TELEGONY

A Superstition that Dies Hard—Experimental Evidence from Breeding and Deductions from Embryology—Supposed Phenomenon as Negligible in Eugenics as in Stock-Breeding.¹

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AMONG the different divisions which naturalists and physicians have amused themselves by creating in heredity, surely none is more remarkable than Telegony. It alone, among all these divisions, has no relation to the line of descent, direct or indirect, between progenitors and offspring. Its principle is that females are *impregnated* by the first males to which they are bred, so that all their subsequent offspring, regardless of their actual father, will show influence of the first male. The latter is thus interposed between direct ascendants and descendants. Telegony, then, is supposedly a phenomenon of particular importance, since it is assumed to be able to modify the inherited constitution of lines of descent among animals; and it is of high interest to examine it just at this time, when eugenics is assuming such a prominent place in the attention of medical men.

As a fact, very serious suspicions rest on the reality of this phenomenon. Even the data cited to support it are not based on exact observations; most, if not all, are the reports of sportsmen or stock-breeders. Even at the present day, these latter jealously keep their female breeding animals carefully guarded, to avoid the chance of their being bred by a male of inferior pedigree.² But their affirmations seem to be more the result of simple belief than of scientific observation.

The first report, which has become

classic and was sufficient to convert Darwin, goes back to 1821: a mare of almost pure Arabian blood, belonging to Lord Morton, was bred to a quagga and foaled a hybrid. Afterward bred on two different occasions to a black Arabian stallion, she foaled two colts of brown color, whose legs were striped more plainly than those of the hybrid or even than those of the quagga himself; well-marked stripes also existed on the neck and some other parts of the body: the hair of the mane was short, stiff and erect, exactly as is the case with the quagga.

Since 1821, numerous similar cases have been reported. Darwin, among others, cites the following, which was communicated to him by Doctor Bowerbank: "A Turkish dog, black and hairless, having been accidentally bred by a spaniel of mixed blood with long, brown hair, produced a litter of five pups, three of which were hairless, while the other two were covered by short brown hair. When bred later by a Turkish dog, equally black and hairless, she produced a litter of pups, half of which resembled their mother—that is, pure Turk—while the other half were exactly like the pups *with short hair*, sired by the spaniel."

STORIES OF DOG BREEDERS.

In a general way, dog fanciers believe in telegony, and in this manner interpret most diverse occurrences. Thus Kiener writes, in his "Observations on Hered-

¹ Translated from *Biologica*, IV, 41,129, Paris, 15 mai, 1914.

² It would be a mistake to think that this statement applies without qualification to the United States. Most cattle, horse and swine breeders now disregard telegony; but dog and sheep breeders, as a rule, still adhere to the idea, and several sheep breeders' associations even refuse to register lambs whose mothers were ever "impregnated" by mating with a common ram.—The Editor.

ity" (1900): "I got from an artésienne bitch, bred to a splendid male of her race, a pup with 'wall' eye. She had previously been bred to a huge mastiff which had that peculiarity." Kunstler, on his part, reports (1901) that a French braque was first bred to an Irish setter and then to a blue braque of Auvergne, and then, by a third male—a Gordon setter—had pups resembling the Irish setter, and by a fourth male (an Irish setter) had a litter of pups, one of whom looked like the Gordon setter.

Resemblances, attributed to the same phenomenon, have been reported among sheep and goats, cattle, horses and swine. White ewes covered by black rams are said afterward to have borne pigmented lambs, even when bred to white rams. A cow of the Angus breed—characterized by absence of horns—first bred to a shorthorn bull, is alleged afterward, when bred to a polled Angus bull, to have produced a calf with horns, which looked like a grade Shorthorn. And as for horses, almost every breeder believes that colts foaled by a mare which has been previously bred to a jack, have a more or less mulish appearance. Cousin, in his excellent study (1904), relates that in Poitou popular belief considers mares as "interiorly mulish" and attributes that condition to the fact that for many years the mares of that region have been constantly used for the production of mules. The same writer quotes the observation of Bernardin (1901), relative to a mare whose first two offspring were two mules. Afterward bred to a heavy Arab stallion, she foaled successively:

(a) A colt of irreproachable form, very easy-gaited, whose feet alone, recalling those of its mother, were a trifle unsatisfactory;

(b) A colt which "showed, with certain diminutions, all the characters of a mule: the head, the mane, the neck, the withers, the back, the rump, the feet, the gait and, above all, the proverbial disposition."

