

criticizing Weismann upon what he calls "reversal of selection," which he now tells us is the same principle as "economy of growth." Yet in the earlier letter he entirely omits to credit Mr. Darwin with the recognition of that principle, and after carefully asserting that Mr. Darwin had overlooked the principle of "panmixia," he gives in an historical form what *he* (Mr. Romanes) had argued some years ago, and what *his* views were—including herein the principle of economy of growth, or more generally, reversed selection. Now that the oversight has been pointed out to him Mr. Romanes allows that "it is a matter of familiar knowledge that Mr. Darwin at all times, and through all his works, laid considerable stress upon the economy of growth (or more generally, reversed selection)."

Mr. Romanes makes an unreal separation between "cessation of selection" and "reversal of selection"; at the same time, for the mere purpose of *badinage*, he affects to suppose that I do not perceive any difference between them—a supposition which cannot be sincere in view of the statements in my letter of March 27. Cessation of selection is not a "principle" at all. It is a condition which alone cannot produce any important result. At the same time, what Mr. Romanes misleadingly calls "reversal of selection," viz. "economy of growth," cannot become operative in causing the dwindling of an organ until the condition of "cessation of selection" exists. The fact is—as Mr. Romanes insisted before it was pointed out in these pages that it was no new principle of his own discovery, and when he wished to lay claim to an improvement upon Weismann's exposition of "panmixia"—cessation of selection must be supplemented by economy of growth in order to produce the results attributed to "panmixia." And inasmuch as economy of growth as a cause of degeneration involves the condition of cessation of selection, Mr. Darwin, in recognizing the one recognized the other.

By the use of the term "the principle of the cessation of selection" Mr. Romanes has created an unnecessary obscurity. To say that a part has become "useless," or "has ceased to be useful to its possessor" as Mr. Darwin does, is clearly the same thing as to say that it "has ceased to be selected"—selection and use being inseparable. Mr. Darwin states that such parts "may well be variable, for their variations can no longer be checked by natural selection." That is panmixia. It is true that Mr. Darwin did not recognize that such unrestricted variation must lead to a diminution in size of the varying part without the operation of the principle of "economy of growth." This was no strange oversight: he would have been in error had he done so. On the other hand, he did recognize that, given the operation of that principle, the result would amount to the dwindling and degeneration of parts which are referred to as rudimentary.

"Panmixia" as a term clearly refers to the unrestricted interbreeding of all varieties which may arise, when selection in regard to a given part or organ is no longer operative. The term, like its correlative "cessation of selection," does not indicate a principle but a natural condition: it does not involve the inference that a dwindling in the *size* of the organ must result from the inter-breeding; but simply points to a precedent condition.

I am by no means prepared to admit that panmixia alone (*i.e.* without economy of growth or other such factors) can be relied upon, as it is by Mr. Romanes, to explain the reduction in size of the disused organs of domesticated animals. I observe that in his letter on this subject to NATURE of April 9, 1874, Mr. Romanes does not attempt to attribute a dwindling action to "panmixia" alone, but assumes a limitation by economy of growth to any increase beyond the initial size of the organ which has become useless. Given this limitation and the condition of panmixia, the dwindling follows; but it is absurd to attribute the result, or any proportion of it, to the panmixia or cessation of selection alone. On the other hand, when we consider shape and structure, and not merely size, it is clear that panmixia without economy of growth would lead to a complete loss of that complex adjustment of parts which many organs exhibit, and consequently to degeneration without loss of bulk. That the principle of economy of growth is ever totally inoperative has not been demonstrated.

E. RAY LANKESTER.

April 9.

Heredity, and the Effects of Use and Disuse.

ALL biologists will, I am sure, agree as to the desirability of a thorough testing of the hypotheses relative to the inheritance of

the effects of use and disuse. As Mr. Spencer says, in the preface to "The Factors of Organic Evolution":—"considering the width and depth of the effects which acceptance of one or other of these hypotheses must have on our views of Life, Mind, Morals, and Politics, the question—Which of them is true? demands, beyond all other questions whatever, the attention of scientific men."

As experiments suggested by those who believe in the inheritance of the effects of use and disuse would hardly carry the weight to those who do not believe in this inheritance which experiments proposed by themselves would, I write to suggest the desirability of undertaking an investigation which, Prof. Weismann thinks, would prove one or other hypothesis. He states it in the following words on p. 91 of the English edition of his "Essays":—

"If it is desired to prove that use and disuse produce hereditary effects without the assistance of natural selection, it will be necessary to domesticate wild animals (for example the wild duck) and preserve all their descendants, thus excluding the operation of natural selection. If then all individuals of the second, third, fourth, and later generations of these tame ducks possess identical variations, which increase from generation to generation, and if the nature of these changes proves that they must have been due to the effects of use and disuse, then perhaps the transmission of such effects may be admitted; but it must always be remembered that domestication itself influences the organism,—not only directly, but also indirectly, by the increase of variability as a result of the suspension of natural selection. Such experiments have not yet been carried out in sufficient detail."

