

them all in a single individual. Here the "selection" is *intentional*; and therefore the whole ground on which the "difficulty" stands is absent. This ground is the supposition of *fortuity*, with regard (a) to all the variations A, B, C, D, &c., happening to occur in any one individual to begin with, or (b) being afterwards preserved (by suitable mating) from obliteration by free intercrossing. Therefore, thus to appeal explicitly from natural selection to the analogy of artificial selection is to be cheated by a metaphor.

How, then, does it fare if the appeal be made implicitly, as in Prof. Meldola's review, by supplying *utility* in the one case as corresponding to *intelligence* in the other? Obviously, here again, the element of *fortuity* is ignored, and therefore, as previously, the "difficulty" is not met, but evaded. For no one who believes in natural selection could deny, that if *each* of the variations, A, B, C, D, &c., is of advantage *per se*, they would all be preserved as they severally happened to arise in this, that, and the other individual, till, by general intercrossing, they would eventually coalesce in single individuals—as in the case of artificial selection. But all this is quite wide of the mark. Indeed, intercrossing is here a necessary condition to, instead of a fatal impediment against, the blending of co-operative modifications; and therefore Mr. Spencer would have been a fool had he brought his "difficulty" to bear upon this case. This case, however, is not that which is meant by "co-adaptation": it is the case of a confluence of *adaptations*. Or, otherwise stated, it is not the case where adaptation is *first initiated in spite of intercrossing*, by means of a fortuitous concurrence of variations, each in itself being without any adaptive value; it is the case where adaptation is *afterwards increased by means of intercrossing*, on account of the blending of variations each of which has always been of adaptive value in itself.

The "difficulty," therefore, remains just where it was before; and the only way of meeting it is to show that the phenomenon of co-adaptation does not occur in nature. In other words, it must be shown that the difficulty is fictitious, by showing that, as a matter of fact, there are no cases to be found where *n* modifications, each being useless in itself, become useful in association. Whether or not the difficulty does admit of this the only rational solution, I will not occupy space by discussing; but I have thought it desirable to state what I have always understood to be the real nature of Mr. Spencer's "well-known objection."

Oxford, March 10.

GEORGE J. ROMANES.

### Neo-Lamarckism and Darwinism.

It has been sometimes said that it is difficult to tell the difference between the supposed effects of the environment upon an organism, and the accumulation of favourable variations. There can be no difference; for they are but two explanations or theories to account for the same thing. A species is characterized by certain features; it is *these* which have to be accounted for; and any number of theories may be propounded as to the cause. It is simply a question as to which can be "proved" to be either the most probable or actually true.

At one time it was thought satisfactory to account for everything by a direct creative act. A man is exactly the same, whether he was created as he is, or evolved from animals; and if evolved, whether by the direct action of the environment or by natural selection or any other way. We may say with Burns, "A man's a man for a' that."

It is also said that the value of a theory depends upon the number of phenomena it can satisfactorily explain or account for. This is not altogether the case. The theory of creation accounted for everything; but we have abandoned it, nevertheless. The value of a theory really depends, not so much on what it can explain, as upon *the number of facts* on which it is based.

Now, are not many theorists forgetting the importance of this? I have just read Mr. Cockerell's paper on the "Alpine Flora" (NATURE, January 1), which will illustrate my contention. He has studied the flora of the mountains of Colorado, and finds that, as a whole, the plants are characterized by certain features. These are the same as are noticeable, not only on European and the Rocky Mountains, but in Arctic and Antarctic regions as well. He comes to the conclusion that "If this [lack of nourishment] were the only cause of dwarfing, the Alpine flora would present clear evidence for the transmission of acquired characters, as the character has undoubtedly become a *specific* one in several mountain plants." He here alludes to

one, viz. a dwarf habit. The cause, however, which he gives is not the only one, nor is it in this case probably always the right one. If it were, then all mountain and all Arctic and Antarctic regions must have poor soils, for which there is no evidence. All these regions, however, have a relatively lower temperature.

Here, then, we have *two coincidences of universal application*—a dwarf habit and a low isotherm. Now we all know from experience how suddenly cold weather instantly checks growth in spring, &c.; therefore, we can infer, or draw the deduction, that the constantly low temperature of the Alps and Lapland perpetually check growth in those regions.

This alone would be a perfectly legitimate conclusion; as the probabilities of there being a distinct cause and effect underlying these coincidences are so great as to amount to a "moral conviction" of the truth.

Though this is logically sufficient, the deduction has been "verified by experiments." When seed is gathered from Alpine plants and sown at low altitudes,<sup>1</sup> and *vice versa*, the plants raised after a few years begin to assume the characters, respectively, of the same species which are natives of the places.

Now the argument is complete.

The preceding facts, therefore, warrant one in stating the theory thus: "That Alpine plants have acquired their special characteristics, by the responsive power of their protoplasm under the influence of their environment."

Having lived generation after generation under that same influence their characters have become relatively fixed, hereditary and "specific" as Mr. Cockerell believes. Such plants, however, probably never lose the power of changing again, as experiment shows.

To this scientific explanation Mr. Cockerell superadds the theory of natural selection. He endeavours to explain how natural selection "may" come into play as well. He says:—

"(1) They may escape the violence of high winds which prevail at those altitudes; taller plants being broken off before the seed matures."

Instead of appealing to facts, as he did before, he now begins with an hypothesis. Has he ever seen a taller plant broken off (as often occurs at lower altitudes)?

*A mere suggestion is scientifically of little value, unless it be founded upon something which actually occurs.*

"(2) They may obtain some additional warmth from their close proximity to the ground and partial shelter."

Here is a remark which ought to have been tested experimentally before being given to the world. Why should not a close proximity to the ground give a chill as well as, or instead of, warmth? As a fact, radiation at night begins on the ground, as the presence of hoar-frost tells us; and therefore we might ask, Are not dwarf plants just as likely (*a priori*), if not more so than tall ones, to suffer as well as to be benefited?

To what *facts* does "partial shelter" refer? Alpine plants are particularly exposed.

"(3) The short summer of the mountain tops necessitates very rapid development; and requires every energy to be thrown into the essential function of producing flowers and seed, leaving nothing to spare for the production of branched stems and diffuse foliage."

This seems like putting the cart before the horse; for how can a seedling plant know that the summer is going to be very short, and that it must, therefore, put forth all its energies? If it understood its own functions, it would know that the flowering depends entirely on the foliage; and, since M. Bonnier, for example, has shown<sup>2</sup> that the chlorophyllous tissue is increased in Alpine plants, this justifies us in looking to it as probably a sufficient cause of Alpine plants having fine flowers.

Finally, has Mr. Cockerell observed any plants which have "failed in the race, and so have been ruthlessly cut off by the autumn storms?" If so, will he give examples? If not, I would refer him to the paragraph italicised above.

To refer once more to the difficulty mentioned above. It must not be forgotten by those who feel that difficulty, that, while the action of the environment on plants is a thing which can be tested, and in many cases admits of easy proof by experiment, the accumulation of many useful variations which mark any living species must ever remain an *a priori* assumption, which is absolutely incapable of verification.

Cairo, February.

GEORGE HENSLAW.

<sup>1</sup> As by M. Bonnier, see ref. *infra*.

<sup>2</sup> *Bull. Soc. Bot. de Fr.*, 1886, p. 467.