

"Feasible Experiments on the Possibility of Transmitting Acquired Habits by Means of Inheritance"

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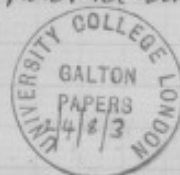
Fowls. The art of hatching in incubators is now so well understood and so largely practised for commercial purposes ^{that it is well worth considering whether any} and what very simple experiments might be regularly ^{carried out} on chickens hatched in incubators. ^{at small cost} during the process of rearing them. I do not profess to do more than suggest certain general ideas for discussion in the hopes that they may be ultimately ~~cast~~ moulded into a practically useful shape. The particular example ^{that I have} will now given must be looked upon in that light only, namely as an illustration of what is wanted and of what seems in a general way to be feasible, ^{and} as a matured and definite proposal. The example is as follows:-

Certain mimetic insects which are in reality good and desirable food, are avoided by certain birds, some say instinctively, on account of their superficial resemblance to other insects which are acrid to the taste or dangerous from their ~~biting~~ stings. It might be possible to find ~~such~~ mimetic insects that are instinctively avoided by fowls ^{and that at the same time can be easily reared}. These would be given to the chicks ^{who would at length become accustomed to eat them and had learnt to like them}. Then it would be a matter of observation open to any one to determine whether the stock of a breeder reared under these conditions for several generations, showed ^{lessened} signs of ^{any} instinctive avoidance of these insects, ^{being} ^{lessened}, when they were first presented to them. ~~Others~~. The whole of the stocks of some breeders being submitted to this experiment and those of other breeders being left alone. ^a comparisons ~~between~~ ^{between} the two could always be made between them.

Though ~~these~~ this particular experiment may perhaps not prove feasible, its description will serve as an example of what should be aimed at. Namely, to teach the poultry to welcome some particular signal for food or other desirable end ^{which} was at first instinctively unwelcome to them. Considering the terror ^(at hearing certain cries) that fowls are said to show ^{instinctively}, it is quite conceivable that sound-signals might be an effective ~~means~~ means of experiment.

However ^{at every part} the ^{first} step before ^{deciding} ^{seriously} ^{determining} ^{any} course of experiment, should certainly be to observe more carefully than has ^{heretofore} been done, the degree of aversion or of pleasure that newly hatched fowls ^{may} ^{really} ^{show} to each of a variety of sight and of sound signals. ^{and} ⁱⁿ ^{the} ^{early} ^{stages} ^{of} ^{their} ^{life}. ^{though} ^{numerous} statements have been made ^(it is quite possible that) very little may ^{have} ^{been} ^{correctly} ascertained, and that ^{the} ^{experiences} [&] ^{associations} ^{of} ^{thought} in ^{these} ^{young} ^{things} may play a larger part than we are aware of, in the interpretation placed by chickens even of a few days old, on the sight and sound ^{and} signals by which they are guided.

Feasible Experiments on the possibility of transmitting
acquired habits by means of inheritance.
by Francis Galton F.R.S.



Feasible experiments have yet to be designed that shall be recognised by experts as satisfactory tests of the hereditary transmission of acquired habits. In other words, of the existence of a tendency in parents to transmit congenital aptitudes to their children to do things for which they themselves had no congenital aptitude, though they had acquired an aptitude for doing them under the compulsion of circumstances.

It is almost needless to say that this is one of the most important problems that vex the student of heredity at the present day, and perhaps the most difficult of solution. But it is only of late that its difficulty has become generally appreciated. Thanks especially to the searching criticism of Professor Weissmann, grave doubts have fallen on the adequacy of the data upon which the old popular belief is based, of the possibility of inheriting ^{purely} acquired faculties. We now see more clearly than before, the insufficiency of all evidence that shows no more than this; namely when a parent has acquired some particular aptitude there are many instances in which his children are found to inherit that aptitude. Whenever the evidence goes no further than this, it is fatally incomplete; for it may well be, that the parent was himself congenitally gifted with a disposition that made the process of acquirement a pleasure ^{to him} and with a capacity that made it easy. In this very common case, the children may have inherited no more than the congenital gifts of the parents, & not the acquired ones, and these will frequently be developed ^{in their} early life through imitation or tradition. Such instances as these, give no help to solve the question whether a parent who had no peculiar congenital aptitude, but who acquired a peculiar aptitude through long practice enforced by circumstances, is thereby enabled to transmit to his children a stronger congenital aptitude than he had himself possessed.

~~With~~ ^{for} devising experiments to solve this difficult problem, many stringent requirements must be regarded. The first is completely to eliminate the influence of parental education and of social tradition from the children. Another is uniformity of nurture. Thirdly we ought to carry on experiments of an almost self-acting character in many different stocks, simultaneously, and during many generations. We must do this without fear of the results of our experiment being biased through the influence of any form of natural or artificial selection. Lastly we have to economise in time, labour and money.

All these requirements taken together, point with great emphasis to the selection of such creatures only, as are artificially reared from eggs, for the purpose of experiment. Fowls seem especially suitable, partly on account of the variety of their aptitudes, but moths, small fish, and even frogs, might perhaps be utilised. Plants, from their deficiency in mental faculties, hardly come under consideration.