These few examples are enough to

state the problem, while showing the kind of evidence that has led to its acceptance. I have taken the most classical examples, and the most convincing ones, too. Should they be accepted without discussion, as the expression of facts rigorously controlled and rigorously interpreted?

Not one of them, as a fact, is beyond criticism, and it appears that a careful examination leads us to give to each case a different explanation, which is not necessarily the same in every instance.

VALUE OF THE EVIDENCE.

At the start, it must be insisted that facts alleged to show telegony are never the result of carefully controlled experiments, nor even of observations which take all the circumstances into consideration. When a breeder talks of "pure bred," he refers to similar individuals which, bred together, usually and exclusively give offspring that resemble their parents; he refers, then, to a fixed race, but fixed only in these conditions, and he pays no attention whatever to things that happen outside these conditions. As a matter of fact, he can not really know whether the animals under consideration actually belong to one of these "pure breeds," carefully selected. The white mice furnish the best proof of this statement. No "race" is better fixed. Bred with each other, white mice continue indefinitely to produce descendants of the same color. And yet, this uniformity, like this fixity, conceals notable differences which make themselves evident when one of these white mice is bred to a wild gray¹ mouse. The posterity of a number of white females mated to the same gray male will include diverse individuals, whose variations can not necessarily be compared to the differences between one female and another. The posterity of some females will comprise nothing but gray and white individuals; but that of others will show, in addition, yellows, blacks, striped grays, striped blacks, etc. Often, if the matings were

¹ This color is technically known as *agouti*, and is considered to be the ancestral color of many small mammals, including mice, rats, rabbits and guinea pigs.—The Editor.

not rigorously controlled, one would feel compelled to suggest telegony. In reality, the explanation is found in the genealogy of the white mice. One can certainly find—and we will notice some of them—similar results among all sorts of animals.

After that, when a French braque bitch, first mated to a Gordon setter and then to an Irish setter, produces pups which differ both from her and from the Irish setter, it would be a good idea, before talking about telegony, to try the same matings separately with bitches that have never before been bred.

Similarly, when Lord Morton's mare, stated to be seven-eighths Arab, is bred by a stallion of pure Arab blood, there is nothing to prove that the zebra-like stripes and other peculiarities are traces of the zebra which fecundated the mare for the first time; they can just as easily be produced when the mare is bred to a black stallion. And as a fact, the stripes, in particular, have not the slightest importance. We know, now, that they appear rather commonly among horses, when there is not any possible reason to suspect the intervention of a zebra. Darwin himself, who unreservedly admits the fact of telegony, at the end of the first volume of this "Variation of Animals and Plants under Domestication," gives at the beginning of the same volume a series of examples showing that zebra-markings do not constitute a valid proof. Among others, there is the case of a brown Devonshire pony which "had on its back a very distinct stripe along the spine, light transverse rays on the inside of the legs, and four parallel bands on each shoulder." In volume II, Darwin considers these stripes to be the result of a cross between individuals of different color, and as constituting a case of "atavism." This last hypothesis opens up a different question altogether—we are absolutely in ignorance, and so was Darwin, whether the ancestors of the modern horse were or were not striped. It is a matter of little importance, anyhow; it is enough for us to know that the birth of a colt with stripes is far

from being sure evidence that its mother had been previously fecundated by a zebra.

A STRIKING CASE.

In support of this statement, Nathusius brings up a well-ascertained fact: a light bay mare, five times bred to a stallion of pure blood, foaled five colts with the same coat color. Afterward bred to a dappled gray stallion, she foaled a colt on whose back, shoulders and lower legs were stripes perhaps even more distinct than those on the offspring of Lord Morton's mare.

One can not argue from the appearance of the mane, either, to sustain the theory that a colt shows zebra characters. A short, erect mane is found in many breeds of horse, particularly among the ponies.

