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TWO PROBLEMS OF REPRODUCTION.

Lecture delivered at St. Thomas' Hospital, on February 28th, 1895.

By EVERETT MILLAIS.

Author of "Rational Breeding," &c.

MR. MILLAIS said:—Mr. Chairman and Gentlemen. You have had the opportunity of examining this evening two varieties of the dog, together with the cross between these two varieties, and finally the produce of the cross and one of the varieties, and you will no doubt have noticed that in this second cross we have returned to the original type of one of the varieties. Now, gentlemen, although there are a number of scientific questions mixed up in the experiment which has been placed before you, I will, at the outset of my remarks this evening, allude to the primary object of the experiment. Just one and twenty years ago I imported into this country from France the ancestor of the crooked legged hounds, which you have seen to-night, and by the additional importation of a few more shortly after I first introduced them here, we have built up a small army. Since then we have practically kept breeding from the same stock without the further addition of fresh blood to add vigour to the breed. In a word, we have been indulging in what is known as consanguineous alliances, or inbreeding, and you, as medical men, must know that such breeding very speedily, unless careful selection is practised, brings about its own logical end, namely, deterioration first of a bodily, and then of a mental nature. Up to now mental deficiency has not greatly appeared, but in one case it appears to have been fairly typical, for one which I gave to Dr. Clifford Allbutt he described to me as "a canine idiot." Bodily deficiency has, to a certain extent, appeared, and it was to repair this form of deterioration that the experiment was made, together with other questions, that I need not here allude to.

At this point I will presume that you ask me why I went to another variety of hound for fresh blood, and why I did not go again to France and bring over fresh examples of the deteriorating variety? Well, gentlemen, in answer to such a question, I may state at once that when we imported our Bassets from France, we imported the best that France possessed, and that, notwithstanding deterioration, what we have in England is better than France can now offer us. Again, supposing that I had gone to France, I doubt whether any animal we could have bought there would have improved our stock here, for the very simple reason that the type one meets with in France is, to our minds, a very inferior hound type to what we have; and not only would our type as a hound type be upset, but by the time we had bred out the inferior type which such a cross with ours would undoubtedly produce, we should be no better off in size than what we were previous to the cross.

Now, since you have had the opportunity of seeing the Basset and the Bloodhound together, it might appear to you, without careful examination, that the two varieties are very different in nature, namely, different in anatomy and in colour. But, gentlemen, this is not so, for there was a day when anatomically, and probably in colour, the two hounds were similar. In other words both are descended from the same stock, namely, the St. Hubert, the immediate ancestor of all the hounds. I will ask you to believe me when I say that the Basset, as it exists in England, is only one of the many varieties of the Basset as it is known on the continent of Europe, and all these Bassets, whether they be smooth-coated or rough, have their prototypes in hounds resembling the Bloodhound in every feature, excepting that of the texture of their coat or their colour. In a word, gentlemen, all the straight-legged hounds come from a common stock, the St. Hubert, and all the crooked-legged ones come from them, and the *raison d'etre* of their being is not difficult to find. Man desired a slower hound than that as typified by the Bloodhound and his foreign cousins, and man set to work to produce him by breeding always from the shortest on the leg, that is, by selection for a given object. By such breeding, there was no impairment in other portions of the hound's anatomy, consequently man obtained what he required, namely, a hound of equal proportions, but differing in the anatomy of the upper and lower extremities. But how came the crook? That, gentlemen, was in the shortest legged hounds, a necessary adaptation to enable them to move with freedom, and to give the necessary stability to the body, which would otherwise be absent, and such adaptation is to be seen for similar purposes in the anatomy of the human female. The Bloodhound is about the highest type of hound-type which exists, but in France there are many more such types than we possess, and there they are termed Chiens Courants, consequently the bird's-eye view of the derivation of the Basset a jambes Torses, commencing with the hound from which it has been bred, is as follows:—The Chien Courant, of some breed; the Petit Chien Courant, of the same breed; the Basset, a jambes droites; the Basset, a jambes demi-torses; the Basset, a jambes Torses. You have thus, as it were, the whole keyboard before you, and it is plainly seen through what various stages the hound has passed, from the big hound to the Basset, with the crooked legs, and if further evidence were wanted we have it in the animals name. The word Basset means a "dwarf," and, as you are aware, a dwarf does not mean necessarily something that is stunted in every organ, as in the case of the Bantam. It may

mean stunted in the limbs, consequently the Basset ought to resemble the bigger hound, but stunted in the limbs, and this is exactly what the Basset is, namely a "stunted hound." The Bassets you have seen to-night are called Artesian Bassets. Why? Because they are the stunted or dwarfed representatives of the Chien D'Artois. The Artesian Basset, a group of which I now place before you on the screen (Phot. I.), is, therefore, a true hound, and although it at first appears to be far removed from such a hound as the Bloodhound, it is in reality very closely related, and differs, as you will observe, simply in the matter of colour, and a modification in the form of the upper and lower extremities, brought about by selection on the part of man, and adaptation on the part of the animal. I therefore employed the Bloodhound as the vehicle for importing fresh blood to counteract the commencing degeneration on the part of the Basset, considering that this cross would be of infinitely greater value to us as breeders than the importation would be of a number of French Bassets of the same variety, but of inferior type and size.

Now, gentlemen, I have remarked that there were other questions at issue besides this question of breeding, and one of them I will mention now, since it was an experiment made at the suggestion of my late—I may, I think, say our late—friend, Prof. Romanes.

This was an experiment to determine whether it was possible for a female in one of the higher forms of life to produce young to two males at the same time, without superfœtation, and I believe that Prof. Romanes asked me to do the experiment because I had been singularly fortunate in inducing pregnancy in bitches by other than natural means.

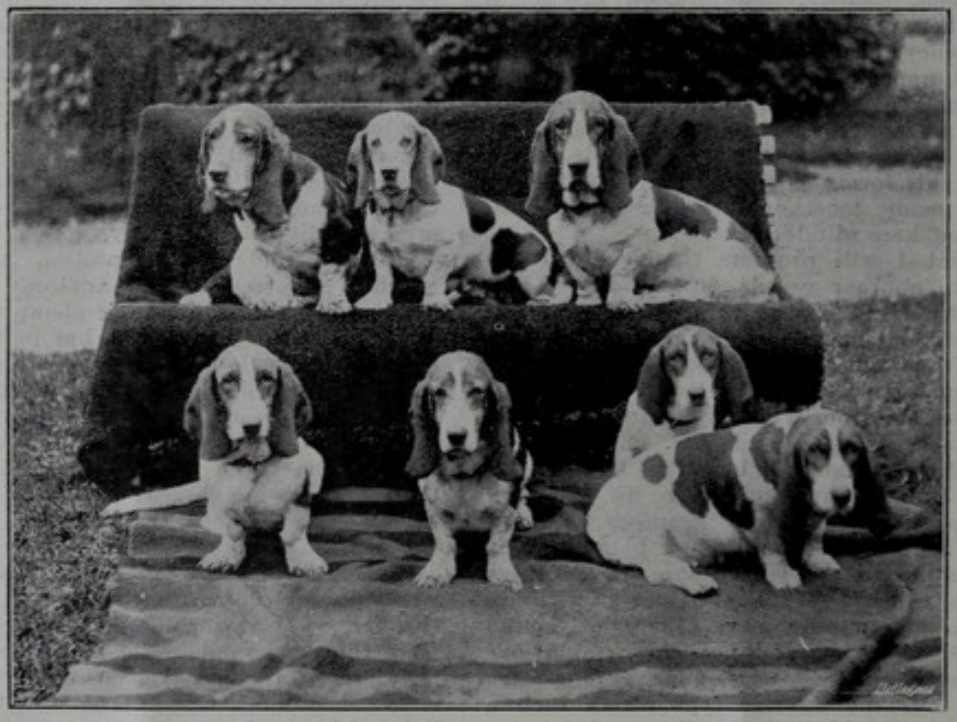
Now, when we come to consider the vast difference in height which has arisen between the Basset and the Bloodhound by man's process of selection, it must be obvious to you that, notwithstanding the natural instincts of the two varieties to form alliances at the proper periods, this selective process has all but made such alliances impossible.

A natural alliance can result if we make use of a Basset that stands very high on its legs; that is, a Basset of bad type, and a diminutive or ricketty Bloodhound, and this has been accomplished in one instance by my friend, Mr. Marsden, of Leeds, within the last two months. But I consider that in such a case (the bitch employed was a diminutive weed) we sacrifice the very point which we wish to attain, namely, size; and if we breed from a ricketty subject we lay ourselves open to ricketty offspring, on the ground that rickets is not only common in the hound varieties but hereditary, especially so through the female.

The result is, therefore, that if we desire size, and do not rely on the off-chance of atavism, we must employ other means than natural to obtain our ends.

Now the photograph which I now throw on the screen (Phot. II.), and which I regret to say is not a very good one, is, however, sufficient to show you the impossibility of an alliance between the two animals here represented; and yet such was obtained by injecting the large female with seminal fluid obtained from the diminutive male which you have seen here to-night, and the experiment which I allude to by which two males were the parents of different offspring in the same litter, was successfully accomplished by making use of this method and immediately having the female naturally served by one of her own breed.

In this case the bitch gave birth to eight pure Bloodhounds and three half-bred Bassets, and should any of you here to-night be curious as to the method by which the operation is performed, I will refer you to my first reported case, which is to be found in the April number of the *Veterinary Journal* of 1884.



A Group of Artesian Bassets (Mrs Walsh's).
Photo. I.

In 1892 I repeated the same experiment on the same two Hounds which you have seen on the screen, as well as on another Bloodhound bitch, in both cases successfully; but unfortunately with fatal results to the first-named, and as it may be a question of some interest to you as medical men I will give more than a passing word to it.

There must be many of you here present who have met with, and many who will meet with, a fever which follows childbirth. In dogs, we, as breeders, have an almost similar fever, but the puerperal fever of the dog differs from that of the human subject, inasmuch as it immediately precedes parturition, and does not or rarely follows as a sequæla.

It is always accompanied by the presence of one or more dead fœtuses, and I have never yet been able to save a bitch when she has been once attacked by it, for all pains immediately cease and the bitch rarely survives two days, although up to a few days before her date of parturition she has been in apparently perfect health.

and found that she had become attacked. This was demonstrated at once through the fact that there was the horrible odour which accompanies the disease, and the additional fact that labour pains had ceased, and that the bitch was weak, ill, and cold.

