antiquity of man, primæval man, and ancient lake-dwellings. The section on "Protoplasm" includes papers on the origin of life, the basis of life, bacteria, disease germs, and fermentation. Under "Colour" we find articles on the colour of flowers, the colour of animals, and warning colours and mimicry. "Movement" takes in essays on movements in plants, the sleep of plants, climbing plants, and carnivorous plants. Discussing so many subjects, the writer is, of course, obliged to content himself with the statement of very wide views; but his expositions are so clear and fresh that the book ought to be of considerable service to the readers to whom he specially appeals. It will give them at least a general conception of the nature and direction of some of the lines of modern research, and may induce them to seek elsewhere for fuller knowledge.

LETTERS TO THE EDITOR.

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Panmixia.

I REGRET that I was led to doubt the sincerity of Mr. Romanes when he professed to have formed the conclusion that my words meant the reverse of their plain significance. I had not supposed that there was any one capable of making such a mistake.

I should be glad to terminate this discussion by a brief statement of the divergence of view between Mr. Romanes and myself as to the original matter in question, from which Mr. Romanes has led the correspondence by raising a variety of collateral issues. At the same time I should like to take the opportunity of saying what I believe Mr. Romanes would reciprocate, viz. that there is no ill-feeling but only a divergence of opinion between us.

Mr. Romanes definitely states that when an organ has become useless it will decrease in successive generations as a result of "cessation of selection" to about half its original size, without the co-operation of any such cause as economy of growth. He has repeated in effect this statement in his last letter. The result attributed by him to mere cessation of selection is, it must be noted (because he shows a tendency to waver and to substitute "degeneration" for "decrease in size"), a decrease of size: a mere failure in the exact adjustment of the parts of a complex organ is *not* the result in question. Of this I have a few words more to say below.

Mr. Romanes not only attributes the decrease in size of a useless organ to the cessation of selection pure and simple, but he calls that condition "a causal principle," and claims to have discovered it.¹ He has also stated that, whilst (to use his own words) "inherited decrease" of an organ *must* be due to this principle, it is "remarkably strange" that Mr. Darwin had overlooked it, and that it was unfortunate that he (Mr. Romanes) only gained the idea of this novel principle just after the appearance of the last edition of the "Origin of Species."

On the other hand, I consider that Mr. Romanes, by these contentions, obscures the theory of organic evolution, and that he presumes to censure Mr. Darwin without cause. There is nothing unfortunate in the date of Mr. Romanes's idea, because the idea is entirely erroneous; and it was no strange oversight of Mr. Darwin not to attribute the decrease of useless parts to "the principle of cessation of selection," or, in other words, to their uselessness alone—for the simple reason that he would have made a blunder had he done so. It is this blunder which Mr. Romanes places before us as his own contribution to the theory of panmixia: it is this blunder which Mr. Darwin not only did not make, but rendered *almost* impossible for others by his discussion of the matter ("Origin of Species," p. 401).

It is an incontrovertible mathematical fact that *the only effect of promiscuous breeding or panmixia* (considered apart from all other influences) *upon an organ or part which presents variations round*

¹ The erection of a negative condition—a cessation—into the position of a causal principle is an artifice which is very likely to obscure the view of the related facts. The "causal principle of non-existence" and "the reversal of being," would be worthy of the author of the artifice who professes also to have extracted an essence from an idea—the idea of promiscuous breeding, or panmixia!

an average mean will be to increase the number of individuals near the average mean, in proportion to the number of generations in which the panmixia is operative. The notion that the haphazard interbreeding of "variations about a mean," must by itself lead to a shifting of the mean in the direction of diminished size—without the assistance of any special cause favouring reduction in size—is, to put it plainly, absurd.

It is, I believe, a mistake on the part of Mr. Romanes to say that Galton, Weismann, and Poulton agree with him in this astonishing fancy. But, were this the case, the mathematical fact would remain as it is.

Given a race of organisms in which a part has become useless, it is only (as Mr. Darwin pointed out) when some cause (such as economy of growth) favouring diminished size is operative, that the average mean of the size of the part will in successive generations shift in the direction of decrease. Mr. Darwin saw this, and explained it. Mr. Romanes not only failed to appreciate the considerations advanced by Darwin, but actually now charges him with oversight for not having made the blunder which he carefully avoided.

In conclusion, I have a few words to say in regard to the possibility of an organ consisting of several nicely adjusted parts losing that adjustment in a state of panmixia without the co-operation of economy of growth. Mr. Romanes erroneously declares that if we admit this we must also admit that decrease in size must similarly result. I am not surprised to find that he thinks so, and do not doubt his sincerity. But really the two cases present very different problems. Suppose the organ in question to be represented by fifty independent variables; then we have to consider not the probability of the average mean of each kind of variable being maintained but the probability of the production of the necessary combinations of fifty of them with the specific initial proportions of each of the fifty elements. Whether it is or is not probable that the complex adjustment and interaction of parts would be maintained in the absence of all interfering causes in a state of panmixia is a difficult question. It is one which is hardly worth further discussion, since it is impossible that the results of panmixia without such interfering causes should ever present themselves in organic nature.