Thus, the ignorance of breeders as to the origin of their "pure breeds," and the absence of any control of experiments in their breeding, demand that we exercise great caution in accepting their conclusions on this subject.

Nor is that all. Let us not forget that in any stock, at any moment, a morphological variation of great or small importance may appear: hornlessness in cattle, taillessness in cats, mice and rats, the cerebral hernia which produces the so-called crest or hood in fowls, merino sheep, niata cattle of Chile (with undershot jaws like bulldogs), the franqueiro cattle of Brazil (with long, spiral horns), appeared thus, without anything to allow us to invoke the influence of telegony. We ought, then, always to suspect a coincidence of this sort when we are in the presence of a unique fact, which no experience or control observation corroborates.

Finally, there is one more way in which an honest observer may be deceived. I refer to superfetation—that is, the possibility that females may be bred twice with an interval of some hours or days, by two different males, and may produce offspring some of which are due to the first male and some to the second. There is no room for doubt about the reality of superfetation among dogs. Engelmann, von Rath, Tornier and others have reported in-

disputable facts. Among horses, there are the remarkable cases of mares mated first with an ass and later with a stallion, which foaled twins, one a mule and the other a colt, and each one, as Chauveau remarks, "bearing its special characteristics absolutely distinct." It will at once be understood how an unknown superfetation may be interpreted as telegony. And in spite of the care with which they are watched, a cow, a bitch or a ewe may very well be bred by different males two or three times at short intervals, without the knowledge of the breeder.

ALL EVIDENCE DUBIOUS.

Thus, the positive observations of telegony are all suspected in different degrees because, generally incomplete and conducted without exactitude, they do not eliminate the possibility of a different interpretation. And after that, it is to be feared that they also include a good deal of imagination. For you must not forget that the same breeders who carefully guard their pure-bred female stock to avoid telegony through interbreeding with mongrel males, also carefully guard their pure-bred male stock to avoid a similar result from interbreeding with mongrel females. They have the same fears for both sexes, and legitimize them by the same peremptory affirmations. Cousin reports, in this connection, the following:

"A breeder has a pure-bred Norman bull and pure-bred cows of the same breed; he has always had pure-bred calves. The bull is mated with a Jersey cow, and immediately afterward to a Norman cow; the latter produces a Norman calf which looks like a grade Jersey."

If it is difficult to know just what may have happened, it is in any case certain that any infection of the male by the female is impossible, unless in the form of a bacteriological infection. And if the calf of the second cow really presented unexpected characters, not im-

putable to her being bred a second time, it is absolutely necessary to fall back on the explanation of a variation due to unknown cause. But is it not enough to establish the necessity of this explanation in the present case, to make it legitimate when the case concerns a female fecundated by two different males?

Along the same line, it is well to remember that influence of a first impregnation has been alleged among birds, too, particularly pigeons and domestic fowls. Brahma hens, for example, fertilized by a Houdan cock, are said to have laid eggs, much later, when penned with Brahma cocks, which produced chickens resembling in certain respects the Houdan. But it is certainly difficult to see how such an impregnation could take place in oviparous females.

Should we henceforth reject telegony without further formality, merely because of the inadequacy of the proofs alleged? Evidently not. Precise observations and experiments are necessary, above everything else. Neither the one nor the other is scarce at present; the oldest date from 1872, due partly to Settegast and partly to Nathusius.

The former records the case of four mares bred first to asses and then to stallions; not one of the colts showed the slightest resemblance to a mule; *nor did their posterity show such resemblance, either.*

Nathusius cites, among other illustrations, the following: An Ayrshire cow had, by a hornless bull, a hornless calf; all her later calves, gotten by horned bulls, bore horns.

COSSAR-EWART'S WORK.

More recently, J. Cossar-Ewart undertook an extensive investigation of the subject, which lasted from 1896 to 1901. It comprised 13 mares, four of which were used for control experiments while the other nine were successively bred first to a zebra and then to a stallion, or vice versa. The results are summed up in Table I.

TABLE I.

