

The Meaning of Biological Classification*

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IN much of the academic teaching of biology at the present time, the view is expressed or implied that biological classification is meaningless except as a more or less convenient device for filing herbarium sheets or arranging our animal specimens on shelves, the inference being that our search for a natural system of classification is futile, because no natural system exists. It is possible that if we could cross-examine one of those who despise the work of the systematist, we might find him reluctant to go quite so far as this. We all, even the youngest of us, profess to accept the doctrine of evolution, if only as a convenient weapon with which to meet the fundamentalists, and we can scarcely believe in evolution while denying altogether that, in Darwin's words, "community of descent . . . is the bond which, though obscured by various degrees of modification, is partially revealed to us by our classifications". Darwin, indeed, saw clearly, so early as 1842, that "the natural system ought to be a genealogical one" and the idea was old even then. It is implicit in Buffon, in 1766, and Lamarck seems to take it for granted. Why then must we now abandon it, and what is to take its place?

In the 'eighties of last century, a fresh direction was given to morphological thought by the rise of the new branch of biology to which Roux gave the name of *Entwicklungsmechanik*. This was an attempt to explain structure in terms of forces acting during the life-history of the individual organism, and to replace the 'historical morphology' of the period immediately preceding it by 'causal morphology'.

The profound influence which this school has exerted on biological thought is well known. In the passage which I have just quoted from Darwin I omitted one clause: Darwin wrote, "community of descent, the one known cause of close similarity in organic beings". It is not too much to say that causal morphology has revealed other factors besides community of descent as responsible for some of these close similarities. In other words, it has not only proved the possibility, but also explained a method, of convergent evolution on a scale unsuspected in Darwin's time.

This physiological point of view has tended to concentrate attention on those features of organic form that can be demonstrated experimentally to originate as individual responses to the pressure of environment, or to be the inevitable outcome of 'laws of growth' that are the same for organisms in different lines of descent. It has tended to neglect features for which no such explanation can as yet be imagined.

There is, however, one group of biologists who have never quite lost interest in questions of descent. Those who are concerned with the study of fossil animals and plants cannot, if they would, ignore the historical succession of organisms; nor can they altogether avoid speculating on the nature of the links that relate each form to those that precede and those that follow it in the geological time-scale. Here again we find that modern research has led to the recognition of many incontestable instances of parallel and of convergent evolution. I need not illustrate this by citing examples, for the subject was admirably expounded only a few years ago by one of the most erudite and philosophical of palæontologists, the late Dr. F. A. Bather, in a presidential address to the Geological Society. His conclusions were that "the whole of our system, from the great Phyla to the very unit cells, is riddled through and through with polyphyly and convergence", and, in regard to the main subject we are considering here, "Important though phylogeny is as a subject of study, it is not necessarily the most suitable basis of classification".

When these words were uttered, they sounded to some of his hearers like a counsel of despair; and now that their author no longer abides our question, we are left doubting what other possible basis of classification he could have had to suggest to us.

It would be mere presumption on the part of one who is not a palæontologist to criticise on palæontological grounds the deductions of an acknowledged master in that science; but I must confess to a suspicion that things are not quite so bad as Dr. Bather painted them.

The number of instances in which the succession of fossils has been traced upwards through successive strata foot by foot with marvellous continuity must not blind us to the fact that such

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instances are, in the nature of things, quite exceptional. Darwin's chapter on the "Imperfection of the Geological Record" is, on the whole, as applicable to the state of knowledge to-day as it was when it was written. Many of the phylogenies which have been based upon palæontological evidence are little less conjectural than those based on morphological evidence alone. The main difference (and, of course, a very important one) is that palæontological series can only read in one direction, while we are sometimes in doubt which end of a morphological series comes first.

When these considerations have been allowed for, however, we have to admit that palæontology has revealed instances of convergence that are beyond question, although they are far from proving it to be so all-pervading as Dr. Bather suggested.

It would be easy to multiply instances to show that convergence has been too lightly assumed by some phylogenists as an easy way of getting over difficulties; but, on the other hand, it is impossible to construct a reasonable scheme of phylogeny for any considerable group of organisms without finding it necessary to admit convergence in certain important characters.

When confronted with such examples of convergent, or, as Darwin called it, analogical resemblance, we are tempted to ask where we are to stop. Is structure ever an indication of phylogenetic affinity? Is community of descent ever a cause of organic similarity? Is blood ever, in fact, thicker than water? Dr. Bather, as we have seen, came very near to answering these questions in the negative.

Most morphologists have believed that, however exact the resemblance produced by convergence, investigation would always reveal the underlying evidence of descent. As Darwin put it, "In all such cases some fundamental difference in the growth or development of the parts, and generally in their matured structure, can be detected".

We must, however, beware here of a very ancient type of fallacy.

"Treason doth never prosper. What's the reason?
Why, if it prosper, none dare call it treason."

Convergent evolution, if carried out to the end, would obliterate the evidence of its own existence. Can we believe that it never does so?

I do not profess to have any ready-made and conclusive answer to this question, but it seems to me that it might be helpful to approach it from another side; from the side, namely, of the pure systematist.

Throughout great sections of the animal and vegetable kingdoms the broad outlines of a consistent and symmetrical natural system have now been established beyond all possibility, as it seems, of question. It is true that there still remain very considerable assemblages of both animals and plants of which the classification is still tentative and open to dispute. Since it is upon these debatable groupings that research and discussion have naturally concentrated, we may need sometimes to be reminded how extensive is the territory within which we no longer discuss the natural classification because it has long ago been settled, taken for granted, and put out of mind. Just as, for many of us, fading memories of the Latin grammar consist mainly of lists of exceptions, lingering on after the rules themselves have been forgotten.

It is certain that a natural system does exist. We may never be able to see all the details of its structure, and even its broad outlines may remain, here and there, a little blurred, but the general pattern cannot be mistaken. The framework of the *Systema Naturæ* is something different in kind from the framework of the London Telephone Directory. It is an objective fact, not an arbitrary construction of human inventiveness. It calls for explanation as urgently as, for example, the periodic classification of the elements; and no scientific explanation other than that offered by community of descent has ever been given.

If Dr. Bather and those who think with him were right; if the whole of the system were "riddled through and through with polyphyly and convergence", then it is inconceivable to me that the resulting ruins would have presented anything resembling the coherent pattern that we observe in our approximations to the natural classification of animals and plants. As Darwin pointed out in the fourteenth chapter of the "Origin", the categories of our classification cut across the categories of adaptation and those of environment. To suppose that they can be attributed to inherent laws of growth seems to verge upon mysticism.

I suggest, therefore, that the results of taxonomic research are, in their broad outlines, entirely inconsistent with the view that convergent evolution has been the rule rather than the exception in the phylogeny of animals and plants. It has often played an important part, but never the dominant one.

To know something of the course of evolution is a desirable preliminary to discussing the causes of evolution. If taxonomy can make a contribution towards our knowledge of the course of evolution, then it is a subject worthy of study.