On the other hand, if natural selection cannot thus operate, all we have to say is that there still remain many other causes adequate to explain the occurrence of physiological selection—to wit, those causes which are concerned in the occurrence of variation in general.

The essay by Prof. Weismann on the influence of isolation, to which Mr. Meldola refers, is so replete with facts and arguments unconsciously bearing on my theory, that in writing my preliminary paper it appeared advisable to reserve so rich a mine for subsequent working out in detail. In my paper, therefore, I have merely alluded to Prof. Weismann as one among the comparatively few evolutionists who have hitherto sufficiently considered the influence of independent variation (or the prevention of intercrossing) in the evolution of species.

It only remains to consider Mr. Meldola's extremely able criticism of my view that natural selection ought not in strictness to be regarded as a theory of the origin of species, but rather as a theory of the development of adaptive modifications. My argument is that natural selection can only be a theory of the origin of species in so far as species differ from one another in points of utilitarian significance; and that even then it is only a theory of the origin of species, as it were, incidentally: the *raison d'être* of natural selection is in all cases that of evolving adaptations (whether these be characteristic of species only, or likewise of higher taxonomic divisions); and if in some cases the result of performing this function is that of raising a variety into a species, such a result is merely collateral, or, in a sense, accidental. No doubt if species always and only differed from one another in points of utilitarian character, the collateral nature of the result might be disregarded, and the theory would become a theory of the origin of species in virtue of its being a theory of the development of adaptations. But, as a matter of fact, species are very far from being always and only distinguished from one another in points of utilitarian character, and in so far as they are not thus distinguished natural selection is obviously in no sense a theory of the origin of species. Again, and more particularly, the one feature which more than any other serves to distinguish species from species is that of mutual sterility, and it would be a bold flight of speculation to affirm that this has been in all cases the result of natural selection, when even Mr. Darwin was reluctantly compelled to conclude that such could not be the result of natural selection in any case. On the other hand, my theory of physiological selection explains this very general feature of specific distinction quite independently of natural selection; and then goes on to show that, when once this primary distinction has arisen, many others of a secondary kind will ensue, both with and without the assistance of natural selection.

Now, the objection which Mr. Meldola adduces against this argument is that I have not proved physiological selection to be independent of natural selection. In other words, he does not dispute the probable truth of my theory; but he says that, granting its truth, it is still only "one particular phase of natural selection." But surely the burden of proof here lies on the side of my critic. If he can show any sufficient reason for going much further than I have ventured to go in out-Darwinising Darwin—or for holding that natural selection may not merely help in inducing sterility in some cases, but has been the sole cause of it in all cases—then I should welcome his proof as showing that the principles of physiological selection ultimately and in all cases rest on those of natural selection. But, clearly, it is for him to prove his positive: not for me to prove what I regard as an almost preposterous negative.

So much for the main criticism. But he adds this rider, namely, that, as the struggle for existence is always most severe between the most closely related forms, unless the new or sexually protected form arising under physiological selection possesses some distinct advantage over the old or parent form, it will be exterminated by the latter quite as effectually as it would be by intercrossing in the absence of physiological selection. To this I may answer in the words of my full paper:—"So long as there is no actual *detriment* arising to the variety on account of its being sexually separated from the parent, any ideas derived from the theory of natural selection are plainly without bearing upon the subject" (p. 406). In other words, so long as in all other respects of organisation the sexually separated variation is not less "fit" than its parent stock, so long there is no reason to anticipate any disadvantage in the struggle for existence. And forasmuch as the sexual separation arises only by way of a variation locally affecting the reproductive system, when the variation is first sexually separated, it will in

all other respects resemble its parent stock, and so be able to compete with it on equal terms—mere numerical inferiority being without significance where intercrossing is prevented. It was in order to convey this meaning that I proposed as an alternative name of my theory, "Segregation of the Fit"; seeing that before any physiological segregation can take place there must be organisms to be segregated, and that unless these organisms had already proved themselves fitted to survive in the struggle for existence, in existence they could not be. But I do not call physiological selection "Segregation of the Fittest," because, unlike natural selection, it is in no way concerned with the principle of conflict. So long as the organisms which have been separated by physiological selection are sufficiently fit to have previously passed muster at the hands of natural selection, there is no reason why the daughter type should be fitter than the parent.

But, so far as I can see, the only material point of difference between Mr. Meldola and myself consists in his regarding physiological selection as "subordinate" to natural selection, while I consider the two as quite independent principles, although, as explained in my paper, I believe that they frequently and in several ways play into each other's hands.

GEORGE J. ROMANES

Geanies, Ross-shire, N.B., August 30

Earth-Currents and Aurora

THERE appears to have been a very remarkable and widespread earth-current storm on March 30 last, full particulars of which it would be extremely useful to have on record. My attention has been drawn to this storm through witnessing, on the evening of that day, one of the most vivid and interesting displays of the aurora that I have ever seen. Mr. G. H. Kinahan, writing in *NATURE* for April 8 (p. 537), describes the same aurora as observed by him in Donegal between 8 and 9 p.m., and notes its peculiar bright silvery type. It must, however, have been a far less imposing display at Donegal, where the weather was less favourable, than at Kingstown, where I saw it between 9 and 10 p.m., the most brilliant display occurring between 9.30 and 9.45 p.m. From the northerly horizon there rolled up to the zenith in quick succession streaks and masses of white light, until the whole face of the sky to the north and west was illuminated with swiftly mounting flames of silvery whiteness and wonderful beauty. A correspondent in *NATURE* for April 15 (p. 559) describes the same aurora witnessed by him between 8 and 11 p.m. on March 30, at Königsberg, in Prussia.

According to the *Electrician* (April 2, p. 404), on the morning of March 30 a violent earth-current storm occurred in London, stopping all telegraphic work for some time. During the same day strong earth-currents are reported on the Mediterranean cables, and in the afternoon of that day (March 30) this terrestrial electric storm had reached India, lasting from 2 to 5 p.m., and stopping work on the Bombay and Madras line. At the same time powerful earth-currents are reported on the Madras and Penang cable, causing work over it to be stopped from 3 till 7 p.m. on March 30. Similar disturbances are reported on the Java cable, beginning the same afternoon, and becoming fainter on all lines at 10 p.m. Recently an account has been published of a great earth-current storm on the China and Japan cable on March 30, and a diagram of the perturbations produced on that line is given in the *Electrician* for August 6. The storm began between 4 and 5 p.m., Shanghai time, on March 30, and lasted till 11 p.m. that day, the maximum strength of the earth-current on the cable occurring between 6 and 7 p.m. being then equal to 3.6 milliamperes. Smaller renewals of the storm took place both in Europe and Asia on the morning of March 31.

Perhaps some of the readers of *NATURE* can give further details of this extensive storm, and it would also be useful to have on record the magnetic perturbations noticed on this occasion in different localities, and the time of their occurrence.

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Chlamydomyxa in the Engadine

YOUR readers will be interested to hear that I have found here in Pontresina the very interesting Protozoon described twelve years ago, in the *Quarterly Journal of Microscopical Science*, by