Couples	Date of Foaling	Descendance
Walda x zebra.....	1898	hybrid twins
x stallion.....	1899	colt without resemblance to zebra
x zebra.....	1900	hybrid
x stallion.....	1901	colt without resemblance to zebra
Mulatto		
x zebra.....	1896	hybrid
x stallion B.....	1897	colt lightly striped at birth
x stallion L.....	1899	colt lightly striped at birth (Note.—The stallion L gave, with two fillies, striped colts like those of Mulatto).
Nora x stallion W.....	1895	colt bearing several stripes at birth; three persisted on the sholder.
x zebra.....	1897	hybrid.
x stallion C.....	1898	colt without stripes or any resemblance to zebra.
x zebra.....	1898	hybrid.
x zebra.....	1900	hybrid.
x stallion S.....	1901	colt like its sire, without stripes or any resemblance to a zebra.
Laura		
x zebra.....		abortion.
x stallion L.....	1898	colt without any resemblance to a zebra.
x stallion L.....	1898	colt without resemblance to zebra.
x stallion S.....	1900	colt without resemblance to zebra.
Rona x zebra.....	1898	hybrid.
x stallion M.....	1899	colt bearing no traces of zebra.
Lady Douglas		
x zebra.....	1897	hybrid.
x zebra.....	1898	hybrid.
x stallion M.....	1900	colt bearing no resemblance to zebra.
x stallion S.....	1901	colt bearing no traces of zebra.
Biddy		
x zebra.....	1897	hybrid.
x stallion T.....	1898	colt (Kathleen) without resemblance to zebra.
x stallion G.....	1898	colt bearing no traces of zebra.
x stallion G.....	1900	colt bearing no resemblance to zebra.
Kathleen		
x stallion M.....	1901	colt bearing no resemblance to zebra.
Timdra		
x zebra.....	1897	hybrid.
x stallion B.....	1898	colt having no resemblance to zebra.
x zebra.....	1899	hybrid.
x stallion S.....	1900	colt bearing no traces of zebra.

This table constitutes an important argument against the doctrine of telephony. In the cases of eight of the mares, the lack of influence of the fecundation by a zebra is, *morphologically*, indisputable because not one of the colts born

after the hybrids is marked by stripes of any kind. The colts foaled by the mare *Mulatto* by the stallion B and then by the stallion L, after the birth of a hybrid, alone show stripes. These stripes are not persistent; nevertheless,

they would have been quickly seized upon as evidence in favor of telegony, had not J. Cossar-Ewart had control experiments going on at the same time. In these, two mares which had never before been bred, were bred to the same stallion L and each one foaled a striped colt, very similar to the two colts of Mulatto. The influence of the zebra therefore becomes more than doubtful for her two colts, as well for the one sired by stallion L as for the one sired by stallion B.

The progeny of the mare *Nora* also merit special mention. Bred to the stallion W, before she had ever seen a zebra, she foaled a colt which, *richly*

striped at birth, kept three permanent stripes on the shoulder; bred then to a zebra, she foaled a hybrid; finally, she was bred to the stallion C and foaled a colt without the least sign of stripes. Again bred, in two different seasons, to a zebra, she produced two more hybrids; and finally bred to the stallion S, she foaled a colt which bore not the least resemblance to a zebra. Thus the only striped colt, which could have given hope to the advocates of telegony, is the one first foaled, at a time when the mare had never been bred to a zebra. All the other colts, born after the hybrid crosses, showed no resemblance to a zebra. This example shows once more

TABLE II.