I at once examined her, and found a huge dead fœtus, which broke away every time it was seized by the forceps, and continued to do so, notwithstanding that I had the assistance of Mr. Slocock, of Hounslow.

By the afternoon it was evident that the bitch would not live through the night; consequently, as a last resort, we chloroformed her, and extracted the living fœtuses by Cæsarian section, and I believe I am correct in saying that the seven we succeeded in rearing to adult life—one of which you have seen here to-night—is the first instance on record of any of the higher mammalia being produced artificially and delivered artificially.

I must, however, pass from these interesting questions, and come to one which is more nearly related

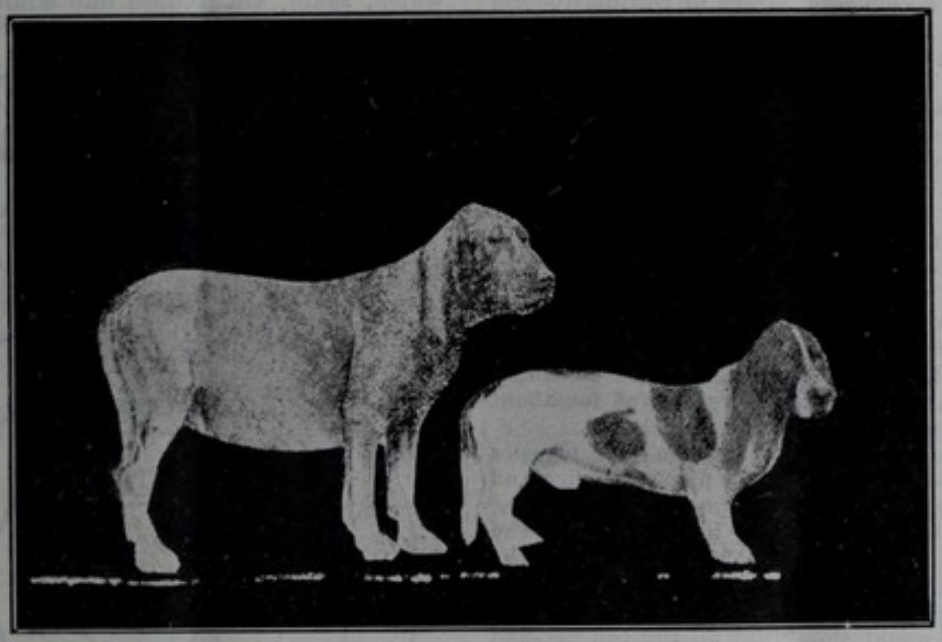


PHOTO. II

I have, however, been very successful in saving the living puppies when I have been able to remove them by simulating labour pains and using forceps; but when, gentlemen, we are unable to do this, we must either lose the bitch and the living puppies or resort to Cæsarian section to save the living progeny, and in the case of the dam bring its sufferings to a close.

In the case of the bitch to which I refer, I was exceedingly anxious, as her time for parturition approached, for I had already lost within the space of three months no less than five bitches from this disease alone; but when I saw her making preparations for pupping, and she seemed perfectly well, I went to bed satisfied that all would be well, and I was doubly assured that this would be so when my kennelman called me at three in the morning to say that she had given birth to a pup.

I make it a rule never to bother my bitches if things are going as they should go, so the bitch was left quiet until eight in the morning, when I entered her kennel

to those which I have to bring before you this evening, namely, had we any grounds on which we could form a correct opinion as to what these half-bred puppies would be like? For they, together with those I have alluded to before, were the first ever produced between these two varieties of hounds, and we had no precedent in such a cross.

In other words, would they be influenced by the sire, or would they be influenced by the dam?

This brings me to one of two questions I have to put before you to-night, namely, "What is the influence of the sire?"

Now, as regards this question, it would be well at the outset if I placed before you the views of the late Mr. James Howard, a gentleman who lived a certain number of years ago, and who was not only a well-known horse breeder, but a very keen and careful observer of facts bearing upon the question of influence as regards reproduction.

With the views he puts forward I am in perfect accord, but there is one which does not deal with

the question of the sire, with which I totally disagree, and which I will speak of anon.

Let me, however, deal with those which I consider correct. He states that

1. From the male parent is mainly derived the external structure, configuration, and outward characteristics; also the locomotive system or development.
2. From the female parent is derived the internal structure, the vital organs, and in more proportion than from the male the constitution, temper, and habits.
3. That the purer the race of the parent the more certainty there is of its transmitting its qualities to the offspring. The parent of the purest descent will have the greatest influence.
4. That, apart from certain disturbing influences the male, if of pure descent and descended from a stock of uniform colour, will stamp his colour on his offspring.

Such views as Mr. Howard's can be brought into a much narrower focus than he places them in, and were I explaining as I do now his views to a medical audience I should say that the sire influences the epiblastic and mesoblastic layers of the blastoderm and the female the hypoblastic; while, on the other hand, if I were to explain the question to a lay audience, I should say that the sire influences what we can see, that is, the colour and anatomy, and the female what we cannot see, *i.e.*, the internal organs, excepting where the female is a better-bred animal than the male, and where certain disturbing influences do not act against the reproduction of colour.

I have said that I am in accord with the views of the late Mr. Howard on this question; but as this does not prove these views to be correct, I think I might devote a few moments to a consideration of the question.

At the late Liverpool Show, held last month, I observed that about 210 classes were given for about 24 varieties of pigeons.

Now you must be aware that the late Charles Darwin, assisted by that eminent pigeon fancier and naturalist, the present Mr. Tegetmeier, proved most incontestably, by experimental breeding, that the whole of these varieties were but variations of the common blue rock pigeon.

In like manner, could we but lengthen our days and experiment, we might show that all the canines had a common ancestor in a wild dog.

In fact, I go so far as to believe that most of the bacteria which give rise to our zymotic diseases have a common origin, and finally that all life, whether it be vegetable or animal, has a common origin in a single cell, and that everything that we see around us which exists as living matter is but a variation of that original cell brought about up to the evolution of thinking man by natural selection and adaptation, and after him by, in a large measure, human agency, which at once accounts for the profusion of variation in those species man has taken into domestication compared with the same forms under natural conditions.

Now, gentlemen, it would be presumptuous on my part were I to attempt to picture the character of the first form of life. I may, however, put forward the view that it must of necessity have been aquatic in nature, but being the first of all it must also of necessity have represented in its person a whole species, and the many species which in due time would be evolved by natural selection and adaptation; and this being the case, my first duty on this question is to say a few words regarding the composition of a species.

If we take, as I have pointed out, what constituted original life, we have a species represented by a single individual, that is to say, represented in its lowest form.

We do not know of such a species nowadays, nor do we know of one which is represented by a number of individuals belonging to a family, in which case such a family would represent a species; but we do know of a strain composed of several families and represented by numerous individuals occupying the position of a species, and that is the extinct Dodo of the Mauritius.

In like manner we know of a variety composed of numerous strains, which are composed of families, and they, in their turn, composed of individuals representing a species; and this is to be found in the still living Platypus Paradoxus, or Ornitherhynchus, which I have myself had the pleasure of trapping.

But what we are most accustomed to, and what is a species in its full comprehension, is a huge collection of individuals, which are grouped into families, which, again, are grouped into strains, which again break up into separate varieties which form a species.

I am afraid, gentlemen, that what I have just put before you must remind you of a certain chapter in the Bible, which reads as follows:—"Who was the son of So-and-So, who was the son," &c., &c. I must apologise if the simile is correct; still, I will put it in another way.

Let A (Phot. III.) represent original life, the single individual, and let B represent a family derived from A, composed of a number of individuals.

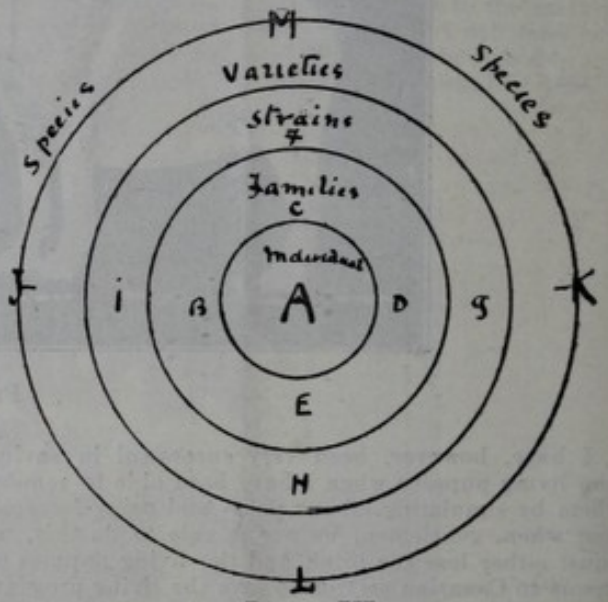


PHOTO. III.

In this case we have a species first represented by a single individual, and then by a family; but now let the individuals belonging to B multiply into families C, D, E; then we have the species represented by a strain composed of families B, C, D, E. And so it goes on into varieties F, G, H, I, and they into species J, K, L, M.

And what brings this about? Individuality and adaptation, and natural selection in the lower forms of life; individuality, adaptation, and artificial selection in the higher, where man has command.

And what happens during the rise from the single individual A to the species J, K, L, M? Why, while

they keep together as varieties they will all breed together without much difficulty, so long as variation does not hinder them from doing so. But when they have arrived at the outer circle they have variated so far apart that they have neither the power nor the inclination to do so; and as an example of such, let us look upon A as the Eohippus. Why, gentlemen, he has worked himself up into the hippopotamus, the tapir, and the common horse, to say nothing of other species; and I leave you to say whether even Marion Sims' operation would ever bring them together again.

Again, just on the borderland of true species stand the horse and the donkey, also derived from the Eohippus, and although they will breed the progeny are barren; but all the varieties of the horses and all the varieties of the asses are fertile.

Just one more question on this chart, and I have done. You often hear people say, "As like as two peas." Gentlemen, no two peas are alike, no two leaves, no two anything on the face of this earth, and consequently no two individuals.

Now, taking A again as a single individual, he would represent species type by his individual type. When he died all those descended from him would have A type as their species type, with their separate family types B, C, D, E, and their own individual types. Consequently, when we arrive at a complete species every individual of that species must not only have A species type, but its separate variety type, a strain type, family type, and an individual type.

