

Desiderata in Hereditary Science

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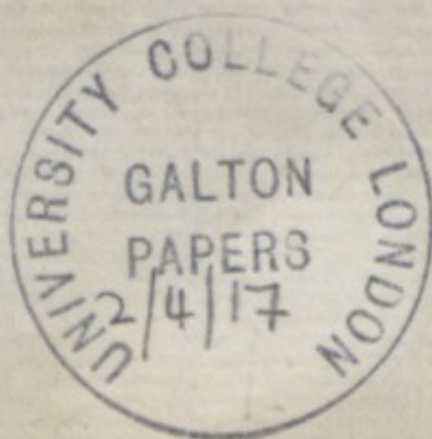
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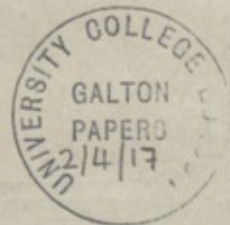
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Desiderata in hereditary science, and observations
that might supply them,
by Francis Galton F.R.S.

Permit me to begin my remarks with a tribute
to the memory of Prosper Lucas, author of
"L'Hérédité Naturelle". Though his examples
may have been often unverified and his conclusions
rash; yet the fact remains that his was the first
substantial work on the subject and that the
formidable array of his instances in proof,
made it thenceforward impossible to gainsay
the claims of heredity to serious consideration.

A congress is admirably qualified to do
two pieces of good work. (1) It can bring
to light such doubts as may exist among
reasonable and educated persons on matters
connected with Heredity, and (2) it may
consider the best methods of solving disputed
questions by observation and experiment.

Mother's marks: -

It appears that there are many persons whose opinions are entitled to respect, who still hold the opinion that mental shocks & strong emotions in the mother during pregnancy may so affect the child that it shall bear marks whose form or character shall resemble the cause of those shocks or emotions. It is therefore proper to make observations that will help to prove or disprove the allegation. I am not aware that those cited by Darwin as having been made by his own father, have ever been repeated under strictly scientific supervision. The statement was that Mr Darwin's father, being for many years physician to a lying-in hospital, had made a point of enquiring of each pregnant woman on her admission, whether she had received any shock, or had suffered in any way that she thought might mark the child, and that ^{although} many of the women had anticipated such results it was in no case found that the child was affected in the way she anticipated. A repetition of this method of observation under a well thought out

System of enquiry and registration, with drawings and photographs in important cases, seems desirable, room being left for those who might think there ^{was} ~~was~~ ^{flaw} in any case to record ^{their dissent}. In this way, some ^{useful} conclusions as the following might be arrived at. (1) That in a certain proportion of cases the mother confessed to somewhat painful anticipations. (2) that these anticipations were verified in a proportion that some observers put as high as 40 & 50 & others as low as 20 & 30. The experience at different lying-in hospitals would serve to check one another, and the results ought to be as trustworthy, neither more nor less so, than statistical results usually are.

Inheritance of acquired habits Feasible experiments have yet to be designed that shall be recognised on all sides as ^{and fair} crucial tests of the possibility of inheriting the acquired mental habits of parents. From a social and moral point of view as well as from a purely scientific one, this is perhaps the most important problem at the present time to the student of heredity and the most difficult ^{of all} to solve. It is needless to

and salutory.

do more than allude to the strict criticism to which the facts have been subjected (especially by Professor Loebman), upon which the popular opinion is based, that acquired habits admit of being hereditarily transmitted. We have learnt ^{more clearly than before, when} to see that an observer states that a parent whom we will call A, has acquired some particular aptitude and that the children of A inherit that aptitude, ^{his} the statement is too incomplete to be of service. There is nothing so far to show that A did not possess a congenital tendency towards the aptitude in question & that his children ^{merely} inherited no more than this same tendency; we all agree that congenital tendencies are inherited, but the doubtful point is whether those who have no remarkable natural gift but who have acquired ^{solely} the force of circumstances and long practice ^{some} remarkable aptitude, do as a whole tend to produce children who ^{are} naturally apt to a remarkable degree to that same aptitude.