Mares	Date of Foaling	Progeny	Characters
Litvinka (brown) born in 1893	1897	zebroid (aborted)	
	1898	foaled a zebroid	
	1902	foaled a zebroid	
	1905	zebroid (aborted)	
	1906	foaled a colt by half-blood Arab "Lebed".....	no stripes
	1907	foaled a zebroid	
	1908	colt by Lebed.....	no stripes
	1909	colt, sire unknown.....	no stripes
Pliakha (piebald) born in 1896	1901	foaled a zebroid	
	1902 to 1910	foaled nine colts.....	none striped
	1912	colt by thoroughbred sire.....	no stripes
Armida (bright bay) born in 1899	1902	foaled a zebroid	
	1904	foaled a colt by half-blood sire.....	no stripes
	1906	foaled a colt by half-blood sire.....	no stripes
	1907	foaled a colt by half-blood sire.....	no stripes
Priimikha (brown) born in 1898	1905	foaled a zebroid (artificial insemination)	
	1906	foaled a zebroid (artificial insemination)	
	1907	foaled a zebroid (artificial insemination)	
	1908 to 1912	foaled five colts	none striped
Vesta (bay) born in 1901	1906	foaled a zebroid	
	1907	foaled colt by thoroughbred sire.....	no stripes
	1908	colt by same sire.....	no stripes
	1912	foaled colt by thoroughbred sire.....	no stripes
Aida (piebald) born in 1902	1907	foaled a zebroid	
	1908	foaled a colt.....	no stripes
	1909	foaled a colt.....	no stripes
	1911	foaled a colt.....	no stripes
Zlodeika (dark brown) born in 1890	1902	foaled a zebroid	
	1903 to 1907	foaled four colts by half-blood sire...	none striped

how carefully we must avoid giving to mere coincidences the value of evidence, in this inquisition.

One more fact should be emphasized. It might happen that a colt without any visible resemblance, in form, to a zebra, should yet have something of the zebra in its constitution; this would then show itself, or at least might show itself, in the progeny of the colt. Now the mare *Kathleen*, foaled by *Biddy* subsequent to the birth of a hybrid, herself foaled an absolutely pure colt. This fact corroborates an analogous one of Settegast. It should be added nevertheless that as the characters of the zebra appear to be "dominant" in all crosses with the horse, this objection has only slight weight; since if there were any stripes or other anatomical characters transmitted, they ought to appear in the first generation.

CONFIRMATORY PROOFS.

Besides, the not less important investigation of Faltz-Fein and H. Ivanov (1913) confirms all the preceding conclusions. In no case did colts born after hybrids show any indications of influence of the zebra. And yet two of the mares (*Litvinka* and *Priimikha*) were in a position to have shown effects, if it were possible, of having been strongly impregnated, because one of them had been bred five times and the other three times to a zebra. Table II summarizes this investigation.

Outside of horses, we possess experiments in full agreement with these: Faelli (1900) reports the case of a Tamworth sow successively bred to a Tamworth boar, a Yorkshire boar, and a second Tamworth boar; all of the pigs sired by the Yorkshire resembled him, while all of the pigs by the second Tamworth male resembled their sire without the slightest trace of Yorkshire characteristics.

Cousin (1904) also cites a number of cases. A shorthaired greyhound bitch bred to a griffon produced a litter of pups of varying aspects; later bred to a male like herself, she produced pups without the slightest appearance of the former mate. A Saint Germain braque

crossed with a spaniel produced a number of pups which showed various evidences of cross-breeding; later mated with a male of her own breed, she produced a litter of typically pure-bred pups. With sheep, Cousin mated Berrichon ewes successively with a merino ram and then with a Berrichon ram; none of the products of this last mating showed the slightest trace of merino blood. It is to be remarked that the products of the merino-Berrichon cross look very much like the merino male, which means that, in Mendelian language, the merino characters are to some extent dominant over the Berrichon characters; they ought, therefore, to reappear in the succeeding lambs of the ewe, if she had really become impregnated.

I come now to the experiments carried out by Bond (1899) with rabbits and white rats, and by Miss Barthelet (1900) with white mice.

Bond mated a Himalayan rabbit (white with black feet) to a wild male, and then to an albino male. The first litter due to this last buck contained several individuals of a gray tint; but the following litters by the same male contained only albino individuals, although between each successive mating with an albino male, there was a mating with the wild buck. Were the gray offspring of the albino male, at first mating with him, due to the influence exercised by the wild male which had previously been bred to the female? That color is to rabbits like stripes to horses. Rabbits of the Himalaya breed possess pigment, because their feet are black, and the vague tint observed in the young is due to that pigment, the existence of which can not be disputed, much more certainly than to any mysterious influence which remains to be proved. It is even common that the crossing of a striped animal with one not striped produces offspring that are not striped, regardless of their color; and this is the case here. Besides, is it not remarkable that the gray tint was not repeated in succeeding litters, when conditions were most favorable for impregnation?