Take, for instance, Champion Forester, whom you saw to-night. I should describe him as of the species dog, of the Basset variety, of the Couteulx strain, of the Fino de Paris family, and we know him to be the individual Forester, because of his individual type, which is different to that of any other Basset.

He is, therefore, an absolutely pure-bred Basset, of the highest breeding. A less pure-bred Basset would be one having a compound family type; a still less pure one, one with a compound strain type. Finally, we come to a mongrel, namely, one whose variety type is a mixture of a Basset and another breed, such as I showed you in the person of the half-bred Basset-Bloodhound.

Still, you must have observed that the half-bred which I showed you this evening takes clearly after the Basset anatomically; and, this being the case, we have, I think, to inquire the reason why, for it is not in accordance with Mr. James Howard's views, for the Bloodhound type is one of the most ancient of breeds, and it was not until long after they had reached a commanding position in canine life that man began to think of evolving Bassets.

To fully understand this question I cannot do better than to place before you a number of crosses, certain of which will be between animals not under domestication, and certain between animals that are not under domestication and those that are, and from these crosses, or illustrations, I hope to be able to show you that

1. That the sire has naturally a greater influence over the epiblast and mesoblast.
2. That this becomes increased when the male parent is a wild one, and the female a domestic one, and *vice versa*.
3. The reason for the latter.

INFLUENCE OF THE SIRE WHEN NOT UNDER DOMESTICATION.

To illustrate the greater influence of the sire when not under domestication I cannot do better than turn to my own brother's work on the Tetraonidæ, which you may perhaps be better acquainted with under the name of grouse.

This species inhabits all the Northern countries of Europe, Asia, and America, and in Great Britain we have four varieties, namely, the capercailzie, the blackcock and the greyhen, the grouse and the ptarmigan.

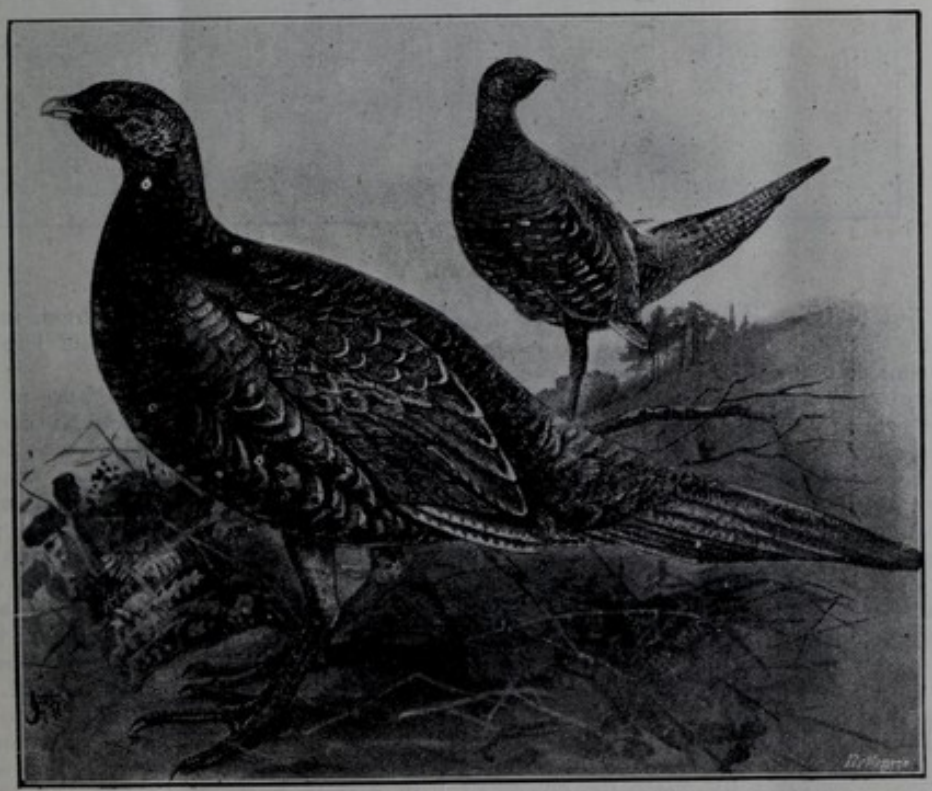


PHOTO. IV.

In the case of these varieties, I am enabled to place before you here to-night, through the kindness of my brother, Mr. J. G. Millais, and his publishers, Messrs. Sotheran and Co., of Piccadilly, W., all the known hybrids between the British Tetraonidæ and other varieties, apparently not very widely related from them; and in each case I think the influence of the male will be clearly demonstrated.

Now, the first which I show you (Phot. IV.) is that between the cock pheasant and the capercailzie hen, and the second (Phot. V.) is that between the cock pheasant and the greyhen, and I will ask you to observe that, with the exception of the feet, the preponderance of the male parent is most marked when one comes to consider what the types of the capercailzie and the blackcock are. In the case of the blackcock and the female capercailzie (Plate VI.), the hybridism is not so marked in the females as the males; but in the male hybrid it is most distinct, especially in the colouring and feathering, and in the formation of the tail, for, as you are doubtless aware, the blackcock has a typical tail somewhat like two of the Prince of Wales feathers, and the capercailzie has a fan-shaped one; this peculiarity is also to be noted in the under feathers of the female hybrid, for while in the capercailzie the feathers come to a single point and in the grey hen to two points, in the hybrid there are also two.

Exactly the same influence is to be noted when the blackcock mates with the grouse; for, as you are aware, the tail in the grouse is also fan-shaped, but in the hybrid it approaches that of the male parent, both in form and colouring. Such an instance is to be seen in this photograph (Phot. VII.).

There is also the best of evidence in favour of hybridisation between the grouse and the ptarmigan, but as the grouse are subject, like other life forms, to albinism, I do not give an example, since such hybrids might be said to be albinistic specimens of the grouse.

What occurs in the Tetraonidæ likewise occurs in the duck varieties, and had I time this evening I could give such instances as are known; but should you care to look into this question I believe you will find my list in Dr. Perry's, of Boston, Mass., work on the "Dog," when speaking on the subject of crossing.

All these hybrids are naturally exceedingly rare, and one of them which I propose to show you now is one of the only two known. This is the cross between the bantam cock (Phot. VIII.), one of man's varieties, and the grouse; and what I wish to demonstrate in this case is that when the sire is a new variety in comparison with that of the dam, the hybrid takes after the dam to a greater extent than the male parent, and this, you will observe,

PHOTO. V.

in the feathering of the hybrid, in the shape of its feet, in the feathering of its legs; in fact, in its general appearance.

I show this specimen because it is the obverse to what is usually to be found in Nature, no matter where we look. Consequently, we may take it for granted that under natural conditions the influence of the sire is greater than that of the dam.

Now, such a case as I have just placed before you was never intended by man to take place. It was an accidental cross which might not occur for generations and generations; but when we turn to crosses between animals and birds, where it is the intention of man to produce hybrids, we observe that the influence of the male is paramount.

Instances of such hybrids are to be found in the common mule which we occasionally see in this country in our streets, but which is common enough in certain parts of France and the United States,

PHOTO. VI.

where the donkey is the sire and the mare the dam.

I am sorry to say that most of our mules are of a very degenerate character, simply because the sires used are degenerate specimens of their race. Abroad, and in America, it is not so; but whether they be degenerate or not the mule takes after the male parent, not the dam.

So it is in the case of birds. Here we have many hybrids between the various finches and the canary, and in every case the identity of the male parent can be seen almost at a glance.

In Paris crossing is undertaken to a large extent in the Jardin d'Acclimatation. From personal observation of a large number of specimens bred there not necessarily mules, I think I may safely say that the influence of the sire is predominant.

This, gentlemen, brings me to the results which we obtain when we cross the wild animals with the domestic.

As a rule, the produce take invariably after the wild animal or bird, whether the wild animal be male or female.

PHOTO. VII.

Of the latter I have afforded you a striking example already; but again, as a rule, the produce take much more after the wild sire than the wild dam.

Some of my earliest experiments were made in crossing the female white mouse and the female white rat with wild sires, to say nothing of rabbits and the usual menagerie that a boy has when at school and when at home for the holidays.

PHOTO. VIII.

I believe if my memory serves me aright that I began this hobby at the early age of eight, and I am afraid that the love which I and my brothers have always had for natural history sadly interfered with my father's comfort at home.

I used to have sometimes no less than fifty mice in my small London bedroom, which my parents strongly objected to, but I was not interfered with until, by accident, one day my collection of large spiders got loose in my father's bedroom, and during the night spun huge webs from the ceiling to the windows, and, in fact, wherever a spider found herself comfortable.

This my father could not stand, so my menageries were banished. At the same time, I managed to carry them on elsewhere, and I managed over and over again to breed tame brown mice and rats this way.

If there is one thing I regret more than another, it was the destruction, by one of my masters, of ova, which I had obtained from the common tiger moth, which I crossed with the common silkworm male, and which were consigned to the flames in the most ruthless manner because I was counting the ova in school hours.

I believe that this cross has since been obtained, but I consider I am really entitled to the credit of it.

But to resume the question of the influence of the male when the female is of a more domestic character than he is, one of the most notable examples is that of the first foal out of Lord Morton's mare, which I shall have to refer to later on. This mare was put, as an experiment, to a quagga, and the produce was a foal marked like a quagga.

Again, the wolf and the dog not unfrequently breed together, and the fox and the dog, and in such cases the produce is, if a suitable bitch, such as a Collie or a Pomeranian, be mated with them, so like the wild animal as to be nearly indistinguishable.

Capt. Wiggins, to whom I applied for information on these points, gave me numerous examples of such crosses, which he had observed when making his way up the Yenesei, in Siberia, to Yeneseisk.

I might quote numerous instances from the reports of the Howietown fish breeding establishment founded by Sir James Gibson-Maitland, where numerous experiments have been carried on, not only between the Salmonidæ of this country but between those indigenous to Europe and America; but I think if we wish to see good instances they are to be observed in the human races.

I have had, during my wanderings round the world, great facilities of observation, and I have remarked that if the male parent be a Mongol, a Polynesian, a Red Indian, or a Negro, the white woman's child will resemble the sire to a much greater extent than where the white man is the father of the dark woman's child.

Both take after the more ancient race; that is, the dark race in features, hair, and anatomy; but where the sire is of the dark blood to a much greater extent, and no greater proof of this is to be found than what a brief sojourn in the Southern States of America will impress on us.