It is, moreover, quite certain that any conclusion we may adopt in regard to that matter will not alter the mathematical fact that, given a numerous race and a long series of generations, the average mean round which the variations in size of a useless organ are distributed will not ultimately shift in the smallest degree either towards increase or decrease of size, as the result of the promiscuous interbreeding of the variations.

April 26.

E. RAY LANKESTER.

The Inheritance of Acquired Characters.

It surprises me to find that anyone who has looked into the evidence can doubt that acquired characters, as distinct from congenital ones, may, like congenital characters, become hereditary, and produce physiological effects. The instance mentioned in Herbert Spencer's letter in NATURE (vol. xli. p. 511), of domestic varieties of animals losing the power of erecting the ears, appears as nearly conclusive on the subject as such an instance can be.

On the habits or instincts of domesticated varieties, Darwin says:—"It may be doubted whether anyone would have thought of training a dog to point, had not some one dog naturally shown a tendency in this line. . . . When the first tendency to point was once displayed, methodical selection and the inherited effects of compulsory training in each successive generation would soon complete the work" ("Origin of Species," 4th edition, p. 256).

I quote another instance from Carpenter's "Comparative Physiology" (p. 987):—"Sir C. Lyell mentions that some Englishmen, engaged in conducting the operations of the Real del Monte Company in Mexico, carried out with them some greyhounds of the best breed to hunt the hares which abound in that country. It was found that the greyhounds could not support the fatigues of a long chase in this attenuated atmosphere, and before they could come up with their prey they lay down gasping for breath; but these same animals have produced whelps which have grown up, and are not in the least degree incommoded by the want of density in the air, but run down the hares with as much ease as do the fleetest of their race in this country."

Mr. Gulick's letter in NATURE (vol. xli. p. 536), insisting that the first and only absolutely essential factor in the

production of new varieties or species is the isolation of a portion of the race, appears very luminous. On this subject, let me again quote from Darwin:—

"Youatt gives an excellent illustration of the effects of a course of selection, which may be considered as unconsciously followed, in so far that the breeders could never have expected, nor even have wished, to produce the result which ensued—namely, the production of two distinct strains. The two flocks of Leicester sheep kept by Mr. Buckley and Mr. Burgess, as Mr. Youatt remarks, 'have been purely bred from the original stock of Mr. Bakewell for upwards of fifty years. There is not a suspicion existing in the mind of anyone at all acquainted with the subject that the owner of either of them has deviated in any one instance from the pure blood of Mr. Bakewell's flock, and yet the difference between the sheep possessed by these two gentlemen is so great that they have the appearance of being quite different varieties'" ("Origin of Species," 4th edition, pp. 37, 38).

JOSEPH JOHN MURPHY.

Belfast, April 24.

THE fifth caudal vertebra of a tortoiseshell cat at the Sussex County Hospital is dislocated and attached at right angles to the long axis of the fourth. The sixth and last vertebra is also affixed at right angles to the fifth. The cat is able to wag the terminal phalanx of the tail, and the distortion has always been considered to be due to an accident when the animal was a kitten. Within the last week the cat has had a litter of several kittens, two of which were born almost tailless, one possessing (as far as I could ascertain by external manipulation) two caudal and the other three caudal vertebrae only. Whether the original distortion is due to accident or not, I think these facts may interest some readers of NATURE.

W. AINSLIE HOLLIS.

Brighton, April 28.

P. S.—Since writing the above note I have had an opportunity of examining the two remaining kittens of the litter, and I find that only one of these has a normal tail. The other is docked of one or two of the terminal vertebrae, and the tail has a slight twist on itself towards the end.

W. A. H.

April 30.

Variation in the Nesting-habits of Birds.

IN considering the interesting question of instinct, one naturally turns to the nesting-habits of birds as affording an apparently good instance of habit acquired and perpetuated so as to become fixed, and, as we say, instinctive. It would be interesting, however, to find exactly how far the art of nest-building is really inherited, and how much uniformity exists among the nests of birds of identical specific characters.