With rats, Bond proceeded in the following manner: an albino female was first mated to an ordinary brown male, and then to an albino male; with the latter she produced none but albino offspring.

Miss Barthelet proceeded in the same way with white mice: first mated with a gray male, they were later mated to an albino and produced only albinos, with no trace of gray.

THE CASE OF ALBINOS.

I must now point out that these two latter experiments have really no significance, either for or against telegony, because there is an important source of error in them. If the numerous researches into the inheritance of coloration during recent years have shown anything, it is that albinos mated with each other (rats or mice) produce albinos indefinitely, whatever be the origin of these rats or mice. In my own work I have mated white mice, born from gray ones, with white mice born from black ones, without ever obtaining offspring bearing a single gray or black hair. So even if we should admit the impregnation of a white mouse by a gray one, that impregnation could not manifest itself as the result of fecundation by a white male; the results obtained in the experiments just mentioned prove nothing, therefore, as regards telegony.

But rats and mice form, none the less, excellent material for the study of telegony; because even if, mated with each other, albinos produce only albinos, the identity of coloration does not conceal important constitutional differences, as I have already pointed out. These differences appear if a wild gray mouse, for example, is mated with an albino born of a striped black or striped gray; black or striped offspring will necessarily appear in the second endogamous generation, thus bearing witness to the influence of the ancestry of the albino.

Reasoning from these experiences, I mated white mice having neither black nor stripes in their ancestry, and wild gray mice, with a striped black male. The members of the first filial generation

were all uniformly gray, and all alike, as usually happens. Mated with each other these mice in their turn furnished gray, black and striped mice, thus proving that the general constitution of the first filial generation can, in certain conditions, show itself by the black coloring or striping. In consequence, if the impregnation of the mother really took place, she ought to be able to reveal it by transmitting black coloration or striping.

EXPERIMENTS WITH MICE.

To provoke its appearance, I substituted for the striped mouse a wild gray mouse in whose ancestry there was not, to my certain knowledge, any individual either black or striped; the appearance of such individuals in the posterity of these females, previously mated to the striped male, would then have been a very strong presumption in favor of telegony. None appeared. All the litters obtained, with one exception, contained nothing but gray mice, quite uniform in appearance.

I have noted one exception: it is interesting from several viewpoints, and particularly from that of telegony. One litter contained three wild-gray mice and two light gray ones with a brownish tint. If this tint had been darker, I should have been led to admit the possible of a telegonic impregnation; if it had been lighter, such an interpretation could not have been supported, for it amounted to no more than a difference in color among the different mice. In many cases, nothing more than this has been necessary to "prove" the influence of a previous sire. Here, however, the difference of tint offers no such explanation, because other wild albino couples, the female of which had never been mated with a striped male, furnished me with absolutely analogous results. Nothing remained but to await the result of matings between the different individuals of this F₂ generation; none of the matings thus made resulted in any trace of a black mouse or a striped one.

I made another experiment along slightly different lines, starting with the

idea propounded by various writers that albinism is an infectious disease. According to this hypothesis, a wild-gray female mated to a white male might later produce albinos, even though mated to another wild-gray male. The females submitted to this test did not at all confirm the proposition of the hypothesis: the product of wild pairs has been constantly and uniformly gray.

Thus all the experiments made with various animals in the endeavor to find effects of telegony under controlled conditions have given results absolutely contrary to that doctrine.⁴

WHAT REALLY HAPPENS.

Really, should we be surprised? Could we not think, *a priori*, that the question was, in fact, poorly stated? In reply, it is sufficient to try to find some means by which such an impregnation could take place.

Various mechanisms have been successively proposed. Some of these suggestions have lost all value in the light of present-day knowledge. The advance of knowledge has thus disposed of the idea of incomplete fertilization of mature ova (proposed by Weismann) and of the persistence of spermatozoa in some fold of the reproductive passages (as Claude Bernard suggested).

