply denial of resultant effects.

When we consider organic evolution this must be borne in mind. If the biologist adduces thousands of examples of changes in living organisms which are interpretable mechanically as strictly resultant, that is no argument which serves to disprove the occurrence of changes which, as we think, must be interpreted as strictly emergent. If both types of change are in evidence, our aim should be to distinguish the one type from the other.

The emergent claim is (1) that there are certain characterising features of the living that cannot be deduced from our knowledge of what happens on the lower platform of the not-living. But the further contention is that this holds good, not only for the living and the not-living, but also at many stadia of evolutionary advance; so that, on like empirical grounds, we may say, for example (2), that there are certain characterising features of the molecule that cannot be deduced from our knowledge of what happens on the lower platform of the atom.

It is with the former claim that we are here concerned. Then the trouble is that one who advocates emergent evolution is sometimes supposed to deny resultant evolution. He is supposed to say in effect: *Not* resultant advance, *but* emergent advance.

Let us consider the attitude of those who do nothing of the sort. In resultant advance the conditions are such that there is homogeneous continuity. Hence deductive conclusions are relevant all along the line of advance. With adequate knowledge of the law of such advance, predictions as to the exact nature of any later phase could be made on the basis of adequate and sufficient knowledge of any earlier phase. Hence the unlimited range of astronomical predictions in so far as they are based on the principles of resultant mechanics.

Now what is the bearing of this on the vexed biological issue? The 'mechanist' says in effect that all processes and products from first to last—from the not-living to the living organism—are susceptible of resultant interpretation. They are all on one continuous plane of resultant advance. And he points with justifiable pride, which others may share, to the outcome of such treatment. There are, no doubt, as he frankly admits, sundry physiological processes which still present difficulties. What of that? Further research on this method of interpretation will resolve them in due time.

What, then, say those who have been led to accept emergent advance? Do they deny any one of the successful achievements based on resultant treatment? They do not. What they do submit is that there are modes of 'behaviour' in the clustering of events within the living organism that are of such a nature as not to be deducible from that which obtains in the not-living. They submit, in further detail, that there are *some* physiological processes which elude the meshes of the resultant net, which are on a different level of emergence, which could not be predicted from the not-living platform.

It may be asserted that with further knowledge it will be shown that there are *no* physiological processes that elude the resultant net. We are, however, dealing with matters as they now are; and our attitude is: Resultant advance in plenty; as much as can be proved; but not a few residual matters which bear witness to emergent advance. If this be so, is not the present position of affairs this: The living organism in physiological regard is such as to exemplify evolutionary advance, not resultant only, not emergent only, but both resultant and emergent?

Should not this be our attitude in broader biological regard? Now that the concept of emergence has been admitted into the field of serious discussion, there is grave danger of its being used wildly and without discrimination as a popular catchword. People talk of the emergence of the elephant or the mongoose; the emergence of the social Hymenoptera, of polymorphism in ants; perhaps the emergence of mimicry or of display in courtship.

It may, however, be said: We thought that evolutionary advance is what you stand for. But now it seems that you propose to introduce sundry rather puzzling reservations. If polymorphism in ants—to select one of your examples—if, in other words, the differences of structure and diversities of behaviour that characterise the constituent members within some social community of ants, be not the outcome of evolutionary process, of what natural process is all this the outcome?

I do not suggest that all this is not the outcome of, or does not afford an instance of, *evolutionary* advance. My aim is to distinguish, within this advance, (1) that which is deducible on the method of resultant treatment, from (2) that which is not deducible on this method. The former I speak of as resultant advance; the latter as emergent advance. I submit that, on the evidence, we find

[1] From a paper read at a meeting of the Aristotelian Society on Feb. 14.

in the field of biological inquiry both emergent and resultant advance. My plea is for careful analysis.

There are a good many critics who seem not yet to have grasped just where the concept of emergent evolution is applicable. They seek to apply it where I, for one, hold it to be inapplicable. They may then ask: What bearing has this concept of emergence on the theory of natural selection? It may savour of extravagance if I express the opinion that on this theory, as such, it has little or no bearing.