There, if you take the half-caste and mate her with the white man, it takes some five generations before you can breed out the negro's blood.

On the other hand, if you mate the half-caste with the negro, the child born of such an alliance returns to the black race at once.

Now, gentlemen, what is the cause of the wild sire producing young more like himself than the domestic female, and why, when we come to deal with human beings do we find a similar condition of things?

There is but one answer to this, whether it has to do with man or the lower animals, and that is, that the domestic animals and the white man—that is, the higher types of man—are of newer creation or evolution than the wild or dark; consequently their type is less fixed than the older ones, and when they by any circumstances are bred together the newer type goes down before the old.

Now, gentlemen, I need hardly say to you that if a man wishes to succeed as a breeder, or, rather, as a cross-breeder, with views such as I entertained when crossing the Basset with the Bloodhound, he must be, to a certain extent, conversant with these questions, otherwise he will not only go hopelessly astray in his expectations, but is not unlikely to do infinite damage to the breed he is experimenting upon, which can only be remedied by years of patience.

Take, for instance, the case of a man who has some weedy Deerhounds, of which he knows nothing whatever. All he knows is that they are weedy, and rather than out-breed to a better kennel, he breeds one of them to a big Bob-tailed mongrel, hoping by inbreeding again to the Deerhound that he will give increased size to his animals.

Here is the result of such an experiment (Phot. IX.), namely, an absolute mongrel, taking, in some respects, after the sire, and others, including the Bob-tail, after the dam.

In the language of ancient Rome, I may perhaps be allowed to remark, "Nescit ubi sit."

PHOTO. IX.

PHOTO. X.

As a contrast to this, I will now show what he might have obtained had his animal been properly bred to start with, I mean his Deerhound.

Here is, apparently, a Great Dane (Phot. X.), yet its dam was a rough-coated St. Bernard. Again, here is a Buff Leghorn cock, a Buff Leghorn hen, and a brown Leghorn hen (Phot. XI.), the sire being the cock, and the dam the brown Leghorn, and regarding questions of this nature, I may remark that, in fowls, as elsewhere, when the sire is properly bred, the produce almost invariably take after him.

Finally, I give an illustration in pigeons, where the sire is the Baldy, and the dam the Fantail (Phot. XII.). The young on the top of the cote will be seen to take after the sire.

To show in this case how very strong the influence of the male is, I may say that the Baldy has only 12 or 14 tail feathers, the Fantail 30 to 40. In the produce we find 14.

In like manner, gentlemen, if the female be better bred than the sire, the offspring generally take more after her than the sire, but as I have previously remarked, not to the same extent, for the influence of the sire is naturally greater than that of the dam, and this fact at once accounts for the habit which breeders have of putting an inferior dam to a superior sire, and not *vice versa*, for, on the one hand, the superior sire raises his offspring to a better type than that of the dam, and where an inferior sire is made use of, he rather degrades the type of the superior dam, to his own.

In both cases, then, gentlemen, whether the sire and dam be wild, or whether the sire be wild and the dam domestic, we find a greater influence on the sire's side than the dam's, and *vice versa*, as when the dam is wild or better bred. And this brings me to the question of what is a better bred animal under domestication, whether the animal be male or female.

A little while ago I mentioned to you that an animal might be highly bred, well bred, and badly bred, and yet be a pure bred animal, but it depended largely as to how the animal was bred what offspring would be born to him or her.

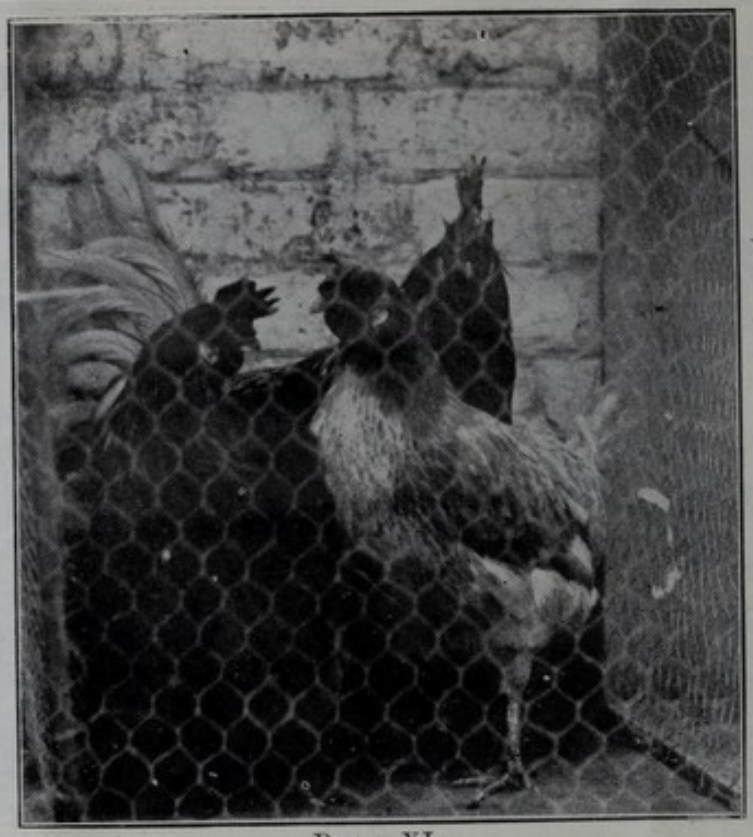


PHOTO. XI.

Now, to understand this question, I will presume that a gentleman sees an advertisement, say, in the *Field*, that a certain Pointer is for sale, and he is desirous of buying one for the purpose of improving his type.

Now, not knowing much about Pointers, excepting from a sporting point of view, he buys this Pointer principally owing to the fact that its pedigree is warranted, and that it contains the blood of animals bred by a number of noblemen and gentlemen.

This sounds good enough, and supposing that the following is the pedigree he receives with the animal, I ask if there is any way by which he can find out whether the animal is likely or not to have the required influence.

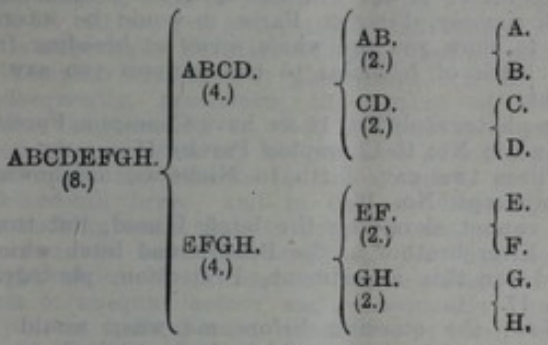
ROVER.	Ponto II.	Ponto.	Dick.	Property of the late Duke of Wellington.
			Peerless.	Ditto.
		Meg.	Rag.	Property of Baron Rothschild.
			Rachel.	
	Betty.	Shot.	Dan.	Bred by Lord Lindsey.
			Breeze.	
		Daisy	Crib.	Bred by the Earl of Devon.
			Careless.	

PHOTO. XII.

Well, gentlemen, whenever I see a lot of the English nobility in a dog's pedigree I have somehow, perhaps from experience, a shrewd feeling that the pedigree is better than the dog, and such is the case which I now put before you.

The true value of a pedigree may not inaptly be likened to the sounds we are accustomed to hear in cardiac or pulmonary mischief, for, *per se*, the pedigree and the sounds are worthless were it not for the information they both afford.

In the case of disease such as I have mentioned, we submit the patient to examination by the stethescope, but in the case of the pedigree we do it by reduction.



Now, presuming the pedigree of the Pointer to be as I show it on the board, and presuming his name to be Rover, you will see below his pedigree, the reduced pedigree, showing him to be a unit composed of 8-8ths, and we arrive at that conclusion by underlining all the stops, that is to say, wherever the pedigree stops, which in this case are grand-parents, each receive their proper value in his composition, viz., an eighth each.

Rover, therefore, is a unit composed of 8-8ths, none of which have a greater value in his composition than another, and for this reason he is unable to influence his progeny in any way whatever, unless the dam he is mated with is worse in this respect than himself, consequently he is absolutely useless as a type producer.

Such an animal is, therefore, bred upon an equal factor system, and although such a system answers well in dogs as it does in human beings, and produces constitutions of great vigour and intelligence, for practical purposes, such as breeding to type, it must be put aside and a more suitable one found.

Now the system that we find most suitable for the production of a type producer is an unequal system of factors, that is to say, a unit composed of several factors of small value, and a big one which supplies the types. In other words, gentlemen, we inbreed to one animal until we set a race going, in which an original animal is the predominant factor, or, in other words, gentlemen, we commence with him just as you saw "A" in original life, and although we cannot raise him into a species, we can, after a few years, almost approach a new variety, the type of this new variety being that of his individual characteristics.

Thus the Basset of to-day in England is practically a new variety founded on the individual characteristics of a Hound, named Fino de Paris, which used to stand at stud in the Jardin D'Acclimatation, in Paris.

This Hound was supposed to be the finest Couteulx Basset that ever existed, and some time after he was imported into this country, it was most amusing to hear how sorry the French were that they had parted with him, and how much they wished they could get him back again, but nowadays the complaint is that we have changed the type of the French Basset.

Gentlemen, we have not changed the French Basset in any manner whatever; we have made a variety, founded on their best, hence the charge.

In like manner I have been accused by the Germans with helping to change the Dachshund, but the charge is almost as puerile as that of changing the Basset.

I was one of the first to import the Dachshund into this country, and all that I and others have done has been to keep on breeding to the type we acquired from the King of Hanover's kennels, and this reminds me of a most amusing episode.

I judged the Dachshunds, which are the German Bassets, at the Jubilee Show, and to increase the interest in the show classes were given for German Dachshunds.

In the classes given for German Dachshunds a large number turned up, and when these were compared with the English classes, the result was ridiculous.

On the English side, every Hound was of the same type and the same size, but on the German, every animal presented a different type and a different size, while some were smooth-coated and others rough, and, to crown all, every German exhibitor submitted to me, not privately, but most ostentatiously, that his exhibit was the correct type.

Now, gentlemen, to all intents and purposes, type is what we make it, and to have a show of animals all of similar type means that all must be animated with the same views of type, and this is what makes us Englishmen a household word wherever the breeding of animals is concerned. We take a specimen from one country, and we say this is what we must breed to, and we do so, whereas abroad no one appears to agree to type unless it is an English breed, and there is no one to put forward private views.