The "blackbird" of this region, *Scolecophagus cyanocephalus*, is rather noteworthy in this connection. Goss, in his "Birds of Kansas," says this bird breeds in trees and bushes, from three to thirty feet from the ground. In Colorado, as observed by Mr. Morrison and myself, it breeds sometimes on the ground, and sometimes in low trees or bushes. In Custer County, Colorado, I find it breeding on the ground, sometimes at the very edge of creeks, in places where arboreal nests might have been made, and also better concealed ones. Captain C. E. Bendire, who inclines to the opinion that this bird breeds diversely in all parts of its range, where opportunities offer, writes (*in litt.*):—"I have found them nesting abundantly both on the ground and in bushes in the same locality and close together in Oregon. One thing struck me as peculiar: the nests when placed on the ground were almost always to be found on the extreme edge of a creek bank, when they could have selected far more suitable places, better concealed ones at any rate, a few feet away from the bank." This selection of creek banks, noticed both in Colorado and in Oregon, is remarkable. It had occurred to me that in Colorado the habit might have been formed to lessen the risk of being trampled upon by the herds of buffalo which used to inhabit this region, but Captain Bendire tells me the habit is observed also in regions where there never were any buffalo, which throws doubt upon my explanation.

Captain Bendire, who has so excellent a knowledge of the nesting-habits of American birds, kindly gives me a few notes on the subject, which it may be permissible to quote.

"Birds in the selection of their nesting-sites will adapt themselves to circumstances, as is well known, but as in the case just mentioned [*Scolecophagus*] it is hard to arrive at an entirely satisfactory conclusion. It is, for instance, easy to account for,

why the *Archibuteo ferrugineus* should breed on the ground in Dakota, in many cases at any rate, and why *Falco peregrinus anatum* in trees in Kansas, but there are a number of other such departures from the old established rules, which cannot be so easily accounted for" (C. E. Bendire, *in litt.*, January 21, 1890).

Captain Bendire also cites *Buteo swainsoni* and *Archibuteo ferrugineus* as birds which sometimes nest on the ground in places where there is plenty of suitable timber, which one might have expected them to make use of.

These variations in habit are certainly puzzling: probably the important factors in deciding the terrestrial or arboreal nesting-habits of a bird are four:—

(1) *Ability to build arboreal nests.*—If this varied in a locality where arboreal nests were not greatly preferable to terrestrial ones, we can see how a minority of clever birds might build in trees, and a majority of duffers on the ground. The slight disadvantage to the ground-builders might be counterbalanced by their numbers.

(2) *Danger of falling.*—In regions where the trees are not suitable for holding nests, or where very high winds prevail, a terrestrial nest might be preferable; though the same species in another part of its range might do well to build arboreally.

(3) *Dangers of nesting on the ground.*—Such dangers would arise from terrestrial enemies, floods, &c., and would vary greatly no doubt in different regions. Where things were otherwise fairly balanced, a slight difference in this respect might decide the nesting of a bird.

(4) *Means of defence.*—Some birds, with special means of defence or of escaping observation, might build on the ground where others would take to trees.

T. D. A. COCKERELL.

West Cliff, Custer Co., Colorado.

Russian Transliteration.

I AM afraid the authors of the "new system" of transliteration have misunderstood my letter in yours of April 10 (p. 534), advocating "the tabulation of the system of transliteration which has been so long in use in this country" in preference to the adoption of the unnecessary novelties they propose to introduce. By the "system in use" I meant that for transliteration from Russian into English, and certainly did not include the transliterations from Russian into German which have been copied from books or memoirs in that language into English catalogues or journals. As practically all the examples the authors adduce in defence of their "new system," including both the atlases and the works with which they associate my name, are of this kind—*i.e.* merely copies of transliterations from Russian into German—I fail to see what bearing they have on the question of transliteration into English, however useful they might be in constructing a system for transliteration from Russian into German.

Another misapprehension is, they seem to imagine that I have propounded a system of transliteration of my own. I sincerely hope I shall never be guilty of doing anything so rash. I merely offered some friendly criticisms on the new system which the authors had devised, and I may supplement my remarks by here giving in tabular form the principal points in which this system differs from that which I conceive to be the English use:—

	English Use.	New System.
В	... <i>v</i>	... <i>v</i>
ВѢ	... <i>ff</i>	... <i>v</i>
г	... <i>h</i> before <i>e</i> or <i>i</i> , otherwise <i>g</i>	... <i>gh</i>
ж	... <i>j</i>	... <i>zh</i>
к	... <i>x</i>	... <i>ks</i>
у	... <i>ou</i>	... <i>u</i>
х	... <i>ch</i>	... <i>kh</i>
ч	... <i>tch</i>	... <i>ch</i>
ш	... <i>shch</i>	... <i>shch</i>
ѣ	... <i>é</i>	... <i>ye</i>
ѣ	... <i>y</i>	... <i>ie</i>
ю	... <i>u</i>	... <i>yu</i>

I have already given a few examples of names which look uncouth when transliterated according to the new system, and I here add one more. It is

SKRZHIPSKIĬ.

When I wrote it down and observed its hieroglyphic appearance, there arose somehow in my mind a vision of a new system of chemical nomenclature devised many years ago by Laurent,