Fowls. The art of hatching in incubators is now so well understood and is so largely practised for commercial purposes, that it is very well worth considering whether any and what very simple experiment might be regularly carried on at small cost, on chickens hatched in that way. I do not profess to do more than suggest certain general ideas for discussion, in the hope that they may be ultimately moulded into a practically useful shape. The particular example about to be given, must be looked upon in that light only, namely as an illustration of what is wanted, and of what seems in a general way to be feasible, and by no means as a matured and definite proposal. The example is as follows -

Certain mimetic insects which are in reality good and desirable food, are avoided by certain birds, some say instinctively, on account of their superficial resemblance to other insects which are acrid to the taste or dangerous from their stings. It might be possible to find mimetic insects that are instinctively avoided by fowls, and ~~which~~ at the same time can be easily reared. These would be given to the chicks who would at length learn to eat them greedily. Then it would be a matter of observation to determine whether the stock of a breeder, reared under these conditions for several generations, showed lessened signs of an instinctive ~~abhorrence~~ avoidance of these insects when first presented to them. The whole of the stocks of some breeders being submitted to this experiment and those of others being left alone, comparisons could always be made between them.

Though this particular experiment may not prove feasible, its description will serve as an example of what should be aimed at. Namely to teach the poultry to welcome some particular signal for food or other desirable end, which signal was at first instinctively unwelcome to them. Considering the terror that fowls are said to show instinctively at hearing certain cries, it is quite conceivable that sound signals might be effective means of experiment.

A very early step before seriously considering any course of experiment should certainly be to observe more carefully than has I believe, hitherto been done, the degree of aversion or of pleasure that newly hatched fowls really show to each

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of a variety of sight and of sound signals, and their individual differences in these respects. It is quite possible, that though numerous statements have been made, very little may have been correctly ascertained, and that experience and associations may play a larger part than we are aware of, in the interpretation placed by chickens even of a few days old, on the sight, and sound ^{and other} signals by which they are guided.

Moths. If a proper species of moth be selected, two broods can be reared in each year without difficulty. The moths that are now being reared for me for quite another inquiry, are the Selenia Illustraria, ~~and~~. I have already many healthy broods in the fifth generation, thanks primarily to the care of Mr F. Merrifield, and now to Miss Prudham and Mr. Welldon also. It is worth considering whether experiments analogous to those suggested in respect to fowls, are worth making upon the larvae of moths. Larvae are fastidious in their diet, but leaves and other food apparently exist, that they refuse at first to touch though they afterwards feed greedily upon it without any harm to health. It would be instructive to gather the experiences of moth-breeders on this point, and doubtless the rearers of silk worms have abundance of appropriate information.

Fish. The fish that inhabit waters previously unvisited by anglers, are readily caught by a baited hook, but they soon become wary. The most guileless of them are the first who are captured, and their breed is extinguished, while the survivors are educated to the fear of latent danger by each newly hooked and floundering fish. How far this acquired dread may assume the form of an hereditary instinct, remains to be proved. I presume that some of the smallest species of fish admit of being easily reared in multitudes, ~~the~~ Members of any generation in which observation is desirable ~~being~~ would be hatched in a separate receptacle, newly cleaned out for them, so that they shall receive no education from members of any preceding generation.

A classical experiment by Mobius shows the possibility of impressing a emotion on the mind of a fish, that is afterwards retained with tenacity, and ought to be transmitted by inheritance if the transmission of such acquired emotions be possible. He prepared a long trough full of water and divided it into two compartments by a strong plate of clear glass, set crossways. He put a live pike into one compartment and live minnows in the other. The pike seeing the minnows through the glass, but not seeing the glass itself, soon made a dash

at them. His snout struck heavily against the interposed glass, and he was thrown back baffled and presumably somewhat hurt. Again he tried, and again with the same result. Being a stupid sort of fish, he continued to make these fruitless dashes at intervals during a month, and then he sulkily and wholly desisted. The idea had become at last fairly hammered into his brain, that the minnows were inviolably protected and that it was both a useless and painful act for him to attempt to seize them. At this stage of the experiment Mobius removed the glass partition, but the pike thence forward made no further attempt in the liver of the minnows. What would the descendants of that pike have done under similar circumstances, would they be taught a little more quickly than he had been? This little anecdote is offered merely to show the sort of experiment at which we should aim, namely the successful imposition of an originally uncongenial habit.

It now remains to consider how far the information that such experiments as these could afford, would settle the question of the possibility of transmitting heredity any habits that may have been acquired. It ought to assign limits to reasonable doubt. We should learn that the number of times in which an acquired habit was inherited to any sensible degree, after some specified number of generations, did certainly not exceed so and so per cent. We might also learn that it certainly did exceed some lesser number. We cannot prove any negative, and therefore cannot hope to prove that acquired habits are never inherited, but we may perhaps find no case of their being inherited in any appreciable degree, even after many generations. This seems to have been already done in the case of many artificial mutations. But as the loss of a limb belongs to a different order of events to the loss of an instinct, it would be dangerous to conclude that what has been found to occur in the one case would necessarily occur in the other. It therefore appears to me that the possibility of experiments along the lines I have suggested, in creatures reared from eggs and away from their parents, are well worth considering. It is only through the hard compulsion of unquestionable facts, that we are likely to forego the wholesome moral doctrine that the self-denials and toils of the parent will earn for his children a happier nature than he had himself been gifted with. That they will be congenitally more disposed than he was to deny themselves harmful pleasures, and to work generously for the good of others. It must be recollected that we are no longer justified in idly holding this comfortable belief, and in preaching about it as if it were unquestionably true.

So far from this being the case the balance of probability seems strongly against its truth. But the question is not yet solved. The possibility of a child inheriting even the simplest aptitudes that the parent has acquired against the grain of his nature, can only be determined by a direct appeal to suitable experiments. The object of this memoir is to obtain the opinion of experts as to the most feasible experiments for the purpose