Now, if we take the best specimen which France can produce, and are able, as at the last Crystal Palace Show, to put on the bench some fifty Hounds of the same type as the one we first imported, by continual inbreeding and selection, we simply copy the method by which Nature has made her types, and that this is by inbreeding and selection we can prove to a certainty, because wherever we go where natural barriers exist to the importation of fresh blood, &c., we find definite types which reproduce themselves to an enormous extent in the same way as wild sires do when mated with domestic females, and *vice versa*, &c., and as instances of these I have only to point to the island populations in the Pacific, where the type of man is as definite as the variation of the Polynesian language he employs, to the cattle of the Channel Islands, to the ponies of Shetland and Iceland, to the cats of the Isle of Man, and to many others in the lower life-forms which I might name.

All this, gentlemen, is the result of inbreeding, and I cannot give you a better example of it than the type presented by our own royalties.

The breeding of an animal and a language has much in common. The English people, as a rule, do not reproduce their type, for they are a cross-bred people, as their language is, but where we find a typical people we find a typical language; and in addition to this we find that such a race has kept to itself. As an example of such a race I may name the Jews.

The Jew will always reproduce himself, and although they have not grossly interbred, they have done so sufficiently to procure for themselves an influence which no white Christian nation can upset.

Time, gentlemen, will not allow us to work as slowly for the production of the same type producer as it has taken to produce the same stamp of animal in the Channel Islands or the Jew, consequently to produce the same powerful animal we must inbreed more grossly and more consistently, and that we have done so I can show you in the pedigrees of two of the Bassets I have shown you this evening.

Here (Phot. XIII.) you will observe that, while under the equal factor system, Ch. Forester and Nicholas ought to have had respectively 64 and 128 ancestors, they have only seven and eight, and although they are again seven and eight generations removed from their prototype, Fino de Paris, they contain almost half of his blood.

Such animals, gentlemen, are not only well-bred animals, but they exemplify, in the fullest sense of the word, pedigree animals, for they not only have a pedigree, but they possess the attribute which only a pedigree animal has, i.e., the power of reproducing its own type in whatsoever female it is mated with.

This, gentlemen, is the value of a pedigree animal, and this is why the enormous sums which surprise the ordinary mind are given for them.

This, gentlemen, is the true explanation of Mr. Howard's views, which are correct, and if, as I hope, you have understood my rendering of them, I will now proceed to show you the breeding of the generations of animals I showed you at the commencement of this evening.

Now, as I have previously pointed out to you at the commencement of my words this evening, my object was at once to increase the size of the Basset which, through inbreeding, had commenced to show signs of degeneration, and with that object I chose to make use of the Bloodhound as the vehicle.

Again, as I wished to bring in this fresh blood rapidly, I was obliged to make use of a pedigree Hound, namely, Nicholas, not because this was the only Hound I had, but because I wished to breed up a family on the individual characteristics of his sire, Champion Forester.

Had I used Forester first, and then Nicholas, it would have taken four generations to complete my work, but by using Nicholas first, I found that I could do it in three, consequently I began with him, and since both Nicholas and his sire contain a large factor of the original Hound we commenced with namely, Fino de Paris, it would be interesting to show you the whole series of breeding from the birth of Nicholas to the puppies you saw to-night.

In photograph No. 14 we have Champion Forester.

And in No. 15 Champion Psyche II.

These two gave birth to Nicholas, as shown in photograph No. 16.

I cannot show you the bitch I used, but this is the litter brother to the Bloodhound bitch which I used for this experiment, Protection, photograph No. 17.

Now, the question before me was, would the enormous inbreeding which had resulted in the birth of Nicholas be sufficient to break down the type of the Bloodhound bitch used? I say this because the Bloodhound bitch used was almost, though not so grossly, inbred as the proposed sire.

My opinion was that in anatomy we should get that of the Basset, but I was not certain as to what the colour would be, but I felt certain that this would come in the third generation.

As a result, I artificially bred the half-breds and brought them into the world as I have explained, and I now give two photographs of them, the first being that of a bitch, the second that of a dog, and I will here ask you to bear in mind that all these photographs were taken at the same distance in order that the exact difference in size and type between the Bassets already shown and the half-breds might be demonstrated.

Here is the half bred bitch shown to-night, Ada, photograph No. 18.

And No. 19 is the male half-bred, Cromwell.

As you observe, both took after the Basset in anatomy, but although the Bassets are descended from a race of uniform colour, their colour was not reproduced in the first cross, because, as Mr. Howard remarks, something may interfere to stop it, and the something in this case was the inbred character of the Bloodhound bitch employed.

XIII

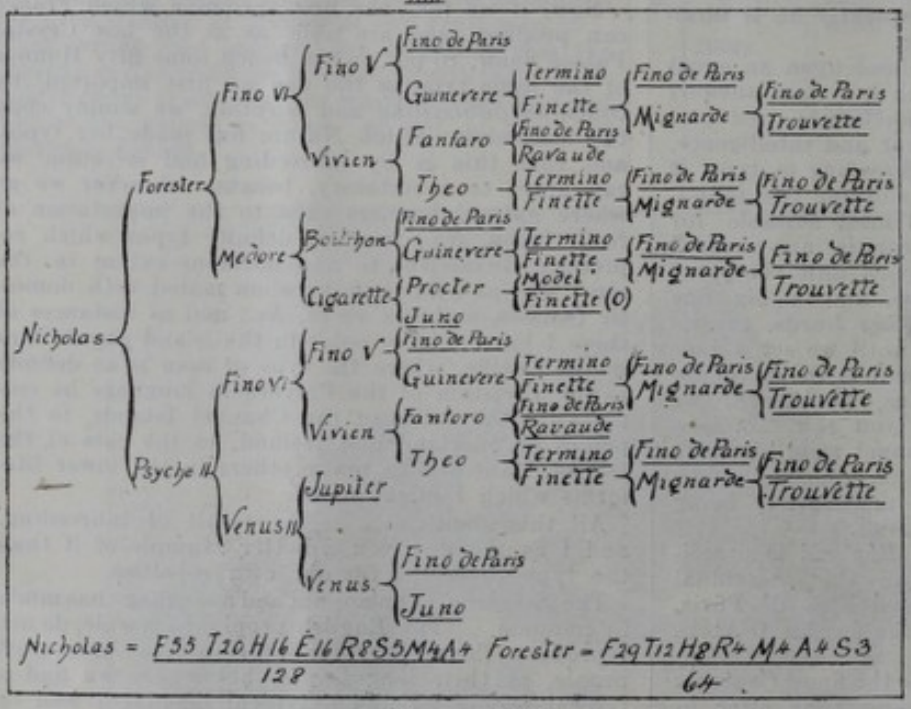


Photo. XIII.

But in the second cross, when a Basset was again used, namely, Champion Forester and Cupid II., we gained the colour desired; for in every case where the half-bred bitches were bred to Bassets we got a superfluity of Tricolors and only a few black-and-tans.*

Here are photographs of the produce, which, as you will have observed, are not only Bassets in colour but anatomy.

In photograph No. 20 we have the half-bred Ada's pups, and

In No. 21 the half-bred Rickey's pups.

Breeding, however, from the half-bred male with Basset bitches, such as this, viz., Juno IV., photograph No. 22, we get, owing to the influence of the sire, a superfluity of black-and-tan Bassets, and I can exemplify this best by photograph No. 23.

Consequently, gentlemen, the upshot of these breeding experiments is, that we have not only gained what we desired, but we have added a new variation to the Bassets in this country, viz., a black-and-tan breed, and in this we have again done nothing more than to copy Nature.

As you will have perceived, all this has been brought about by inbreeding, or breeding on a system of unequal factors, and consequently I may ask you, as medical men, whether it is likely that Nature, who has her own way of controlling inbreeding, will accept such gross liberties with her prerogative as we breeders take?

No, gentlemen, Nature rebels very considerably against our methods, and for producing type as we do she exacts a very heavy price.

I think if I quote correctly from notes, that Dr. Payne, of this hospital, states that certain families in the human subject are not only more prone to zymotic disease, but more deleteriously affected than other families, and he especially refers to inter-marriage as a direct cause.

Dr. Payne, from my point of view, is absolutely correct, for without referring to the disasters that have brought grief and trouble into the homes of our own royalty, I can give such instances of a mild zymotic disease causing widespread calamity in an inbred nation, such as I have myself observed in the Polynesian Islands, while nearer to home both phthisis and cancer are notorious in the Channel Islands.

It is the same, gentlemen, in the inbred canine races, for here we find, and I speak from 20 years' experience, distemper carries off about 60 to 70 per cent. of those attacked, and of those that succumb more than 80 per cent. owe their death to the most common sequaelæ, pneumonia, while the rest go off from enteritis, jaundice, and fits.

In addition to these graver complaints, we also find, following zymotic disease of this nature, such sequelœ as paralysis, partial and complete, fits, and persistent chorea, just as they are found to follow zymotic disease in the human subject.

