CHAPTER VI

TELEGONY AND OTHER DISPUTED QUESTIONS

"The mysterious wireless telegraphy of ante-natal life."—J. W. BALLANTYNE.

§ 1. *What is meant by Telegony*

THE term "telegony" is applied to doubtful, certainly rare, but, if true, very remarkable cases where an offspring resembles a sire which, though not its father, had previously paired with its mother. More theoretically expressed, telegony is the supposed influence of a previous sire on offspring subsequently borne by the same female to a different sire. The ovum or the embryo is supposed to be influenced by the mother's previous impregnation or by the consequences thereof.

To take a simple instance, the racehorse Blair-Athol had a very characteristic blaze or white bald face, and it is said that mares which had once borne foals to Blair-Athol subsequently produced to quite different stallions foals which exhibited the Blair-Athol blaze. It is very generally asserted by dog-breeders that if a thorough-bred bitch has had pups to a mongrel, her value is greatly decreased, for she will not afterwards breed true.

The alleged phenomena are of much interest, but the evidence of their actual occurrence is far from satisfactory, and their theoretical interpretation in terms of telegony is beset with physiological difficulties. But as a belief in telegony is still widespread, it will not be unprofitable to consider (*a*) the alleged facts, and (*b*) the interpretations suggested.

§ 2. *The Classic Case of Lord Morton's Mare*

The classic case, given by Lord Morton (1821), is thus summarised by Darwin : " A nearly purely bred, Arabian, chestnut mare bore a hybrid to a quagga ; she was subsequently sent to Sir Gore Ouseley, and produced two colts by a black Arabian horse. These colts were partially dun-coloured, and were striped on the legs more plainly than the real hybrid, or even than the quagga. One of the two colts had its neck and some other parts of its body plainly marked with stripes. Stripes on the body, not to mention those on the legs, and the dun-colour, are extremely rare—I speak after having long attended to the subject—with horses of all kinds in Europe, and are unknown in the case of Arabians. But what makes the case still more striking is that the hair of the mane in these colts resembled that of the quagga, being short, stiff, and upright. Hence there can be no doubt that the quagga affected the character of the offspring subsequently begot by the black Arabian horse " (Darwin, 1868, vol. i. pp. 403-4).

In 1823 the mare had again a foal by an Arab stallion, and this also showed some quagga characters.

It may well be asked: If this was not telegony, what was it? But the case is not quite so satisfactory as it seems. Settegast* remarks that the drawing made of the foal with the alleged quagga characters merely shows indistinct dark stripes on the neck, withers, and legs, and that similar stripes not uncommonly occur on pure-bred foals. A stiff mane may also occur as a variation in horses. It is possible that the alleged quagga-like characters had nothing to do with the original quagga sire, but were reappearances of latent ancestral characters.

Sanson (1893) sets another case against Lord Morton's. A bay mare had by two different stallions seven foals of a uniform colour, and then by a third stallion a foal *more* zebra-like than Lord Morton's. To which Delage adds that this eighth foal was pommelled grey—a colour with which zebra-like stripes are not infrequently associated.

Cornevin cites a breeder from the Pyrenees to the effect that a mare served by an ass and producing a mule was thereafter served by a horse and cast a foal which had hoofs more mule-like than horse-like. But this is too vague to be of much use, and besides, "asinine" variations sometimes occur in horses where there has been no hybridising (Sanson, 1893).

Moreover, the opposite result has been often obtained. Settegast (1888) gives the case of four stud mares which were served by asses and bore mules. They were subsequently served by horses, and the foals showed no asinine traits.

§ 3. *Representative Alleged Cases of Telegony*

Man.—Herbert Spencer cites from Flint's *Human Physiology* (1888) the case of a white woman who had intercourse with a negro and afterwards with a white man. There were some negro-pecu-

* *Thierzucht*, Berslau, Bd. i. 1878, pp. 223-34.

liarities in the children by the second male. But it is perhaps enough to say that it is difficult to get at the truth in such cases.

Cornevin (1891, p. 356) gives the following case. The widow of a hypospadic man had by a second and normal husband four hypospadic sons, two of whom transmitted the abnormality (*Lancet*, 1884). But in a case like this we require further particulars—*e.g.* as to the normality of the mother, and as to any tendency to hypospadism both in her ancestry and in that of her second husband.

