

THURSDAY, JULY 20, 1882

PERMANENCE AND EVOLUTION

Permanence and Evolution; An Enquiry into the Supposed Mutability of Animal Types. By S. E. B. Bouverie Pusey. (London: Kegan Paul, Trench, and Co., 1882.)

THIS is a thoughtful little book, clearly and ably written, with the view of showing, as its Preface states, "that while Darwinism proper is improbable, evolutionism in any form is as yet unproved; while, on the other hand, the more we investigate the facts of inheritance, the more we are compelled to regard differences so slight, that they would usually be considered casual variations, as within the limits of our existing knowledge strictly permanent." Such being his theme, Mr. Pusey introduces it with the following very appropriate and judicious apology, which we quote in order to show the spirit which throughout characterises his work.

"It may seem almost presumptuous on the author's part to attempt to reopen once more the whole question of evolution, especially as in doing so it is necessary to call in question the views of so many very eminent men of science. At the same time, any one who calls attention to any neglected facts, or who questions assumptions too carelessly allowed to pass muster, helps to elucidate the subject of which he treats, and so aids the cause of scientific knowledge, whether the particular views he propounds are right or wrong."

Having already observed that the work is one of marked ability, we have only further to preface our analysis of it by fully assenting to this justification. Although, as we shall immediately proceed to show, we do not think that Mr. Pusey has been successful in his tilt against the stone wall which has been reared by the school of Darwin, we nevertheless respect his independent disregard of mere authority, as we think that such disregard always deserves to be respected in matters of science where evidence is shown by the malcontent of clear and forcible thought of his own upon the doctrines which he undertakes to criticise.

The principal part of "Permanence and Evolution" is occupied with a criticism of the argument from classification, and especially that part of the argument which has reference to domestic animals. In the author's view Mr. Darwin has failed to prove in the case of any domestic animal that artificial selection has produced a new variety or sub-species. Thus of the varieties of the dog he says, "seeing how true they breed, I do not see why the principal and best marked (the greyhound, the mastiff, the terrier, the spaniel, &c.) should not have so existed (*i.e.* in a state of nature), and the others have been formed by crossing between them." Similarly of the pigeon he says there is no sufficient evidence to show that all the fancy-breeds were not once natural breeds which have since become extinct as such, or that their occasional reversion to the rock dove is not due to an ancient cross with it. "As these races resemble the rock dove, and each other, in everything except one or two conspicuous points, it need not surprise us that they produce perfectly fertile offspring," &c. Thus also he treats of the cases of all the other domestic animals alluded to by Darwin.

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Concerning this mode of criticism, it seems enough to point out the cumulative improbability of all the domestic races of animals having once been wild (notwithstanding the apparent unfitness of some of them to a self-dependent mode of life), added to the further cumulative improbability of all these wild races having become extinct. We do not say that the hypothesis is impossible, but clearly it is so far improbable that even if there were no other evidence of the mutability of animal types, it would be more likely that the domestic races had been produced by artificial selection (and so that animal types are thus far changeable) than that they are all the remnants of more or less fantastic natural forms now as such extinct. If the hypothesis of "Permanence" has to stand upon so improbable a supposition as this, it is so far a less reasonable hypothesis than that of "Evolution," and therefore Darwin is justified in adducing the facts in question as evidence of transmutation to this extent.

But Mr. Pusey carries his criticism further than this, and says:—

"Granting that natural selection with spontaneous variation could within the period of history develop out of a rock dove a fantail, I do not see how we are any nearer the conclusion that in ten times or a hundred times that period these causes would develop the Gourd pigeon; granting that, that a millionfold as much time would evolve any of the true Gallinaceæ."

This way of treating the evidence is, however, hyper-critical. It is certain that either "Permanence" or "Evolution" is the truth, and therefore, if it were established, or taken for granted, that within the historic period selection is able so far to change an animal type as to convert a rock dove into a fantail, the presumption becomes immense that in a hundred times that period the operation of similar causes might develop a Gourd pigeon. Thus, in view of the supposed assumption or proof we certainly are "nearer the conclusion" in question than we should be in the absence of a case analogous in kind though not in degree.

Similarly in dealing with the argument from affinity, we think that Mr. Pusey is hyper-critical. He points to the fact that crystals occur in natural systems, and that their similarities cannot be due to genetic descent; but this analogy is clearly too lame to support any weight of argument, and the same remark applies to his analogies drawn from the similarities found in inorganic nature generally. For in all these cases the similarities occur in objects of far less complexity than organised structures, and therefore the similarities are much less remarkable, while in the case of organisms the known facts of heredity furnish much the most probable explanation of the much more complex similarities. This, perhaps, may most briefly be shown by quoting the alternative hypothesis which our author presents, for clearly it is one which no man of the commonest judgment could for a moment entertain. He suggests that systematic affinity may be due to the resemblance between the chemical elements (? and compounds) of which organisms are made up, and adds what we must regard as a scarcely serious observation—"This hypothesis, though totally without positive evidence to support it, is in itself quite as clear and definite, and (what is called) explains the facts about as well as the hypothesis of evolution."