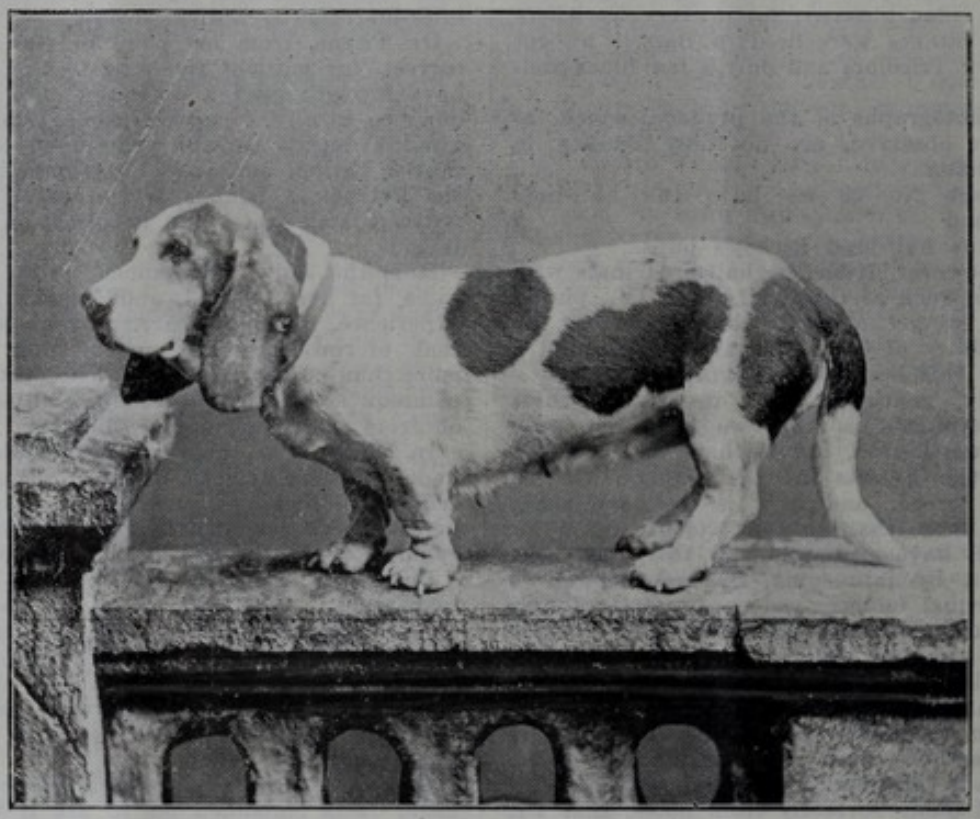
But is this all? No, gentlemen, in our endeavour to reproduce type, not simply by the relying only on the influence of a sire, but by selection of dams fit to mate with him for this purpose, we too frequently encourage an eczematous diathesis, and bring about hereditary deformity, which is to be found in the Basset's tail, such as I showed you this evening.

Rickets, too, are alarmingly present, and these latter facts embolden me to ask if we do not see the results of selective breeding in this hospital, not once in a way, but day by day.

I venture to remark that a very large proportion of the cases treated here, not only in the out-patient department, but in the wards, are the

*N.B.—Mr. Milla's' views have been strikingly corroborated in the result of another litter, got by Napoleon II., ex Bella (a half-bred), born since this lecture was delivered.

XIV.



XV.

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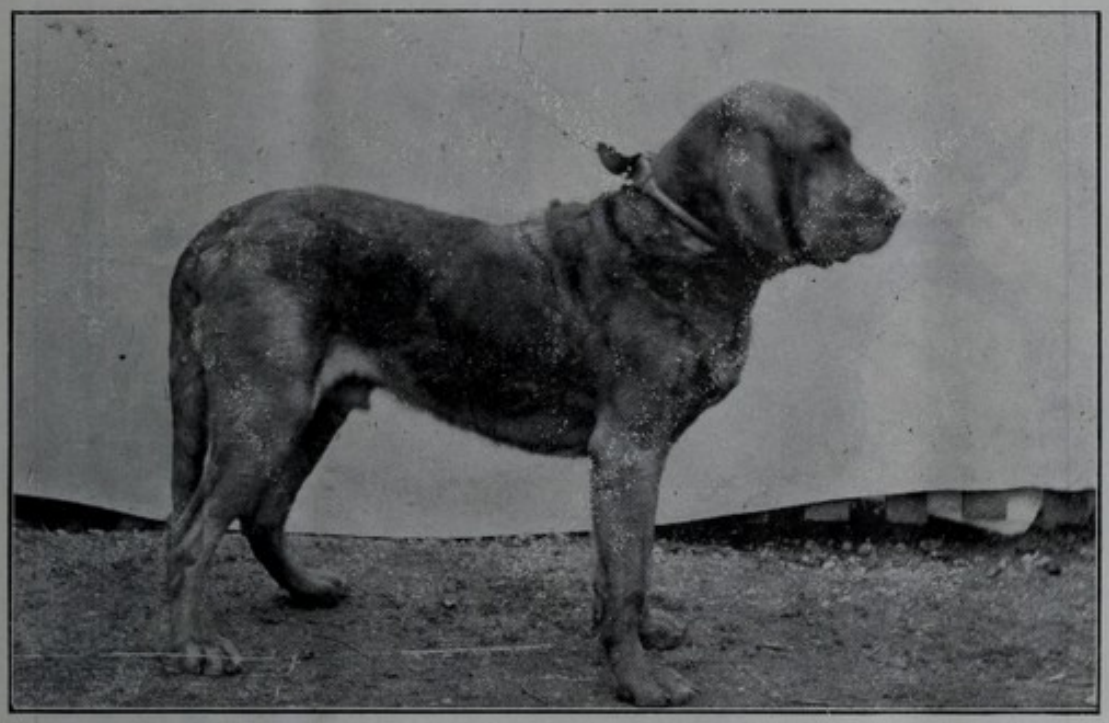
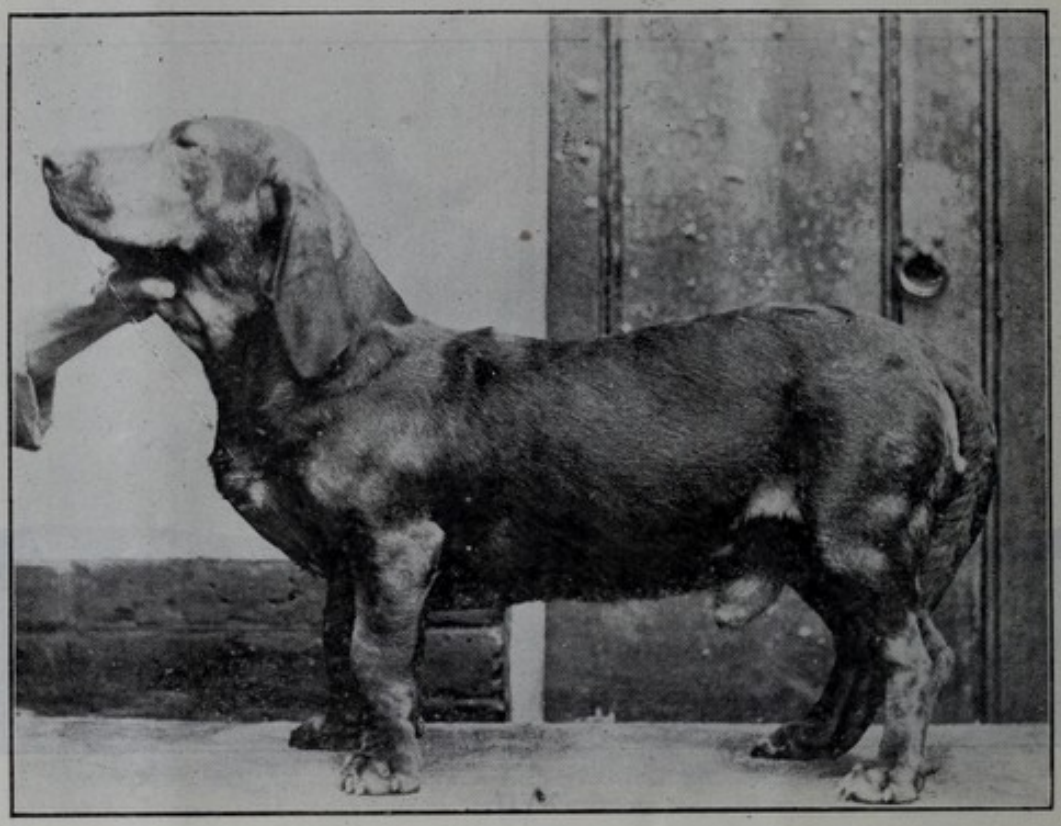


XVII.

XVIII.



XIX

result of unwitting selection on the part of the parents of these patients, and to any person interested in these matters, such as I am, I believe that St. Thomas' Hospital offers a field of enquiry of almost a virgin nature.

TELEGONY.

I remarked some time back when placing Mr. James Howard's views before you, that he added a fifth view, namely, that it was not uncommon, when a mare foaled, to see the influence, not of the horse by which her foal was sired, but that of a horse to whom she had previously cast a foal. This, gentlemen, is what is now known as Telegony, or the influence of the previous sire, and it is the second of the reproductive phenomena of which I intend to speak.

Now it is not my intention here to-night to argue whether Mr. Howard's views are correct from his own observations, because I believe that Mr. Howard left no recorded case of such a phenomenon in his own experience, consequently I can only come to the conclusion that he added this fifth view because of what he had been told by others or had read about; but I do mean to argue, from considerable experience on this question, that although Mr. Howard has a perfect right to say that telegony may occur, he had no right to say that it is of frequent occurrence, because it is not.

I will therefore ask you to bear with me for a few moments while I place before you the historical case on this question.

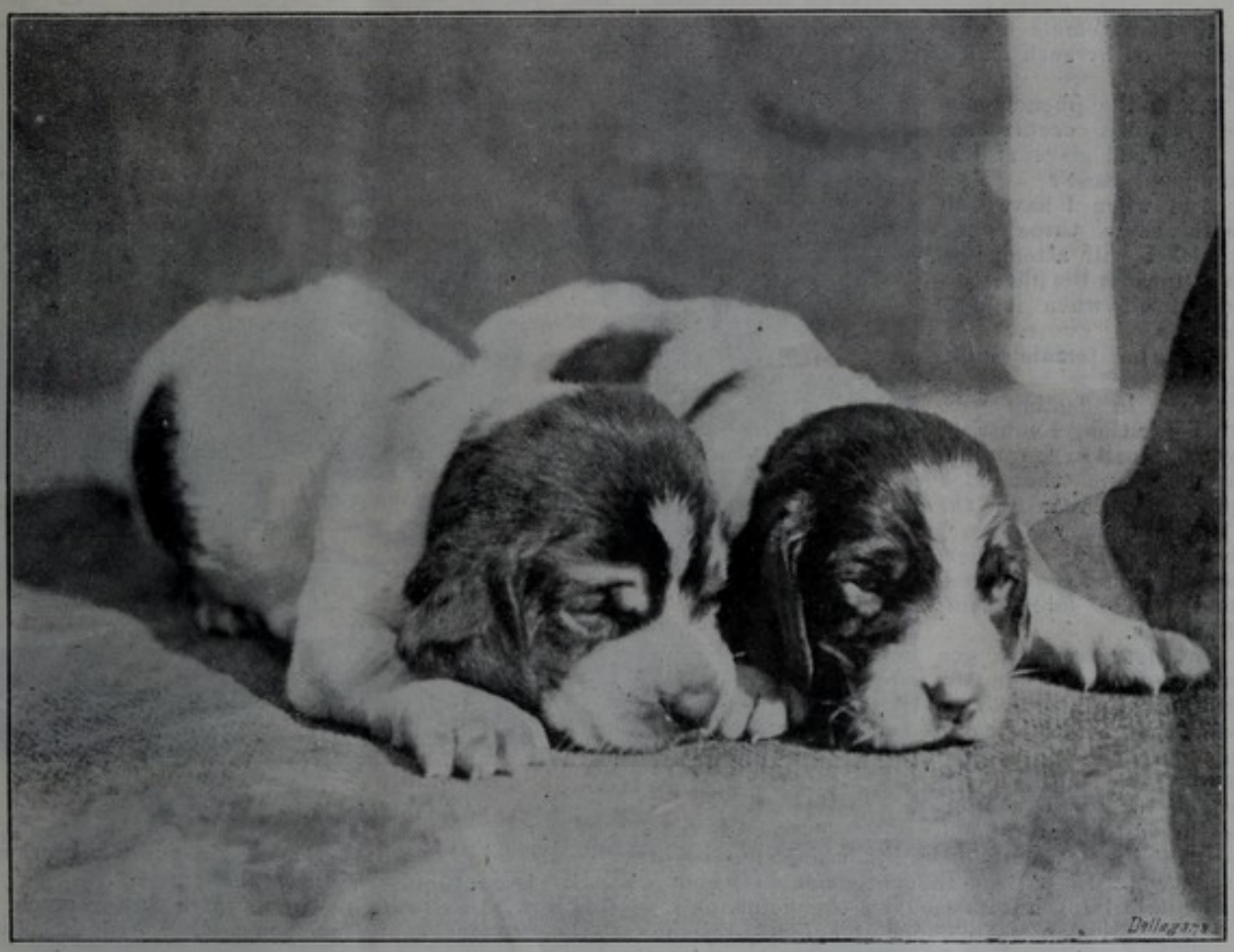
In 1820 the Earl of Morton brought the following matter before the notice of the Royal Society:—