Cornevin also cites the case of a woman married to a deaf-mute, by whom she had one deaf-mute child. By a second normal husband she had a deaf-mute child, and then others who were normal (Ladreit de Lacharrière, in preface to Goguillot's *Comment on fait parler les sourds-muets*, Paris, 1889). But here again it is necessary to know whether there was any tendency to deaf-mutism on the mother's side or in the ancestry of her second husband.

Dogs.—It is the deeply rooted opinion of dog-breeders—doubtless resting on a basis of experience, though it may be misinterpreted experience—that a bitch of good stock once lined by a mongrel is spoilt for further prize-breeding. It is said that many valuable bitches have been sacrificed because of this deeply rooted opinion.

The following case is cited by Cornevin (1891, pp. 356-7), from Kiener (1890). An Artesian bitch was first lined by a wall-eyed mastiff, and afterwards by an Artesian dog. Among the pups born to the latter one was wall-eyed. One requires to know how frequently a wall-eyed variation crops up, and whether there was any occurrence of it in the ancestry of the mother or of the second male.

Darwin (1868) gives the case of a hairless Turkish bitch which was lined by a spaniel, and had some hairless pups and some with short hair. She was subsequently paired with a hairless Turkish dog, but the offspring were as before. It must again be asked whether there may not have been some spaniel strain in the previous ancestry.

Spencer (1893) tells of a Dachshund bitch which was paired with a collie and had a hybrid litter. The following year she bore to a Dachshund a similar hybrid litter. But we require to know how thoroughly pure-bred the Dachshund mother and father were.

Perhaps the most useful comment on the cases of reported telegony in dogs is that made by Prof. Cossar Ewart (1901): "When it is remembered that we are surprisingly ignorant of the origin of the various breeds of dogs, and that, however pure the breed, reversion

to a former ancestor may at any moment occur, it will, I think, be admitted that, for the purpose of testing the 'infection' doctrine, the dog, of all our domestic animals, is the least satisfactory." Mr. C. H. Lane, discussing toy spaniels in his book, *All about Dogs*, says, "I have been told by breeders that they have had in one litter a specimen of all four breeds [*i.e.* of King Charles, Prince Charles, Blenheim, and Ruby spaniels]. In the same way rough and smooth terriers often occur in the same litter, not because of infection, but because of reversion."

Cats.—Dr. H. de Varigny tells of a normal cat which, after producing kittens to a Manx cat, had several tail-less kittens to an ordinary cat (*Journal des Débats*, September 9th, 1897; cited by Ewart, 1901). But the mother, or the second father, or both, may have had a tail-less ancestor, to which some of the kittens happened to revert. Or even if there were no such ancestor, the tail-lessness may have been merely a variation that happened to coincide with the peculiarity of the first sire, but was not in any way due to him. For tail-lessness is not a very rare "sport."

As a counter-case, Prof. Ewart refers to "a pair of young cats, of a somewhat peculiar variety, obtained from Japan. These cats belonged to a small breed, bluish in colour, with the exception of the ears and extremities, which were black. When the female grew up she first had kittens to a common tabby cat. These kittens showed the characteristic tabby markings. Her next kittens were by her Japanese mate, but in no respect did they suggest the previous tabby-coloured mate. No better experiment than this could be made with cats. The imported breed was quite distinct, and yet not sufficiently prepotent to swamp the common domestic English cat. Yet, though the first litter was sired by a common tabby, there was no indication whatever of the previous tabby mate in her second and pure-bred litter." (Case cited by Sydney Villar, F.R.C.V.S., *Proc. Nat. Vet. Assoc.* 1900, p. 130.)

Sheep.—Dr. Alexander Harvey, in a paper "On a Curious Effect of Cross-breeding" (1851), gives on the authority of W. McCombie of Tilliefour, Aberdeenshire, the following case:

Six pure-bred black-faced horned ewes were put, in the autumn of 1844, some to a Leicester ram (white-faced and polled), and others to a Southdown ram (dun-faced and polled), and produced cross-bred lambs.