In experimenting upon this, a primary point is to eliminate the influence of maternal teachings and of social tradition. We should also limit as far as possible the variety of the environment. It is further necessary to deal with ~~very~~ numerous individuals. These requirements as well as considerations of

cost and time seem all to point with great emphasis to experiments on creatures produced from eggs, especially fowls, and in a less degree to fishes & to moths owing to their much lower intellects. The hatching of eggs in incubators is now so well understood and largely carried on for commercial purposes, that it is very desirable to consider whether suitable experiments might not be made incidentally during the ^{ordinary} process of rearing, ^(and) at a small cost. I have myself no experience in rearing fowls and but little in rearing insects, but knowing how certain mimetic insects are ^{instinctively} avoided by birds on account of their outward likeness ^{to some creature they detest, although the insects are good & desirable food}, it would seem possible to rear such insects & accustom the fowls to eat them & to wait for a few years as to whether the descendants of these fowls did or did not eat the mimetic insects when first presented to them. Of course a precaution must be taken not to breed from those fowls only who had shown any special proclivity, but to deal with the whole stock of ^{the same} breeder in the same way; - that of one breeder in one way and that of another breeder in another, & to trace any growing difference in the natural instincts of the two stocks, year by year. Mimetic insects of a suitable kind may not be procurable in sufficient

abundance, but the suggestion may serve as an example of many other feasible attempts in which a summons to ^{or rather} a welcome end, as to food, is given by a signal that was at first terrifying but to which the fowl by slow degrees ^(at first, but at last) thoroughly habituated themselves.

As to Moths, some such experiment might be tried on their hroov.

Also as regards Fish, who ^{inhabiting} any pool or river, ^{bait} very soon acquire dread of an anglers, but how far this result is due to social experience, how far to the destruction of ^{the} naturally unwary fish, and how far to inherited experience remains to be tested.

A well known experimenter, Mobius, placed a pike in a long trough divided into two compartments by a plate of clear glass; the pike was put in one compartment and live ^{innumerable} sustainable ^{his} food in the other compartment. The pike ^{soon} dashed at the fish but ^{he} was checked in mid career by a violent shock ^{of his nose} against the unseen glass partition. He then repeated the process ^{exactly} with the same result, and being a stupid sort of fish, he continued his attempts for, if I recollect rightly, more than a month. At length the idea was fairly hammered into his

Brain that the ^{young} were in some way protected
 and that it was useless for him to try to seize them.
 At this stage of the experiment Nobius removed
 the plate of glass. The pike henceforward ^{abstained from} ~~never~~
 attempting the lives of the little fishes. What would
 his descendants have done? ^{minutely} This little anecdote
 is merely offered as an indication to those in
 charge of marine laboratories, accustomed to
 test fish, of the sort of experiment ^{that is} desirable. It
 is wanted to place small fish, who breed freely when
 in confinement, under conditions naturally distasteful
 to them but to which they learn with ^{some} toil and some
 sorrow to adapt themselves to. Then, - with the
 descendants of those fish, reared from eggs in
 a separate tank, show any of these acquired habits
 as natural instincts?

Regression and variability :- I will now
 consider experiments suitable for determining certain
 numerical constants for use in the mathematical
 formulæ of hereditary probabilities. It can
 easily be shown to those who are familiar with
 the technicalities of the calculus of probabilities,

that equations exist which necessitate certain peculiarities in hereditary relations. It is for example absolutely impossible that brothers should as a rule, be somewhat unlike one another and also that children should as a rule, resemble their parents. If they were the case the statistical conditions of successive generations of the same population could not remain unchanged. It is impossible briefly to explain the reason of this, still less to trace out the results that flow from it, to those who are unacquainted with a very special branch of science, but it may I hope, be provisionally accepted that there are certain very important desiderata that experiment & observation ^{alone} can afford. Those who desire to understand the full object in view, are referred to my recent book called "Natural Inheritance".

The needed inquiries refer to any measurable quality. It will give simplicity to the explanation if stature only is spoken of, taking that as perhaps the best example of what is desired to be learnt.

We want the statures (1) of each & every brother & sister in large fraternities, (2) those of the father of each of these fraternities & of his ^{own} brothers and sisters, also (3) of the

- (1) the measure of the variability among the adult males of the population at large
- (2) that among the females
- (3) the factor required to transmute female measures into their male equivalents
- (4) the mean regression from parent to child
- (5) the measure of "Co-fraternal" variability
- (6) the measure of Fraternal variability
- (7) (Which I have as yet very imperfectly done) ^{change in the} the measure of Fraternal variability & of Regression as the breed becomes more pure.

It is most desirable to check the values I have already obtained, and to obtain analogous values for other animals and for other qualities than those I have dealt with



mother and her brothers and sisters. It is also much needed that such results should be obtained for two distinct groups, (a) that of animals of pure breed, that is to say in whom the parents have been selected for like qualities during many generations, (b) that of animals whose breeding has been promiscuous.

