

One would almost suppose that Mr. Bateson was a bio-chemist. But how much chemistry is there in the analysis of Mendelian factors, or the identification of spots in chromosomes which represent particular genes? The suggestions of the nature of the "chemical process" have come from the physiologists and from those who, without ignoring the methods and discoveries of genetics, have not ceased to discuss evolution and adaptation. It is true that some geneticists have discussed the question whether factors for colour might be chemical compounds reacting on each other, but they have not explained how chemical compounds such as enzymes and chromogens could be contained in separate chromosomes and segregate from each other in the reduction divisions of gametes. I do not remember any case in which "modification by descent," that is the loss or gain of a unit character, has been shown by geneticists to be due to any chemical process. The latest results of the American investigators concerning the localisation of genes in the chromosomes, concerning which Mr. Bateson states that all his scepticism has been removed, are purely morphological.

All the progress that has been made in our knowledge of unit characters and of specific characters has tended to exhibit more and more clearly the difference between such characters and adaptational features. It is seldom that an adaptation is confined to a single species, and it is impossible to perceive any connexion between mutations or unit characters and the relation of adaptations to function and external conditions. One great event in the evolution of both animals and plants was the adaptation of the descendants of aquatic forms to terrestrial and atmospheric conditions. In the case of animals, we have, in the metamorphosis of Amphibia and the embryonic development of higher vertebrates the recapitulation of this transition from aquatic organs of respiration to atmospheric organs, not by conversion but by substitution. It is certain from this evidence that the change was perfectly gradual and continuous, and parallel to the gradual change of conditions and mode of life. Recapitulation in this case, however ancient a subject it may be, is an obvious fact, and nothing that the geneticists have discovered throws any light on it, or diminishes its importance. It is no use dismissing it as early Victorian. The question is, have the recent, much vaunted discoveries explained it, or have they anything to do with it? Variations in wings, eye colour, etc., of flies bred in milk-bottles are important in their own sphere, but they throw no light on the annual growth, denudation, death, and recrescence of the antlers of a stag, or on the remarkable relation between these processes and the hormones from the gonads. The origin of species is a very important problem, but it is not the whole, or the most important part, of evolution. The origin of adaptations is not the same problem as the origin of species, and the methods of modern genetics have very little bearing upon it. Mr. Bateson's address suggests that he has not yet realised the difference between the two problems, or paid serious attention to modern physiological knowledge bearing on functional adaptation. The phenomena of recapitulation, so closely associated with adaptation, imply wherever they occur a continuity in the evolutionary change of which the adaptation was the result, and these phenomena are quite incompatible with the discontinuity which is characteristic of non-adaptive variations, and which is the cardinal principle of Mendelians and mutationists.

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Evolutionary Faith and Modern Doubts.

No one can have read without interest Dr. Bateson's admirable address on evolution published in *NATURE* of April 29. While Dr. Bateson's reputation is justly high and his views necessarily command respect, it must be admitted that some of his arguments are very difficult to follow. When, for example, he says that "the conclusion that species are a product of a summation of variations, ignored the chief attribute of species, that the product of their crosses is frequently sterile in greater or less degree," I am frankly puzzled. The proposition is certainly not self-evident. If a sword and its scabbard are bent in different directions, it will happen sooner or later that the sword cannot be inserted, and the result will be the same whether the bending be effected by a single blow, or whether it be, in Dr. Bateson's words, "a product of a summation of variations." Is this illustration inapt? The sword and its scabbard are the homologous chromosomes. These presumably have to co-operate to produce the somatic cell of the hybrid, and their co-operation might be expected to require a certain resemblance, but for the production of sexual cells they must do more, they must conjugate; and for conjugation it is surely reasonable to suppose that a much more intimate resemblance would be needed.

We might, therefore, expect, on purely theoretical grounds, that as species and genera gradually diverged, it would be increasingly difficult to breed a hybrid between them; but that, even while a hybrid could still be produced, a fertile hybrid would be difficult or impossible, since the cells of the germ-track would fail to surmount the meiotic reduction stage, when the homologous chromosomes conjugate. This is exactly what happens: the cells go to pieces in the meiotic phase.

It would even seem that the argument is exactly contrary to Dr. Bateson's statement of it: it seems easier to imagine sterility arising from a gradual modification, spread over a length of time, and involving many chromosomes, than from the half-monstrous variations chiefly studied by Dr. Bateson and his school, variations which appear to affect only a few chromomeres, and those by loss alone.

Now I certainly cannot pretend to much or special knowledge, either in genetics or cytology. But I would ask Dr. Bateson in all humility whether there is any difficulty involved in this simple solution of his problem. Very likely there is, but he does not indicate it.

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Transcription of Russian Names.

IN *NATURE* of May 20, p. 648, is published a letter from Maj.-Gen. Lord Gleichen, who raises objections to Prof. Brauner's suggestion (*NATURE*, April 29) that we should adopt the Czech transcriptions for the names of Russian men of science.

The argument that there are typographical difficulties is surely a very small one, since *NATURE* and other journals (*e.g.* that of the Chemical Society) already employ letters with diacritical marks in writing the names of Czech and other authors. Whilst diacritical marks are undesirable for place names on maps, the same need not apply to the names of persons.

The main points raised by Prof. Brauner in support of his suggestion, remain unchallenged, and in addition to these it may be mentioned that the Czech language is phonetic and Russian names can thus be accurately pronounced according to it.