In the autumn of 1845 the same ewes were put to a pure black-

faced horned ram of their own breed. The lambs were all polled and brownish in the face.

In the autumn of 1846 the ewes were again put to another fine ram of their own breed. Again the lambs were mongrels, but not so markedly as before. Two were polled and dun-faced, with very small horns; while the other three were white-faced, with small round horns. At length the owner parted with his ewes without getting from them a single pure-bred lamb.

Perhaps, however, the ewes were not so pure-bred as was supposed.

Cornevin cites from Magne the statement that white ewes, first crossed by black rams and then by white rams, bear to the latter, lambs which are piebald or which have blackish eyelids, lips, and limbs (Magne, J. H., *Hygiène vétérinaire appliquée*, p. 206). But black variations are common even when no black rams have been used for several generations.

Cattle.—Weismann (1893, p. 385) refers to a case reported by Carneri. A cow of a dark grey Mürzthal herd was put to a "light-coloured Pinzgau bull"; it bore a calf with the characteristic brown and white patches of the Pinzgau breed, as well as with distinct traces of the dark grey Mürzthal cross. It was subsequently served by a Mürzthal bull, and the second calf, while for the most part grey, showed "large brown spots like those of the Pinzgau breed." But this case is also inconclusive, since it is possible, as Carneri admitted, that "a drop of Pinzgau blood" may have previously got into the Mürzthal herd without his being aware of it.

Pigs.—Another circumstantial case cited by Darwin is that of a sow of Lord Western's black-and-white Essex breed, which Mr. Giles put first to a deep chestnut wild boar and after a time to a boar of the black-and-white breed. The offspring of the first union showed the characters of both parents, but in some the chestnut colour of the boar prevailed. From the second union the sow produced some young plainly marked with the chestnut tint, which is never shown by the Essex breed (Darwin, 1868, vol. i. p. 404).

Rodents.—Breeders of rabbits, rats, and mice have sometimes reported phenomena which suggest telegony; but the great variability of these rodents makes them very unsuitable subjects of experiment.

Prof. Cossar Ewart refers to two cases. Mr. C. J. Pound, bacteriologist to the Queensland Government, "crossed a grey rabbit with a grey-and-white buck, and then mated her with a black buck,

with the result that in the second litter there were grey-and-white as well as grey-and-black young. Again, a female black rat after breeding with a pure white rat produced, to a brown rat, white, brown, and piebald offspring. . . . Had Mr. Pound made a number of control experiments he would doubtless have discovered that black female rats sometimes yield to a brown rat white, brown, and piebald offspring, without having been first mated with a white rat, and that grey doe rabbits often produce to a black buck grey-and-white as well as grey-and-black young."

Experiments on rats and rabbits made by Dr. Bond (*Trans. Leicester Literary and Philosophical Society*, vol. v. October, 1899) yielded no results which could not be readily interpreted as due to reversion and other forms of variation.

Birds.—A case of supposed telegony in birds is referred to by Darwin (1868, vol. i. p. 405): "A careful observer, Dr. Chapuis, states (*Le Pigeon Voyageur Belge*, 1865, p. 59) that with pigeons the influence of a first male sometimes makes itself perceived in the succeeding broods; but this statement, before it can be fully trusted, requires confirmation." Mr. Frank Finn, in a paper entitled "Some Facts of Telegony" (*Natural Science*, iii., 1893, pp. 436-40), cites a number of cases which seem to him to afford evidence of telegonic phenomena in birds, but they are not convincing.

From the above citations it appears that the evidence of the occurrence of telegony is in great part, at least, of the same unsatisfactory character as that adduced in favour of use-inheritance —largely anecdotal, impressionist, and uncriticised. The need for careful experiments like those begun by Prof. Ewart (1896) is obvious.

§ 4. *Ewart's Penycuik Experiments.*

The position of affairs being that a number of great authorities —*e.g.* Darwin and Spencer—had expressed their belief in the occurrence of telegony, and that a number of equally competent authorities had expressed themselves extremely sceptical on the subject, Prof. Ewart resolved on definite experiment—the only secure path.