If Profs. Weismann, Romanes, and Lankester, would agree to some such experiment as the above as definitely proving the point in question (I say "definitely," for the sentence which reads "if the nature of these changes proves that they must have been due to the effects of use and disuse," seems rather to beg the whole question, even if the experiment were carefully carried out), there are two ways in which it might be effected. One is, that the British Association, which by devoting time to the discussion of the hypothesis has shown an appreciation of its worth, should at its next meeting appoint a committee, with a small grant for necessary expenses, to carry out the investigation. The other is, that it should be undertaken independently by the foremost of those on both sides who are interested in this question, and who would no doubt subscribe among themselves enough for the purpose in view—at least, speaking for myself, I should not object to contribute to the expenses of a properly planned investigation.

Regarding the place where the "wild ducks," or possibly some animal with a more frequent recurrence of broods, should be located for observation, I would suggest that the Zoological Society should be asked to afford space in their Gardens at Regent's Park.

F. HOWARD COLLINS.

Churchfield, Edgbaston.

Galls.

THE difficulty raised by Mr. Wetterhan (NATURE, February 27, p. 394) appears at first sight a serious one, but I think it vanishes on examination. Supposing the attacks of the insects to be constant, trees in their evolution would have to adapt themselves to these circumstances, just as they have adapted themselves to the environment of soil, air, light, wind, and so forth. But the fallacy (as it seems to me) of Mr. Wetterhan's argument lies in the supposition that the life of an oak-tree as such, and the life of an insect, may rightly be compared. A tree is really a sort of socialistic community of plants, which continually die and are supplanted by fresh. Bud-variation is a well-known thing, and in oaks A. de Candolle found many variations on the same tree. Now is it unreasonable to suppose that internal-feeding insects might take advantage of such variation—or rather, be obliged to take advantage of it, if it were in a direction to benefit the tree? I will give two purely hypothetical instances, to illustrate the points involved. Imagine two oak-trees, each with three branches, and each attacked by three internal-feeding insects. The insects infesting one tree are borers; those on the other tree are gall-makers. The borers bore into the branches, which they kill while undergoing their transformations; the tree possibly does not die that year, but next year the progeny of the three, being more numerous while the tree is weaker, effect its destruction, and finally the insects perish for want of food. On the other tree, the gall-makers do no appreciable damage, and the tree is

able to support them and their progeny without great difficulty. Now a little consideration will show that the longer the life and the slower the reproduction of the trees, the greater will be the contrast. If the plant infested by the borers had been an annual herb, it might have contrived to perfect its seeds, and the death of the old stem would be but a natural and inevitable process, and fresh plants might have been produced in sufficient numbers to continue the species in spite of all insect-attacks. But in the case of trees—oak-trees especially, the rate of growth and reproduction is such that, unless the insect-borers can live in galls, they will destroy the plants entirely, and themselves in consequence. Indeed, I have no doubt, that if all the gall-makers now existing could suddenly be transformed into stem-borers, the genera *Quercus*, *Rosa*, and *Salix*, now so dominant, would shortly disappear from off the face of the earth. The other hypothesis—here assuming that the production of galls is due more to the tree than the insect—is this. Suppose an oak-tree with four branches, all attacked by internal-feeding insects. Two of the branches produce swellings in which the insects live, while the other two produce none, and the insects have to devour the vital parts. Now the two branches which produced no swellings would quickly be killed by the insects, but those which produced galls would live, and the more perfect the galls, the greater the insect-population they would be able to support. Hence the tree would finally, by the survival of its gall-producing branches, become purely gall-producing, and we may assume that its progeny would inherit the peculiarity.

I am aware that the above arguments will sound a little like those of the Irishman, who said he ought not to be hanged, because, "in the first place, he did not kill the man; in the second place, he killed him by accident; and thirdly, he killed him in self-defence,"—but I do not represent either of the above hypotheses as the precise truth of the matter, and I think they sufficiently illustrate the principles involved.

T. D. A. COCKERELL.

West Cliff, Custer Co., Colorado, March 16.

On the Use of the Edison Phonograph in the Preservation of the Languages of the American Indians.

THE present state of perfection of the Edison phonograph led me to attempt some experiments with it on our New England Indians, as a means of preserving languages which are rapidly becoming extinct. I accordingly made a visit to Calais, Maine, and was able, through the kindness of Mrs. W. Wallace Brown, to take upon the phonograph a collection of records illustrating the language, folk-lore, songs, and counting-out rhymes of the Passamaquoddy Indians. My experiments met with complete success, and I was able not only to take the records, but also to take them so well that the Indians themselves recognized the voices of other members of the tribe who had spoken the day before.

