

variation in the same lines of function as those which their acquisition by the individual called into play. But there is no need in either case to assume the Lamarckian factor.

The intelligence holds a remarkable place in each of these categories. It is itself, as we have seen, a congenital variation: but it is also the great agent of the individual's personal adaptation both to the physical and to the social environment.

The emphasis, however, of the first of these two lines of hereditary influence gives prominence to instinct in animal species, and that of the other to the intelligent and social coöperation which goes on to be human. The former represents a tendency to brain variation in the direction of fixed connections between certain sense centers and certain groups of coördinated muscles. This tendency is embodied in the white matter and the lower brain centers. The other represents a tendency to variation in the direction of alternative possibilities of connection of the brain centers with the same or similar coördinated muscular groups. This tendency is embodied in the cortex of the hemispheres. I have cited 'thumb-grasping' because we can see in the child the anticipation, by intelligence and imitation, of the use of the thumb for the adaptation which the simian probably gets entirely by instinct, and which I think an isolated and weak-minded child, say, would also come to do by instinct.

IV. Finally there are two general bearings of the position taken above regarding the developmental function of intelligence and imitation which may be briefly noted:

1. We reach a point of view which gives to organic evolution a sort of intelligent direction after all; for of all the variations tending in the direction of an instinct, but inadequate to its complete performance, *only those will be supplemented and kept alive which the intelligence ratifies and uses for the animal's personal adaptations.* The principle of selective value applies to the others or to some of them. So natural selection kills off the others; and the *future development of instinct must at each stage of a species' development be in the directions thus ratified by intelligence.* So also with imitation. Only those imitative actions of a creature

which are useful to him will survive in the species; for in so far as he imitates actions which are injurious he will aid natural selection in killing himself off. So intelligence, and the imitation which copies it, will set the direction of the development of the complex instincts even on the Neo-Darwinian theory; and in this sense we may say that consciousness is a 'factor' without resorting to the vague postulates of 'self-adaptation,' 'growth-force,' 'will-effort,' &c., which have become so common of late.

2. The same consideration may give the reason in part that instincts are so often coterminous with the limits of species. Similar structures find the similar uses for their intelligence, and they also find the same imitative actions to be to their advantage. So the interaction of these conscious factors with natural selection brings it about that the structural definition which represents species, and the functional definition which represents instinct, largely keep to the same lines.*

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INSTINCT.

EDITOR OF SCIENCE: Some remarks appended to my letter published in SCIENCE No 62, on the subject of Prof. Morgan's views on instinct by 'The Writer of the Note,' in view of the importance of the subject are worthy of further consideration.

Before drawing conclusions from observations on domestic animals, it is well to consider similar facts in connection with their wild congeners, especially if such conclusions are of a far-reaching character, and it cannot be too well borne in mind that our experiments are very clumsy imitations of nature in a large proportion of cases.

* In conversation with Prof. Lloyd Morgan I was glad to find that he was inclined to interpret the facts which I have quoted from him (and others) in somewhat the same way—that is, as pointing to general conclusions similar to those reached above. While I have reached my conclusions quite independently and from a psychological point of view, any confirmation which they get from so expert and eminent a biologist gives them much greater weight.

If food be set down in considerable quantity before newly hatched chicks, and in a vessel similar to that in which water is usually held, they will be relatively slow to recognize and eat such food. But in a wild state the congeners of the domestic fowl, as grouse, pheasants, etc., do not find food or water before them in such way. Their food is distributed, however, much more like the particles we scatter before the chick than does their water supply resemble that of our methods.

A young grouse would naturally get its water from the dew on herbage, possibly from rain water that had gathered in little hollows of the ground, surface, etc. And when the birds approach a stream the surface near is moist or wet, the particles it would naturally peck at would be found up to and beyond the very margin of the water, so that the contact of the beak with water in all these cases would be inevitable and drinking would come about as naturally as eating.

When the 'writer of the note' says, 'A chick swallows water instinctively, but must be taught to drink by example or accident,' the latter term evidently having reference to the observation specially described in my letter, he plainly either misses the real point of my observation or neatly evades it. One might as well say a puppy learns to smell by accident, for in the case in question the chick did not swallow water merely, but raised its head like an old fowl and *drank* perfectly well on the very first occasion that its beak had ever been immersed in water (as a puppy sucks when its lips first come in contact with a teat, etc.); and this I take it is what happens in nature. The young grouse in the forest, or even the chick on a grass plot or in a garden, would come in contact with water without any assistance from the mother bird.

The assumption that 'the chick might die of thirst in the presence of water, as the sight of water does not call up the movements of pecking at it as do food and other small objects,' is purely gratuitous. It is not primarily so much the sight, but rather the touch of water, inevitable, as I have tried to show, in a wild state that in the very first instance leads to drinking, though the bird would also peck at shining dew drops, as my chick did at the drops on the rim of a

vessel containing water. With a fair chance and plenty of water about in a condition at all resembling that in nature, there is no such thing for a vigorous, hardy chick as death from thirst.

That habits may be hereditary in dogs I have many times observed in my own kennel during the last eight years, and, without expressing any opinion as to the origin of instincts now, I can see no impossibility in their dating back to habits.

A doctrine which asserts that eating is instinctive, but that drinking is not, is to my mind one to marvel at, and is a poor foundation for theories of evolution or heredity.

Comparative psychology will, I fear, continue to suffer till those who assume to deal with it authoritatively spend more time among animals, and less in their studies. A few observations or experiments do not give them insight into the psychic nature of animals, and it were well, I venture to think, if the qualifications of the comparative psychologist, as set forth by Dr. Groos, in the preface to his admirable work, "*Die Spiele der Thiere*," were thoroughly known and believed in by all psychologists.

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PECULIAR ABRASION OF TREE TRUNKS.

PASSING recently through a tract of rather open forest land, I could not help but notice a very peculiar appearance or color showing to a nearly uniform height on the westward side only of many trees of different species.

This shade of dull yellow extended from the surface of the snow to a height of about three to four feet, and at a little distance had much the appearance of a fungoid growth which often may be seen in nearly this color on dead or decaying trees.

At first I was completely deceived, thinking it to be a growth of this nature, and wondering why it should have attacked so many trees at the same time, I proceeded to investigate. A close examination at once revealed the truth of the matter. It was a plain case of wind-carried snow and sleet versus tree trunks, and the outer moss-grown bark had succumbed as its cut and abraded surface made plain. In places this