Others seem unsatisfactory. Romanes, for instance, thinks that the sperm, after penetrating into the uterus, is absorbed by the maternal organism and exercises an influence on ova that are not yet mature. But in truth it does not seem that the protoplasm thus absorbed can have any very different effect from that produced by the absorption of any other substance whatever. This protoplasm does not play the part of a fecundating agent; it does not reach the ovule until it has been very radically changed by contact with the protoplasm of the cell-tissues of the walls of the uterus which absorb it and that through which it must pass; so that if any modification of the protoplasm of the immature ova took place, it would be some

modification of the same kind that results from the absorption of some common alimentary substance; the spermatozoa could not produce, except under highly improbable conditions, a modification in the ovules, which would recall the characteristics of the male in question.

The only theory which rests to a certain extent on a solid foundation of fact is the one proposed by W. Turner, adopted by Cornevin, and by the latter expressed as follows:

"Would not the persistent influence of a first reproducer result from the fact that the mother herself had been materially impregnated by something belonging to the male, not by the direct action of the sperm, but by the intermediation of the fetus? Might it not happen that the latter possessed in its blood special properties belonging to its father, and that when its blood was exchanged with that of its mother, it might act on the latter as vaccin acts on the blood of a vaccinated subject? The mother's blood, thus impregnated, would act upon the ova which would be later fecundated by another reproducer."

THE EXCHANGE OF BLOOD.

The existence of exchanges between the blood of a mother and the blood of her fetus can not be the object of any serious controversy. Not only is the passage of toxins from the one to the other indisputable, but it is also known that a sort of equilibrium is established between the constitutions of the two serums. The experiment of Carlson and Drennon (1911) confirmed by E. Lafon (1913) is an irrefutable demonstration of this. These experimenters removed the pancreas of a pregnant bitch toward the end of her gestation; no trace of sugar appeared in her urine as long as fetuses remained in her uterus; but glycosuria became manifest immediately after the expulsion of the last of her pups; the quantity of sugar changed from 0 to .25 grams in the course of 48 hours.

⁴ For an illustrated account of the latest such experiment see "The Grevy Zebra as a Domestic Animal," by George M. Rommel; American Breeders Magazine, IV, 3, 129, Washington, D. C., July-September, 1913.—The Editor.

All the evidence indicates that the pancreas of the fetus completely took the place of the pancreas of the mother, which is the same as saying that the constitution of the maternal blood was equivalent, from that point of view, to the constitution of the fetal blood. Under the circumstances, the former indubitably derived from the latter.

The important point is to notice that constitutional equivalence did not exist after parturition; it depended strictly on the incessant exchanges of fetal serum with maternal serum. The result of the experiment will not permit us to go beyond that conclusion; and this result leads us to the belief that the influence of the fetus on the mother, in the circumstances observed, is but a momentary influence: soluble products filter through the placenta and parasitic organisms pass through it.

Henceforth we are in a position to put the question on the proper basis. Enlarging the interpretation of this experiment, shall we admit that in addition to the diverse products of the activity of the organism, the blood also contains the "character" of the animal considered, or at least the substances which determine these characters, and that these substances, transmitted by the fetus to the mother, become permanent constituents of the latter? For everything is there; and impregnation, as it is understood, necessitates the transmission of fully determined characters: stripes, colorations, forms of hair, absence or presence of horns, etc. Once this was believed to be the reality, and such a belief served as an argument against the transfusion of blood. Alain Lamy, of Caen, declared in 1668, in the *Journal des Sçavants* "that it was to be feared that, transfused, the blood of a calf would communicate to a man the stupidity and brutal instincts of that animal." The author also demands to know what becomes, in the veins of a man, of the particles of blood destined, in the calf, to produce the horns.

At the present day we are forced to reject such an interpretation. If it was really based on fact, telegonic phenomena ought to be produced with

extreme frequency, as a result of the use of serotherapy alone. Besides, the experiments described above, not to say the most elementary logic, fully prove that the peculiarities of an animal do not promenade in its blood, but that they are the very substance of that animal. And since the spermatozoon does not intervene directly in impregnating the ovum, it certainly does not intervene indirectly by the intermediary of the fetus, in the constitution of which it has become a part. We must, then, deliberately reject all idea of the transmission of definite characters, conformation or coloration of parts, of a given male to the descendants of another male, by the impregnation of a female.