One of the most interesting records which was made was the song of the snake dance, sung by Noel Josephs, who is recognized by the Passamaquoddies as the best acquainted of all with this song "of old time." He is always the leader in the dance, and sang it in the same way as at its last celebration.

I also took upon the same wax cylinder on which the impressions are made his account of the dance, including the invitation which precedes the ceremony.

In addition to the song of the snake dance I obtained on the phonograph an interesting "trade song" and a "Mohawk war song" which is very old. Several other songs were recorded. Many very interesting old folk-tales were also taken. In some of these there occur ancient songs with archaic words, imitation of the voices of animals, old and young. An ordinary conversation between two Indians, and a counting-out rhyme, are among the records made.

I found the schedules of the United States Bureau of Ethnology of great value in my work, and adopted the method of giving Passamaquoddy and English words consecutively on the cylinders.

The records were all numbered, and the announcement of the subject made on each in English. Some of the stories filled several cylinders, but there was little difficulty in making the changes necessary to pass from one to the other, and the Indians, after some practice, were able to "make good records" in the instrument. Thirty-six cylinders were taken in all. One apiece is sufficient for most of the songs and for many of the short stories. The longest story taken was a folk-tale, which occupies

nine cylinders, about "Podump" and "Pook-jin-Squiss," the "Black Cat and the Toad Woman," which has never been published. In a detailed report of my work with the phonograph in preserving the Passamaquoddy language, I hope to give a translation of this interesting story.

Boston, U. S. A., March 20.

J. WALTER FEWKES.

Solar Halos and Parhelia.

A MAGNIFICENT display of solar halos and parhelia was witnessed here this afternoon, exceeding in beauty and brilliancy that observed on January 29, 1890, and described in NATURE, February 6, p. 330.

The phenomenon was similar to the one of January 29, except that the mock suns were distinctly outside the first circle or halo, at a distance of 5° or 6°, and were when first seen at 3 p.m. *above* the level of the true sun; a handkerchief stretched at arm's length from one to the other gave the blurred image of the sun several degrees lower.

At 3.49 the patch of white light appeared about 90° from the right mock sun and connected to it with a *curved* band of white light, concave side upwards. The right mock sun must then have been *below* the level of the sun, as the band appeared to pass upwards through it to the sun. This band only remained a few minutes; the right sun and zenith arc at the time were most intensely brilliant, with the colours exceptionally clear and vivid. The zenith arc, and the patch of white light, were the last to disappear at 4.22.

The cirro-stratus cloud during and after the display was rapidly advancing from the north.

Driffield, April 9.

J. LOVELL.

Cambridge Anthropometry.

I HAVE read with much interest, in NATURE of March 13 (p. 450), Mr. Venn's very interesting article on anthropometry at Cambridge.

There is in his tables one rather peculiar feature, of which I find no notice taken in the text. It will be seen on reference to the tables that, while the other physical characteristics increase from A to B, and from B to C (weight and height being irregular, however), the *breath* is highest in A, less in B, and least in C; thus falling with the intellectual fall.

It is true that the difference in this as in most of the other characteristics is so slight as to be—as Mr. Venn says—practically negligible; but still the fact that this should steadily *fall* instead of rising with the other physical characteristics strikes me as peculiar. I should be glad therefore to hear if Mr. Venn has any comment to make on this phenomenon, or any explanation thereof to suggest.

F. H. P. C.

April 4.

A Remarkable Meteor.

ON Thursday, April 10, at 10.40 p.m., I observed a meteor of extraordinary brilliancy shoot from a point just east of B Leonis. It travelled over about 10° in a north-westerly direction, and was visible for fully two seconds. Its apparent diameter, as nearly as I can judge, was about a quarter of that of the full moon; its colour, a very vivid pale green.

J. DUNN.

Much Marcle, Herefordshire, April 11.

Earthworms from Pennsylvania.

NEARLY twenty years ago, a very aberrant earthworm was described by a French naturalist, who obtained it from Pennsylvania. I should be greatly indebted to any naturalists or travellers who may find themselves in that part of the United States, if they would collect some of these worms and send them to me. The most convenient mode of transmission would be to pack the living worms in *moist* earth with moss or grass, in a tin box perforated at one end: this should be inclosed in a wooden box. Both small and large worms should be collected: some might be preserved in strong spirit, but living specimens would be the most useful.

W. BLAXLAND BENHAM.

University College, London, April 10.

Crystals of Lime.

SINCE the appearance of my letter on this subject (p. 515) I have found that similar crystals have been recently observed by Mr. J. Joly, and were described by him in the Proceedings of the Royal Dublin Society, vol. vi. p. 255.

H. A. MIERS.