In general terms, he made a number of experiments likely

to give telegony the best possible chance of declaring itself, and although he has displayed his scientific mood in abstaining from dogmatic conclusion, and in suggesting many other experiments which should be made, there is no ambiguity in his verdict that the evidence of any undoubted telegony is very unsatisfactory. The Penycuik experiments proved this, at least—that telegony does not generally occur, even when what were considered to be favourable conditions were secured; indeed, anything suggestive of telegony occurred only in a very small percentage of cases. Moreover, where peculiar phenomena of inheritance were observed, they seemed to be readily explicable on the reversion hypothesis.

The general nature of the experiments may be understood by taking one of the best cases, which loses much, however, when summarised apart from the beautiful pictures illustrating the book (Ewart, 1899). A Rum pony mare, Mulatto, of remarkably pure breed, was served by a Burchell zebra stallion, Matopo, and the result in August, 1896, was Romulus, whose markings were quite different from those of his sire, being suggestive rather of the Somaliland zebra. In 1897 Mulatto had a bay colt foal to a grey Arab stallion, and this foal—unfortunately short-lived—gave no proof of telegony. The stripes which most frequently occur in horses were absent; there were others which are not uncommon in horses; but the most distinct markings (not that any were strongly developed)—namely, those across the croup—were of a sort extremely rare in both foals and horses. In short, the markings of Mulatto's second foal were puzzling, but in no definite way suggestive of the influence of the previous zebra sire. In this, as in the other cases, the verdict as to the occurrence of telegony was "non-proven."

In regard to experiments it should be remembered, however, that if telegony (supposing it to be a fact) be due to some strange persistence or unusual influence of the spermatozoa of a previous sire, then many *isolated* cases with negative results do not prove

much. As Pearson observes (1900, p. 462), "should it occur once in a hundred trials we are hardly likely just to hit upon the successful instance."

§ 5. *Suggestions which explain away Telegony*

(*a*) It has been repeatedly suggested, by those who do not believe in the reality of telegonic influence, that the phenomena are simply illustrations of *reversion*. A normal cat has kittens to a Manx cat, and afterwards to a normal cat. In the second litter some are tail-less. "It does not follow, however, that some of the subsequent kittens were tail-less *because* their dam had been previously mated with a cat of the Manx breed. . . . The most likely explanation is that tail-less individuals occurred in the ancestry of one or both of the parents; in other words, the absence of the tail is due to reversion to an ancestor" (J. Cossar Ewart, *Trans. Highland and Agricultural Society of Scotland*, 1901).

This view amounts to denying telegony in the strict sense. We are asked to believe that there is no causal nexus between the previous sire and the subsequent offspring who resemble him. They happen to resemble him because he resembled one of their ancestors. This seems to us easier than believing in telegony.

The plausibility of this explanation will vary in different cases. Thus Finn points out that the occurrence of feather-legged fowls in a pure Dorking breed, or of polled lambs from black-faced horned ewes, cannot be set down to reversion, "feather-legged fowls and polled sheep not being ancestral types."

(*b*) It has also been suggested that the subsequent offspring have accidentally varied in the direction of resemblance to the previous sire. The resemblance is a mere coincidence. As the reliable facts are few and far between, there is much to be said for this view.

(*c*) Another suggestion explains away the alleged facts of telegony by referring them to maternal impression, the supposition being that the mental image, etc., produced in the mother by the first sire exerts an influence on subsequent germs or on their development after fertilisation by another sire. There is little to be said in favour of this interpretation !

§ 6. *Suggestions as to how a Telegonic Influence might be effected*

(*a*) It is well known that in most European bats sexual union usually occurs in autumn, but the spermatozoa are simply stored in the uterus, ovulation and fertilisation taking place in spring after the winter sleep. A somewhat similar retention of stored spermatozoa, which become operative long after impregnation, is familiar in insects : thus, in some queen bees the store has been known to last for two or three years, and Sir John Lubbock gives the remarkable instance of an aged queen ant which laid fertile eggs thirteen years after the last union with a male. From a consideration of such facts the suggestion has emerged that the second offspring are really fertilised by persistent spermatozoa derived from the first sire.