From these data we may obtain (as I have already done for a certain group of human statures) the desired contrasts, whose technical names are

- (1) the mean regression from parent to child
 - (2) the measure of fraternal variability
 - (3) that of co-fraternal variability
 - as well as (4) the factor required to transmute female measures into their corresponding male equivalents,
 - (5) the variability of the adult males among the population at large. & (6) the relative influence of the father & of the mother in transmitting different qualities.
- The values obtained for these, which it is very desirable to check, also to possess for many other qualities, are fully given in the work just alluded to.

It seems to me that the horse breeding establishments, the "Haras" under national control in France, might with great ease be utilised to obtain data of the kind required.

Each stallion is annually the sire of some 40 or 50 foals who are all the subjects of report & registration. All that seems needed besides good will, is to ensure as much exactitude as can reasonably be expected in the reports, and to ask for such other moderate amount of information as shall be requisite from a scientific point of view and not ^{too} onerous to undertake. I can hardly entertain a doubt that a well considered plan would lead to valuable results from every point of view.

Moths appear well suited to the simpler forms of these experiments, and the large establishments of silk worms must afford excellent opportunities for experiment. I have myself established one race of pedigree moths, which seems now to have surmounted the initial risks due to interbreeding and I possess three distinct stocks each of many families ^{each of which} now being reared in three different localities. I heartily wish I could persuade more persons to help in this simple but very instructive experiment. The first step was to obtain eggs of a suitable moth.

The Sellenia Illustraria was chosen for many reasons among which was its rapid breeding, for it passes through two generations in one year. When the moths emerged, a few specimens were selected for breeding, consisting severally of large or A-sized moths, of medium or M-sized, and of small or Z-sized moths. The descendants of these were the originals in the 1st generation of my 3 stocks & were known respectively as A₁, M₁, & Z₁. Then the process was repeated so that the issue of the A descendants of the A₁ stock, the M descendants of the M₁ stock and the Z descendants of the Z₁ stock, formed the second generation known as A₂, M₂, & Z₂. I have now got to A₅, M₅, & Z₅, & every parent & all his fraternity are preserved very compactly, in trays for future study. All the details of this experiment were well thought out by my friend Mr. Merrifield, who most kindly has conducted it for me & they are described in the Journal of the Entomological Society of this & last year. There is found to be no difficulty in retarding the emergence of the moths by a refrigerator or in hastening it by a warming apparatus, so that the emergence of (say) all the A moths can be

rendered simultaneous. They can be chloroformed without any subsequent danger, for purposes of measurement during life, & in fact, partly through Mr. Merrifield's skill & partly from the hardiness of the moth, it seems to be now very completely under control.

What I expect from these experiments, besides the "improvement" which is usually the most interesting of all in such cases, is to find the rate of diminution in Fraternal Variability, (and the diminished Regression as the 'breeds' become more pure.

Origin of new varieties It is very desirable that copious historical records should be encouraged of all remarkable "Sports", whether occurring in plants, brutes or men, and whether they are neglected & allowed to disappear or are nursed by successive selection into new varieties. The object is to learn the way in which the law of regression acts under these ^{various} circumstances. I have shown reason in the book above referred to, why the stability of all varieties ought to

be considered as unsymmetrical, so that if ^{happen to} it be overthrown, the variety is more likely to revert to the parent form than to any other form that departs still more widely from it.

If we have the complete history of the fraternity among whom the sport first occurs & that of the descendants of each of its members, a great deal can be learnt.

Human Heredity Though all heredity seems subject to the same general laws, yet as the constants may differ and as the intellectual faculties of man are so unique in their force, it is best to consider human heredity in some degree as a separate study. The only point on which I have any thing new to suggest is the advisability of limiting any large inquiry to the three fraternities, - Filial, Paternal & Maternal, - and to pay especial attention to those cases in which the members of each of these fraternities are numerous. The statements made about them ~~would be more precisely~~ ^{are} verifiable, the majority of the persons ^{(referred to) would be} actually living, and when the families are large a fair indication of the latent qualities of each individual

is afforded by the patent qualities of some brother or sister. By not attempting too much, more of real value may in the end be obtained. Beside this, it must be recollected that we have to work not only for ourselves but for future generations ^{of scientific men} and that we are doing really good service if we can succeed in establishing the custom of family records, that will afford information to them of a kind that is inaccessible to ourselves.

I have thus endeavoured briefly to sketch out the principal topics ^{on} which it seems (myself) the discussions before this Congress, and the private informal conversations among its members might most profitably be directed in the present stage of our knowledge. It is eminently desirable that suggestions for experiment should be freely thrown out and be helpfully but searchingly criticised before they are undertaken. It is certain that the suggestions and criticisms of the many experts who are assembled at this Congress will have a rare value.

[End]