To make my meaning clear, I must ask: Are we, under natural selection, dealing with the survival of variants or with the origin and transmission of variations? In the opinion I express I assume that the theory of natural selection *as such* deals with variants, and that the origin and transmission of variations fall for discussion under a different theory—that of genetics. If this be so, the issue for natural selection is a plain issue. Are some variants weeded out in 'the struggle for existence' or are they not? If some are weeded out, leaving others to survive, I regard such elimination as a resultant effect.

That leaves the origin of variations (or of mutations) to be discussed as a separate issue under genetics. It opens up a wide field of inquiry, including Mendelian research. Here the question does arise: Is this or that variant the outcome of resultant, or emergent, advance; or is it a joint product of both? If both are given in the evidence, the emergent factors should be distinguished.

My plea is: If the concept of emergence be accepted, let us make quite clear just where this concept is applicable. When I express the opinion that it is not applicable to natural selection, as such, it should be obvious that this does not preclude the survival of those variants which have genetic characters that can be shown, under searching analysis, to be emergent in origin. Biological inquiry includes both natural selection and genetics; and genetics discloses, as I think, both emergents and resultants. Is there not pressing need for the exercise of distinguishing analysis?

Fat-soluble Vitamins.

BARELY two decades have elapsed since the concept of 'vitamins' first began seriously to attract the attention of investigators. Scurvy had been recognised as a clinical entity for a couple of centuries, and the treatment of it, by means of fresh vegetables and fruit juices, was well known. But the idea that disease might be caused by the *deficiency* of some factor in the diet was, for many, too novel to be accepted without question, and much work was necessary before the reality of the accessory food factors or vitamins was generally admitted. Recognised at first solely by the effects produced on experimental animals when absent from their carefully purified diets, it was not long before chemical investigations began to define their properties, from which tentative conclusions as to their chemical nature might be drawn. With the discovery that ultra-violet light could cure rickets, and was also capable of making a diet, previously inactive, protective against this disease, a new key was provided for the unlocking of the door which led to the chemical constitution of the anti-rachitic vitamin, or vitamin D, as it is also called. At this stage the work came into contact with other investigations on a group of compounds of widespread distribution in Nature, but of almost unknown biological significance, the sterols. At the present time it is certain that vitamin D, if not actually a member of this group, is closely related to one, and it is extremely probable that vitamin A, or the fat-soluble growth-promoting vitamin, is also of a similar nature.

Following the discovery that exposure to ultra-violet light could render a deficient diet anti-rachitic, it was soon found that the unsaponifiable fraction of the fat of the diet was responsible for this effect. O. Rosenheim and T. A. Webster, working at the National Institute for Medical Research, and Steenbock and Hess and their collaborators in America, then discovered independently that 'chemically pure' cholesterol was rendered anti-rachitic by this exposure. Further work by these and other investigators has now sufficed to determine more definitely the properties and nature of the compound which undergoes this change, although the actual nature of the change itself is undetermined.

Rosenheim and Webster (*Biochem. Jour.*, 1926, vol. 20, p. 537; *Lancet*, 1927, vol. 1, p. 306) were unable to convert more than 0·1 per cent. of cholesterol into vitamin D under the influence of ultra-violet light. They also showed that the presence of the unsaturated linkage and of the secondary alcohol group of the sterol was essential for the reaction to take place, and that the vitamin was not precipitable by digitonin. The fact that only a minute amount of the cholesterol could be 'activated' raised a doubt as to whether this substance was the true precursor of vitamin D, and the doubt became a certainty when it was found that cholesterol purified by way of the dibromide could not be activated and, moreover, no longer possessed the characteristic absorption spectrum in the ultra-violet region. These experiments proved that the vitamin precursor is not cholesterol itself, but some substance which is closely associated with it when obtained from all natural sources.

Further work showed that the precursor was easily oxidised and could also be precipitated by digitonin, unlike the vitamin obtained from it. Attempts to separate it from cholesterol by making use of the latter property, or by fractional crystallisation (Heilbron, Kamm, and Morton, *Jour. Soc. Chem. Ind.*, 1926, vol. 45, p. 932) or by fractional distillation in a high vacuum (Windaus), resulted in a considerable concentration of the precursor, but it was not obtained in a pure state.