Weismann (1893, p. 385) suggests the possibility that " spermatozoa had reached the ovary after the first sexual union had occurred, and had penetrated into certain ova which were still immature." When these ova mature amphimixis might occur, and coincide in time with a second coitus to which the subsequent offspring would be ascribed.

But were this the explanation, we should sometimes find, as Weismann remarks, that offspring were produced without any second sire at all. No such phenomenon is known among higher animals.

Moreover, there is no warrant for supposing that spermatozoa can persist as such through a period of gestation. " There is abundant evidence," Prof. Cossar Ewart says, " that in the

rabbit, as in other mammals, unused sperms lose their fertilising power and disintegrate long before the period of gestation comes to an end."

For these two reasons the above interpretation may be rejected.

(*b*) Somewhat subtler is the suggestion—often also called the "infection hypothesis"—that although the sperms of the first sire cannot be supposed to persist and fertilise ova discharged long afterwards, yet it is conceivable that the disintegrated substance of the sperms may persist and influence the ovaries and the ova, or that the sperms may exert an influence which does not amount to fertilisation.

So great a physiologist as Claude Bernard seems to have believed in the possibility of such an influence, though it is somewhat suggestive of the "aura seminalis" of the ancients. In this connection, however, Cornevin recalls the facts that a turkey-cock's impregnation of the female suffices for the score or so of fertile eggs which are laid during the season, and that the common cock's act suffices for seven or eight eggs. In both cases the fertile eggs are succeeded by other "clear" eggs, which are incapable of developing, and Cornevin asks whether we can believe that there is a brusque separation between the two sets, or whether the first at least of the "clear" set may not illustrate this supposed *partial fertilisation.* Romanes also suggested that the supposed effect was due to an absorption by the eggs of surplus sperm-material.

(*c*) Another slightly different suggestion is that the surplus sperms derived from the first sire exert a physiological influence on the *constitution* of the mother, such that subsequent gestations are affected. Perhaps no one will deny that the male may in this way affect the constitution of the female, and Brown-Séquard's experiments on injections of spermine or testicular extract may be recalled in this connection; but it is difficult to conceive that the influence should be of so precise a nature as to

evoke, for instance, the alleged quagga mane and quagga stripes in the second foal of Lord Morton's mare.

Baron compares this supposed influence to the influence of pollen upon fruit (see § 10), and Darwin says that this analogy "strongly supports the belief that the male element acts directly on the reproductive organs of the female" (Darwin, 1868, p. 405). But no specific effect on the female animal has ever been demonstrated.

(*d*) Perhaps the most plausible theory is that the mother is influenced through the fœtus during pregnancy, and that the influence re-acts on subsequent offspring. On this so-called "saturation hypothesis" the suggestion is that the characters of the sire, while expressing themselves in the unborn embryo, also saturate into the dam and affect her constitution in such a precise way that her offspring by subsequent sires may through maternal influence acquire (or inherit ?) some of the characteristics of the first. Thus Sir William Turner (1889), in discussing Lord Morton's case, says, "I believe that the mother had acquired, during her prolonged gestation with the hybrid, the power of transmitting quagga-like characters from it, owing to the interchange of material which had taken place between them in connection with the nutrition of the young one. . . . In this way the germ-plasm of the mother, belonging to ova which had not yet matured, had become modified whilst still lodged in the ovary. This acquired modification had influenced her future offspring, derived from that germ-plasm, so that they in turn, though in a more diluted form, exhibited zebra-like markings."

Similarly, Cornevin (1891) asks, may not the fœtus have in its blood special properties derived from the father, and may not these act like a vaccine on the blood of the mother? The blood of the mother, thus affected, will act on the ova subsequently fertilised by another sire (Cornevin, 1891, p. 359). So also Harvey, 1851. A similar hypothesis has been suggested to explain

certain facts connected with the so-called transmission of syphilis.

This view did not, however, commend itself to Darwin, for he says (1868, vol. i. p. 405): "It is a most improbable hypothesis that the mere blood of one individual should affect the reproductive organs of another individual in such a manner as to modify the subsequent offspring." He also points out that this hypothesis would not apply to telegony in birds, which has been alleged, though denied by Harvey and still requiring confirmation (Darwin, 1868, vol. i. p. 405).