REAL EFFECT OF EXCHANGES.

Nevertheless, since exchanges take place between the fetus and the mother, since these exchanges provoke a transformation in the maternal serum sufficient, for example, to furnish a substitute for the internal secretion of the pancreas, is it plausible, that that transformation should wholly disappear after parturition, that the maternal constitution should again become exactly what it was prior to gestation?

Evidently not! Every change induced in an organism necessarily leaves traces after it. But these traces do not and can not have anything to do with the supposed process of telegony. Beyond any doubt, the maternal organism has been placed, during gestation, under conditions of life very different from the antecedent conditions, and from these conditions a modification necessarily results. But that modification is as much a function of the maternal organism as of the condition in which it is found; the fetus does not impose its own constitution on its mother, no more than the temperature imposes on organisms on which it acts the "constitution" of heat-radiation. In fact, the exchanges between fetus and mother result in a new state for the latter, different not only from her antecedent state, but from that of her product. The maternal organism is not "impregnated" in any degree; the real phenomenon therefore

has no relation to the supposed phenomenon. The female does not receive from the male which fecundates her, anything which belongs essentially to that male; the constitution of that female does not become in any degree the image of that male, and there is no reason for surprise if the offspring of that female by a second male give no hint of the first male.

In fact, the question is quite different. Gestation naturally produces in the female a modification which we must suppose to be to some extent permanent. As a consequence, the female which produces a second offspring is no longer the female that produced the first offspring; whether the two gestations be due to the same male or to two different males, the fetus of the second gestation evolves in conditions different from those surrounding the fetus of the first gestation. But it does not undergo in any way the influence of the first male; in reality, what takes place is as if two different females were involved, mated with the same male or with two different males.

Such is the conclusion to which the experimental facts have logically led us. Can we draw from them any lessons relating to the improvement of breeds, particularly of the breed of mankind? If telegony, in its strict sense, could be considered a fact, the course to be pursued would promptly be indicated: every female impregnated by a male of inferior quality ought to be rejected. But under the circumstances what shall we do? Though a first gestation has certainly modified the female, we are ignorant of the direction of that modification; further, we have no way of finding it out. The quality of the male

gives no information on this point. Since the change which has taken place is not the passage from the male to the female of the qualities of the male, but the result of exchanges between fetus and mother, this result may be good or bad—eugenically speaking—by reason of conditions which at present we are unable to appreciate.

FACTOR IS NEGLIGIBLE.

The only line of action possible for us, therefore, is to neglect this factor, being persuaded anyhow that it is really negligible. The change which gestation produces in a female can not be, *in normal condition*, anything but an insignificant change; the important thing is to reckon with abnormal conditions, when a genuine infection is produced by infectious germs.

* * *

Telegony is not a mode of heredity. The latter is, in itself, a phenomenon of sufficiently complicated mechanism, without the necessity of adding to its study parasitical viewpoints which complicate it without gain. There are no other progenitors than direct ones; a previous gestation carries with it no predetermined effects; it is only one of the conditions which go to make up the general condition of the female at the time of her second gestation; but that general condition is in no sense the "warehousing" in any way of characters belonging to the first male. It is a result of the continued influence of the environment upon the organism. It is, then, an entirely new condition, and we do not have any means of foreseeing its exact nature. These facts must be borne in mind, whenever the question of heredity is involved.⁵

⁵ It is customary to connect telegony with xenia. This name is given to the following phenomenon: when a plant is fecundated by pollen of another species, the resulting seed and fruit ordinarily resemble the seed and fruit which would have been produced by fecundation with pollen of the same species. Now it sometimes happens that the fruit recalls in appearance the fruit of the species which furnished the pollen. But this phenomenon does not seem to be identifiable with the phenomenon of telegony. Admitting that the pollinated ovule exercises an influence upon the tissues of the fruit, it certainly does not exercise such an influence upon subsequent fruits. Besides, there is reason to believe that xenia results from pollen grains going astray outside the ovule and, consequently, that there is an impregnation in the positive sense of the word.