A virgin mare of almost pure Arabian blood was crossed with a Quagga, and the result was a female Hybrid, which in her form and peculiar markings showed extensively the influence of the sire, which is usual when domesticated races are crossed with wild.

Lord Morton then sold the mare to Sir Gore Ousely, who bred from her by a black Arabian stallion, when the produce exhibited the influence of the Arabian in the mesoblastic structures, but in the epiblastic took after the previous sire, the Quagga.

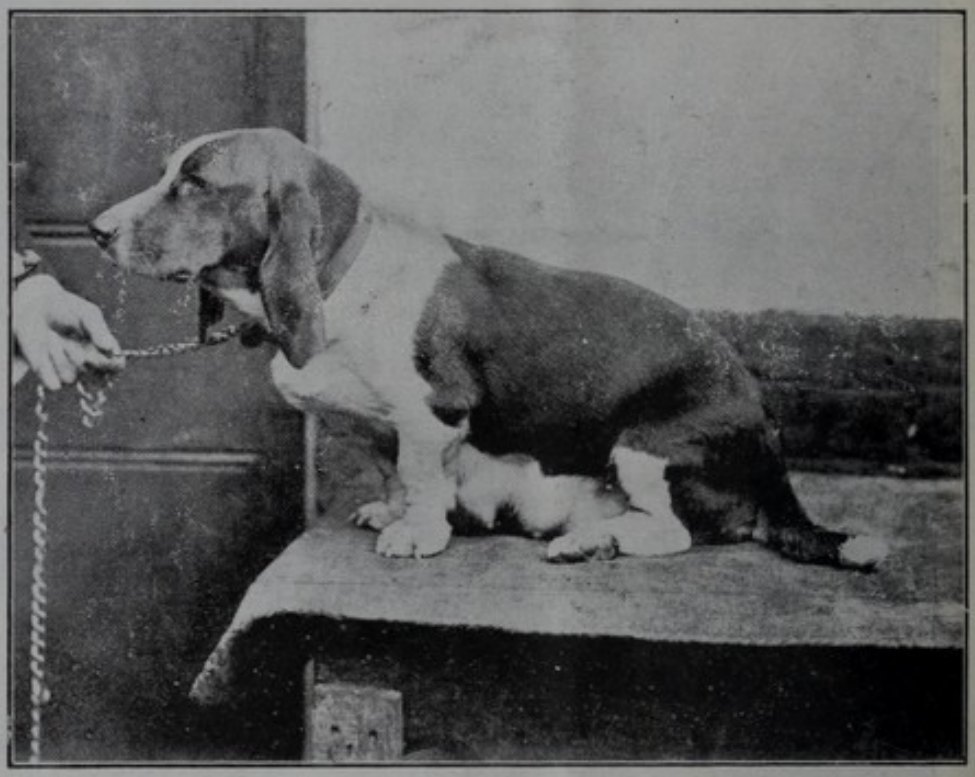
Now, gentlemen, I beg, in the first place, to state at once that I in no way doubt the truth of this communication to the Royal Society, but, in the second, I feel that we have here the key to Mr. Howard's fifth view, as also to the very general and accepted view held by many, that if a dam has been once served and borne offspring to an animal of different variety type to her own, or indeed a cur, she is for ever afterwards rendered useless as a pure breeder.

The questions, then, that I have to put before you this evening regarding this phenomenon are three in number, viz.:—



XX.

XXI.



XXII.

1. Is a female always influenced by such a mesalliance?
2. Is the phenomenon of frequent occurrence?
3. What gives rise to supposed cases?

And when I have dealt with these three questions, I will attempt to account for the phenomenon itself when it does occur.

1. Is a female influenced?

Now, in dealing with this question, I must ask you to recall to your minds a discussion that took place last year in the "Contemporary Review," between Mr. Herbert Spencer on the one hand, and the late Prof. Romanes.

In that discussion you will remember that Mr Herbert Spencer held the view that a female could not produce pure bred stock after she had borne young to a sire of a different breed to her own, and to still further emphasise his argument, he reproduced a letter from a gentleman to whom he had applied for information on the point.

This gentleman was a judge at agricultural shows (I know him personally), and one would naturally suppose that information would be forthcoming regarding horses, cattle, sheep, or pigs, but such was not the case, for the gentleman supplied Mr. Herbert Spencer with two cases in dogs, not, be it remembered, in his own experience, but in those of others.

He further made the assertion that he had asked various friends, and that their view was that telegony invariably resulted, as a sequel to a mesalliance.

The examples given to Mr. Herbert Spencer, and made public by him, were those of a Dachshund bitch and a Pointer bitch, which had strayed, and had never afterwards thrown pure bred puppies.

So far so good, but I was fortunately able to give authentic cases in which no such results had come about, not only in the same breeds as brought forward by Mr. Spencer, but in others, and in every case the dams were not only pure-bred bitches, but bitchesknown exceedingly well to the exhibitingpublic both here and in America. In each case, too, these bitches produced champion dogs, not one each, but a large number, after unquestionable mesalliances.

I may further adduce the fact that in a breeding experience of nearly 30 years' standing, during which I have made all sorts of experiments with pure-bred dams and wild sires, and returned them afterwards to pure sires of their own breeds, I have never seen a case of telegony, nor has my breeding stock suffered.

I may further adduce the fact that I have made over fifty experiments for Prof. Romanes to induce a case of telegony in a variety of animals, dogs, ducks, hens, pigeons, &c., and I have hopelessly failed, as has every single experimenter who has tried to produce the phenomenon.

Now, the next question which bears much on the first is: Is the phenomenon of frequent occurence?

Well, gentlemen, if I cannot produce it, and have never seen it in thirty years' work, and no one who has tried to do so has seen it or produced it either, I think you will agree with me that we have an answer to the two first questions, *i.e.*, that dams, as a rule, are not rendered useless by making mesalliances, and that the phenomenon is not only rare, but absolutely abnormal. But, gentlemen, if you require further evidence on this point you have it in the fact that owing to my connection with the press which deals with questions of breeding, I was asked to get information on the question from breeders by Prof. Romanes.

This I did not only in the English press, but the American and the continental, with the result that we got an enormous number of so-called cases of the phenomenon, which would not bear a critical inspection for a moment, but when we went to breeders who produce nothing but Hybrids such as the great mule breeders (animals and birds), we got the information that after breeding mules no case was on record where telegony was observed when the dams were put to their own type of sires.

Evidence such as this, gentlemen, at once does away with Mr. Spencer's views, and those of his friends, but it does not account for the many so-called instances that are brought before our notice. With this I propose now to deal.

A very large proportion of these cases which we get, and which are constantly brought before our notice, are reported by men, and, you will be surprised to hear, ladies, who have just begun to breed animals or fowls. Others come from men

who have preconceived views on the subject, and others come from people of the most ignorant description, who lay on the doorstep of telegony anything that strikes them as extraordinary.

Now, gentlemen, it is very rare indeed that we have brought before our notice cases of the phenomenon in the mare or the cow, although it has become, owing to the case of Lord Morton's mare, a recognised practice on the part of small farmers and herdsmen, to put the young mare or cow in her first breeding phase to a good sire because of the popular belief in telegony, but it must be readily observed by you that when an inferior sire is used in the second instance, pecuniary questions arise.

I must personally remark, however, that I have never known a case of a man using a thoroughbred in the first instance and a half-bred or hackney in the second that the produce has been as good in type or like the first sire, or where atavism might not explain the phenomenon; but apart from this I wish to point out to you that most of the cases we get are in dogs and poultry, and I wish to suggest certain reasons for this extraordinary fact.

You are aware that in the case of mares and cows they are not allowed to roam at large.

In dogs and poultry, however, the animals and birds run frequently loose, consequently mesalliances are far more common and easy of accomplishment than in mares and cows.

This being the case, we have an answer to the reason why the cases are mostly those of dogs and poultry.

I will, therefore, argue the point from the question of dogs.

Now supposing that a bitch goes astray, and commits herself, as a general rule the puppies are at once given the order of the bath, and are thus got rid of, and the time comes round for a second litter.

In this case the owner takes her himself, or sends her to some so-called pure-bred dog, when she either throws mongrels or some pure-bred pups and a mongrel or two, upon which the owner at once, remembering the mesalliance of six months or a year before, says here is a case of telegony, and immediately furnishes us with it.

Now, on the face of it, this undoubtedly looks like a case of telegony, but before we can accept it as authentic we have to draw the line somewhere, and I think you will agree with me that this line ought to be just as strict as it can possibly be drawn if telegony is to be established.