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Mr. Pusey's treatment of the evidence from rudimentary organs does not seem to us more fortunate. His only criticism here is that because organs are rudimentary we have on this account no warrant for concluding that they are useless; "if these aborted structures were the only ones in which we could see no use, then the explanation would have some *locus standi*." But here the important fact is lost sight of that all rudimentary organs are the *homologues* of organs which when of larger size present observable utility. Can it be reasonably supposed that in the case of all the thousands of these "aborted structures," some new function, always unobservable, is performed by an organ which by some strange chance happens to be the homologue of an organ which when of larger size performs some other and observable function?

Again, the argument from embryology obtains but very scant justice; only three pages are devoted to it, and the core of the subject is not touched. For the force of this argument does not consist in our seeing "a vast number of animal forms, many of which are very like each other, and their distinctions less pronounced in youth," or in such cases as that of the stripes on the young lion, &c. The force of the argument consists in the progressive imitation of lower morphological types by the successive embryonic stages of higher ones; and of this very remarkable fact Mr. Pusey takes no notice.

The argument from palæontology is dismissed in a similarly high-handed fashion, but somewhat more consideration is given to the argument from geographical distribution. The view advanced is "that the facts of distribution can, to a great extent, be shown to have originated in an opposite manner, not by the origination of new forms, but by the destruction of old ones." If this could be shown, no doubt the proof would be one of much importance to science, and would serve largely to modify the argument from distribution; but the fact certainly has not been proved, or even shown to be generally probable, by the book before us.

Concerning the specially Darwinian theory of evolution Mr. Pusey says that personally he thinks "whatever else is the origin of species, natural selection certainly is not." His reasons for this opinion are that *a priori* the way in which we should expect natural selection to act "would be by conferring fertility, hardiness, and early maturity" (none of which qualities are presented by the higher Primates); and also that allied animals living on the same areas and apparently exposed to similar conditions of life, are nevertheless "dissimilar in a number of minor points, apparently unconnected and without teleological purpose." Now concerning the first of these objections, it seems enough to observe that *a priori* considerations of this kind are extremely hazardous. Fertility, hardiness, and early maturity may all be good for species, and yet other qualities (perhaps incompatible with them) may be even better, such as high nervous organisation, intelligence, &c. In short, where the conditions of the problem in any given case are so many and complex, it would be idle to determine beforehand what qualities we should expect natural selection to lay a premium upon—as much so, for instance, as to say, after the event, that a man would be better suited in his environment if he had had a very much more brutal constitution, could run about like a chicken when a few hours old, and was the most prolific

animal in creation. And of course the other difficulty, being of a similarly *a priori* kind, admits of being similarly met. There may be a thousand unobservable reasons why, after a long course of evolution, allied species living on the same areas should be dissimilar in minor points of structure, colour, &c.

We have now briefly noticed all the leading points in Mr. Pusey's criticism, and if we had more space we might go more in detail with him. But we have said enough to show that we deem his strictures throughout to err on the side of over-scepticism. In science, as in everyday life, true judgment is shown, not by suspending our decision until a theory is demonstrated by observation, but by yielding assent to probability in a degree commensurate with the evidence. At the same time, it is, of course, most important that a clear distinction should always be drawn between a probability, however high, and a proved fact. In every department of inquiry, therefore, the hyper-critical mind is of service in insisting upon this distinction when there is danger of its being neglected; and in view of this consideration we think there are many evolutionists who would do well to read Mr. Pusey's work. As we have already said, we do not consider that this work has in any way affected the main evidences of evolution; but it is well calculated to steady the course of speculative thought in a direction where with less hurry there may be more speed.

GEORGE J. ROMANES

CRYSTALLOGRAPHY

Geometrische Krystallographie. Von Dr. Th. Liebisch (Leipzig: Wm. Engelmann, 1881.)

THIS is the most complete and exhaustive book on crystallography which has been so far published, and it is especially characterised by the importance assigned to the dualism observed in crystallographic problems considered as relations of a system of planes or lines connected together by the law of rational indices. The book consists of three main parts—the first dealing with the general relations of a system of planes and lines subject to the law of rational indices; the second with crystallographic representation and construction; and the third with the developments of the six crystallographic systems and the determination of crystals.

The general problems in the first part are treated by the processes of modern geometry. The problem of the transformation of the axial system is very exhaustively treated, but curiously enough Dr. Liebisch seems unacquainted with the elegant solution of this problem, given by the late Prof. Miller in his Tract on Crystallography (1863). The analysis of this problem, given by Dr. Liebisch, is laborious and somewhat complicated, and the results are not really more general than those of Prof. Miller. Dr. Liebisch has entered into the question of the conditions of perpendicularity in a crystal system, but his analysis is not so elegant as that of Prof. H. J. S. Smith, nor is it capable of more ready application than the latter. The chapter treating of this portion of the subject is largely occupied by the proofs of the ordinary propositions of spherical trigonometry by means of a cumbrous notation and an analysis of great difficulty. One can hardly believe that this analysis is needed by