

Thus we may say, in very young *Ammocoetes* the parietal eye possesses black pigment, in older *Ammocoetes* white pigment, and in adult *Petromyzon* there is a reversion to black pigment. In what relationship these three pigments stand to each other I am unable to say.

The last point concerns the hypotheses as to the origin of the eye. These were really two in number. The first of them—that which derives the paired eyes and the parietal eye from one common dorsal sense-plate—I hold to be fairly certain, and, indeed, there are many facts to support it.

The second, which derives the parietal eye as a later involution of a portion of this same plate, an involution which was supposed to have taken place after that of the paired eye "Anlage," I only believe to be conceivable. My hope of establishing it lay in the verification of an observation of Goette's; there are no facts to support it, and from more recent investigations of the development I am disposed to attach less value to it. For, from these developmental researches, from studies of the types of eye presented by vertebrates and some invertebrates, and lastly, but not least, from valuable discussion with and criticism by Prof. Wiedersheim, a new track has been found, which gives the explanation of a good deal, but the problem is too long and complicated for treatment here.

The first hypothesis mentioned above is taken as the starting-point, but for the further details there are several other questions which have first to be solved.

J. BEARD.

Anatomisches Institut, Freiburg i/Br., July 20.

#### Physiological Selection.

LIKE so many others who have written on this subject, Mr. Rusden freely criticises my views without having deemed it desirable to read my paper. Had he taken the trouble to do so, he would have found a sufficient recognition of the general fact that instinctive habits not unfrequently serve to mitigate the swamping effects on incipient varieties of intercrossing with their parent forms. Moreover, he would have found that there are others of these habits mentioned by me which are probably much more effectual in this respect than is the one to which he draws attention. Nevertheless, it appears to me evident that all these habits taken together cannot count for much, even where they occur; while it is unquestionable that they occur only in a very small fractional part of organic nature considered as a whole—namely, in some among the more intelligent species of animals. The whole of the vegetable kingdom, an immense majority of the Invertebrata, and a considerable majority of the Vertebrata, cannot possibly have had any of their specific differentiations influenced by any of these forms of what I have already designated as "psychological selection." This sufficiently obvious consideration appears to have entirely escaped Mr. Rusden. He adduces a well-known and a comparatively limited form of psychological selection as a "simple solution" of the difficulty from free intercrossing in all cases!

The other parts of his letter merely indorse the views which are published in my paper. I there say that the theory of natural selection is not, strictly speaking, a theory of the origin of species, but a theory of the development of adaptations. Having read this statement, your correspondent writes:—"To consider the theory of natural selection as a theory of the origin of species is, therefore, clearly an error. . . . The theory of natural selection is one, not of the origin of species at all, but of the preservation of particular varieties," *i.e.* those which present an adaptive character. I do not see how his agreement with my views in this matter could be more clearly expressed, and therefore I cannot understand why he supposes that he is here criticising anything which I have written. If the point of his criticism is that I imagine Mr. Darwin to have fallen into the error of regarding the theory of natural selection as (primarily) a theory of the origin of species, this would merely show again that he has not read my paper. My contention from the first has been that upon this point I am in full agreement with Mr. Darwin, and differ only from those Darwinians who differ from their master in holding that *all* specific changes are likewise adaptive changes, and *vice versa*. It is only in the presence of this non-Darwinian assumption that specific changes and adaptive changes become synonymous terms, with the consequence that the theory of natural selection is to be regarded as in all cases the only theory of the origin of species.

And this leads me to the last point in my critic's letter. I

have argued that the above-mentioned non-Darwinian assumption is opposed to observable fact, seeing that "in a large proportional number of cases" specific characters appear to be wholly useless. Nothing has surprised me so much on the part of my critics as to have found this statement vehemently challenged by so accomplished a naturalist as Mr. Wallace, and therefore I am now engaged in collecting a quantity of evidence upon the subject. But the point here is that Mr. Rusden appears to think there is some ambiguity attaching to the terms "use" and "utility." For he asks whether these words have "any real significance outside human interests and considerations." Now, I can scarcely understand how anyone at this time of day could suppose that when these words are employed in their Darwinian sense they are intended to have any reference to human interests. When an evolutionist speaks of the utility of an organ, it is hardly conceivable that anyone should understand him to mean anything else than the utility of that organ to the species which presents it. Therefore, the term "utility" is equivalent to the term "adaptation," and to say that any organ or structure is of use is one and the same thing as to say that it is adapted to the performance of a function which is of benefit to the organism or to its species. Such, at any rate, is the only sense in which I have myself employed these words; and in doing so I have, of course, followed the terminology of Mr. Darwin, as my critic might have observed without going beyond one of the quotations which he himself makes from the "Origin of Species"—namely, "I have called this principle by which each slight variation, if useful, is preserved by the term 'natural selection.'"

GEORGE J. ROMANES.

Geanies, Ross-shire, N.B., July 29.

#### The Droseras.

MISS ANNE PRATT in her "Wild Flowers," vol. ii. p. 155, in describing the three British species, after stating the character of the stems and flowers, remarks, "but many persons who know the plant well have never seen the flowers fully open." Two of the species, *D. rotundifolia* and *D. longifolia*, are found in a bog on a common near here, and these have lately flowered in captivity. They were transferred from their habitat and placed in a large saucer with peat and Sphagnum, under a bell glass. The flowers have expanded from 10 a.m. to noon each day, after which the sun left them. *A. D. longifolia* in another position was seen to flower at 2 p.m. Moisture and sun seem the conditions to bring out the blossoms. I am not aware whether they have flowered *in situ*, as my plants were gathered in the early morning.

*Ramondia pyrenaica*, brought from Bagnères de Luchon ten years ago, has flowered each year on an outside rockery in my garden.

J. RAND CAPRON.

Guildown, Guildford, July 28.

#### Comrades.

MY children and their governess, when staying in the north of Ireland lately, witnessed the following curious display of feeling, in animals not usually credited with feelings. A boar pig was in the habit every morning of going to the basket where a blind kitten of about six weeks old was kept, allowing the little thing to creep on to his back, and then taking it about and caring for it during the day. The kitten got its food at the same time as the pig, and at the same trough. In the evening the man who saw to the animals used to carry the kitten back to its basket to pass the night. "Où donc la vertu va-t-elle se nicher?"

Pollokshields, Glasgow, August 1.

E. R.

#### A NEW COSMOGONY.<sup>1</sup>

##### II.

DR. BRAUN has earned by his excellent series of observations on sunspots (NATURE, vol. xxxv. p. 227) a title to be heard with particular respect on subjects connected with solar physics. In unfolding his views

<sup>1</sup> Ueber Cosmogonie vom Standpunkt christlicher Wissenschaft. Mit einer Theorie der Sonne." Von Carl Braun, S.J. (Münster: Aschendorff, 1887.) Continued from p. 323.