For instance, we have to know whether the bitch was pure bred; we have to know whether the reputed sire is pure bred; and here let me make a remark. From what I have said to-night, you will be aware that a sire has frequently such an influence as to completely annihilate the type of the dam. Such an instance I have given you, viz., the produce of the Great Dane and the St. Bernard.

This dog, gentlemen, if you will remember, resembles the Boarhound so greatly as to be positively misleading, except to a practised eye.

XXIII

If, then, this is the case, you cannot but doubt that many so-called pure-breds are mongrels themselves, consequently the purity of the dam or the purity of the reputed sire is a question of the most vital importance.

Then again comes in the question: Did the owner after the bitch was lined in her second or third œstrum keep her under lock and key?

This the owner generally says he did; but we know that a man, say a business man, does not go about with a key in his pocket, and we know that the animal is frequently left to stablemen, &c., when on heat. Furthermore, we know the erotic nature of the bitch and the persevering attendance of the dog, and we know what that means.

Why, gentlemen, I once sent a Basset, who had in her first litter thrown pups to a Collie, to a noted Basset, and on her return she threw more half-bred Collies. A clear case of telegony, you might say? No, gentlemen, a clear case, as I afterwards found out on writing for an explanation, of the bitch being warded a second time by another

Collie belonging to the owner of the Basset, this through gross negligence on the part of the servant in whose care she was placed, and I should never have known anything about it had it not been through a second servant giving the other away.

The master, gentlemen, is the last person to whom such little lapses from duty are confided, and I may say I could enumerate quite two dozen such examples resulting in supposed telegony.

Again, supposing that the bitch was not shut up, but allowed on her return home to get to another dog, or was insufficiently shut up, then we have before us the possibility that the bitch either did not conceive to the reputed sire, supposing the whole litter to be mongrels, or superfœtation has taken place, and she has conceived to two, supposing that there were pure-bred and mongrels in the same litter.

Regarding this question of superfœtation, let me say a few words. A bitch, as a rule, will admit the attentions of the dog for a week or more, and if she is served very early in her heat, there is no reason why she should not conceive again to a second dog in a later stage. In fact she not unfrequently does. As a result superfœtation is far from uncommon.

You can therefore readily see, gentlemen, that if a satisfactory answer and absolute proof be not given to each of the questions relating to such possibilities, such as atavism, superfœtation, and fertilisation, by a second sire in the same heat, we have no right to admit telegony, for what appears so may be atavism, or a perfectly natural result, and I may say that I personally did try to investigate both of Mr. Spencer's cases, and in the one I could get no reply, as the owner had been long since dead, and in the other I could not get any information at all.

From such facts as I have been able to place before you to-night, gentlemen, I think you will agree with me that many instances of telegony may very readily be ascribed to other causes, but what is, I think, the crux of the whole question, and the most convincing proof against the frequency of the phenomenon is this: If the phenomenon is of such daily occurence as unscientific people would lead us to believe, why can it not be produced experimentally by scientific men, and I venture to say that the experimental crossings made in one year to demonstrate the phenomenon considerably exceed the natural mesalliances that occur in the same period.

Obviously, gentlemen, because telegony is exceedingly rare, and therefore abnormal.

And now, gentlemen, before I sit down, I have to give you my views as to the cause of telegony, in which I have every faith, but before I do so there is one question that I must bring before you.

You are aware from what I have said this evening that the impress of the male—I do not mean the previous sire—is very great, and that when a bitch or a female of another variety produces young to him, he not only passes on to his offspring his epiblastic attributes, but his mesoblastic. I bring before you the case of the Boarhound bred from a St. Bernard again as a most perfect example of its kind. Gentlemen, if you examine an authentic case of telegony you will find the results to be different, and this is of an important character.

In telegony, the epiblastic properties are what the previous sire passes on, but the mesoblastic are given by the putative sire.

In other words, the animal which is the subject of telegony has the variety type of his reputed parent, but the coat and external markings are those of the previous sire.

I will give you an instance. A few years ago I saw a very remarkable Fox-terrier, which was spotted exactly like a Dalmatian. I found it in this hospital, It belonged to our treasurer's son. Now we know that Fox-terriers are occasionally spotted, and many which appear white have, when washed, a spotted appearance about the skin, but no Beagle ancestry, from which I opine the Fox-terrier gets its markings, could account for the very remarkable spots on this Fox-terrier. The friend to whom it belonged informed me that it was sired by a prize Fox-terrier, and anyone who saw the dog would at once recognise that it was well bred, but on making further inquiries my friend informed me that the litter previous to the one in which this phenomenon appeared was sired by a Dalmatian, and I have not the slightest doubt that in this Fox-terrier we have a perfectly authentic instance of telegony.*

So it was in the case of the telegonous offspring of Lord Morton's mare. They were absolutely pure as far as their variety type was concerned, but they possessed the epiblastic features of the Quagga, namely, its markings. But there is one fact that deserves notice in dealing with the offspring of Lord Morton's mare, and one which seems pretty common where there have been other examples of recurring telegony in the same female: that as time goes by, the epiblastic features of the previous sire become less and less until the female appears to return to a normal condition, and she becomes the dam of animals which show no telegony.

Now, gentlemen, these two facts which I have just placed before you, namely, the differences in type displayed by the telegonous animal, compared with the non-telegonous, and the gradual return of the dam to a nornal condition, are very extraordinary, and they can mean but two things, namely,

1. That both the previous sire and the reputed sire enter into the composition of the telegonous animal, the reputed sire having the greater power of the two, because variety type is more fixed than individual.

2. That the dam is for a while under the sway of the previous sire, but that she eventually casts this influence off.

I repeat, gentlemen, although we are taught that an animal can only have one sire, I consider that it is possible, under abnormal circumstances, for an animal to have two. And that telegony is abnormal you have only to recall the facts I have already put before you this evening.

Now, gentlemen, having no proof that the spermatozoa can live for months and years, we cannot consider that telegony is due to any abnormality on the part of the previous sire, and as we have no proof whatever that the phenomenon is due to any abnormality on the part of the reputed sire, for such offspring are not born when he forms alliances with other females, we cannot consider that the phenomenon is in any way due to him, but we *have* proof that the occurrence is abnormal. We *have* proof that it can occur once, twice, or

*N.B.—Since Mr. Millais delivered this lecture, the American journal, *Forest and Stream*, report a case of a Fox-terrier bitch in Mr Belmont's kennel, after having been served by a St. Bernard, giving birth in five successive litters to telegonous offspring, and finally in the sixth giving birth to pure Fox-terriers; the sire in every litter being a pure-bred Fox-terrier after the mesalliance

several times in the same female, and, finally, we *have* proof that the female recovers eventually from the influence of the previous sire. Consequently under those circumstances we *have* a right to look upon the phenomenon as the result of some abnormality on the part of the dam, and I ask you with all due deference if we ought not to look for that abnormality in the ovaries of the dam, for nowhere else could such an abnormality as telegony arise?

In a word, gentlemen, I wish you to believe with me that the ovaries of a female that produces telegonous offspring, are infected to a less or greater degree by the spermatozoa of the previous sire.

This, gentlemen, entails upon me the necessity of proving that spermatozoa can reach the ovary, and that there can be no doubt on this matter is proved by the researches of a number of observers, which I think you will find either in Landois and Stirling's Physiology, or Galabin's Midwifery.

Personally, I have found them not only in the fallopian tubes, but outside the fimbriated appendages 18 hours after coitus. As, however, it may be said that these might have been squeezed out by rough handling on my part, I prefer to rely on much stronger evidence than this, viz., in the existence of ovarian pregnancies, few though they be, and abdominal pregnancies, which could not possibly have arisen had the spermatozoa not found their way direct to the ovaries. Consequently there can be no doubt but that spermatozoa do occasionally find their way to the ovarian surface, and my argument in the first case is that the spermatozoa of the previous sire get there, and, as it were, influence the ovaries of the dam, so that when mated with one of her own type she, in her second, or third, or several future pregnancies, shows the influence of the previous sire.

The next question that I have to put before you is the manner in which this influence is effected.

Now, on this question I have no direct evidence to offer you. Indeed I wish I had. Consequently I can only theorise on the subject, and leave others to prove that I am wrong.

I will at once state that my view is that the spermatozoa of the previous sire gain entrance to immature ova, and this I consider possible under three circumstances.

1. If the dam's ovaries are abnormal, say, for instance, where the capsule and stroma is imperfect and teased out, the spermatozoa might gain access to immature ova in this way.

2. Or, they might gain access to immature ova through the ruptures in the capsule caused by the escape of ripe ova at or about the time of coitus with the previous sire.

3. Or, they might be carried down as involution occurs in the formation of fresh ova.

Gentlemen, I do not pretend to say in which of these ways the spermatozoa of the previous sire gain access to immature ova, but that it is possible is quite within the bounds of reason and probability.

Believing, then, in this possibility, I ask what would happen if they did gain access to immature ova?

They would not fertilise such ova to the necessary extent that is required for full fertilisation and the formation of an embryo, owing to the immature condition of the ova, otherwise we should have the phenomenon of a dam in the higher mammalia producing in due time after each ovulation offspring without the attentions of a male, but what we do have is a condition of things not sufficient to set up embryonic life, but sufficient to effect such an alteration in the ovum, or ova, that when they are sufficiently fertilised at a later period, by the spermatozoa of the second or reputed sire, or even after this in recurrent telegony, this alteration is manifest not in the variety type, but the epiblastic character of the future progeny.

Finally, we have to deal with another possible cause of the phenomenon.

From what I have stated this evening, there can be no possible doubt that spermatozoa do reach the ovary, and, this being the case, I would ask you to consider for a moment the ultimate fate of such wandering cells?

We cannot for a moment believe that, failing in their object to incorporate themselves with ova, they return again down the fallopian tubes to the uterus, and are thus got rid of. No, we must believe that they eventually perish, and, having broken down, are absorbed.

Under such circumstances is it too much to put forward the view that such material is absorbed by the ovary and conveyed as food material to developing ova?

I do not see why such a suggestion should not be another explanation of the cause of the phenomenon, for such food material would infallibly cause a variation in future embryos, just as certain food materials are known to modify life in lower forms.

These, gentlemen are my views of the question, new ones, I admit, but at the same time they are ones which I shall hold until better are given, or I am proved to be wrong.

I beg to thank you most sincerely for having listened so patiently to me to-night.

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