It is conceivable that something like the "saturation" above indicated may occur in a case of a poison or protective anti-toxin, which might diffuse in and out. We can imagine that a sire infected with some virulent disease, and showing certain structural disturbances associated therewith, may have offspring which are similarly affected, and that the influence from them may pass before their birth into the constitution of the mother, and so affect her that subsequent offspring by a healthy sire are diseased after the manner of the first. But while we have some facts to go upon in regard to the diffusion of toxins and anti-toxins, we have none as yet which justify us in supposing the diffusion of structural characteristics or of representatives of these.

§ 7. *A Statistical Suggestion*

Prof. Karl Pearson (1900, p. 461) has approached the problem from the statistical side. If the female can be influenced at later reproductions by a male who has been associated with her in earlier ones, and if the alleged telegony is not due to some abnormal persistence of the spermatozoa of earlier unions, then in the *permanent* union of a pair we ought to find an increasing influence of the paternal type. But there seems to be, as regards stature, no evidence of any increase in the "hereditary influence"

of the father, therefore "no evidence of any steady telegonic influence."

But an increasing hereditary influence of the same father seems to us rather different from the precise point at issue in the controversy over the occurrence or non-occurrence of telegony. It must be remembered that the bias of the child this way or that depends on the relative potency of the various items in the paternal and maternal contributions to the fertilised egg-cell, and that this relative potency may be affected by a variety of circumstances—*e.g.* the relative age or vigour of the gametes at the time of amphimixis.

Careful comparisons of the families of the same mother by two successive husbands would be interesting—especially if there be anything in the suggestion that the telegonic influence is an influence exerted on the mother during gestation *by the previous offspring*, rather than *directly* through the previous father.

§ 8. *The Widespread Belief in the Occurrence of Telegony*

The belief that offspring sometimes resemble not so much "the father, but an earlier mate of the mother," is widespread among experienced breeders, and, like the belief in the influence of maternal impressions upon the offspring, is probably very ancient. Apart from stock, the belief is often expressed in regard to man himself. "We certainly know that what used to be spoken of as the 'infection of the germ,' but which, following Weismann, we nowadays call 'telegony,' was considered possible by physiologists at the end of the seventeenth century; we know the infection tradition has long influenced Arab breeders, and that believers in this hypothesis may now be found in every part of the world, more especially wherever an overlapping of distinct races occurs—as *e.g.* in the southern states of America and in certain Turkish provinces. Further, until quite recently many biologists considered that what is commonly and conveni-

ently known as Lord Morton's experiment has proved 'infection of the germ' to at least occasionally take place" (Ewart, 1899, p. 57).

It is psychologically interesting, therefore, to ask for some explanation of the widespread belief in the occurrence of a phenomenon the scientific evidence of which seems so slender. There is no doubt, we are told, that the value of a pure-bred bitch at once goes down if she has been accidentally lined by a mongrel, and it is possible that there may be good reason for this apart from the fact that the episode is not one which figures well in the record. It is possible that the constitution of the bitch may be subtly affected by a crossing—especially a fertile crossing—with a dog of inferior strain; and that the deteriorated constitution may react upon future offspring although real telegony does not ensue.

One must remember, however, that the statements one hears are often fairly precise. "If a pointer bitch gets accidentally served by a collie dog and produces a litter, the pups will be of various types, some like the pointer, some like the collie, and some a blend. And let that pointer bitch be afterwards served by a pure pointer dog, the result will be a litter among which the collie type can be unmistakably observed." It is desirable that some effort should be made to secure absolutely definite statements, supplemented by photographs.

It is hardly sufficient to remind ourselves that people are indescribably careless about their beliefs, and that breeders are notoriously superstitious; for considerations of money value have a potent effect in evolving carefulness, and breeding is gradually becoming an art based on scientific conclusions. There must be some basis for the widespread belief, and the answer given by the practical men themselves is that they have had abundant experience of the occurrence of telegony. This assertion leads us to look for phenomena which might be readily mistaken as telegonic, and there can be little doubt that Prof.

Ewart is right in maintaining that the mistake is in the misinterpretation of reversions.

A glance at the chapter on reversion (Chapter V.) will remind the reader that the crossing of different strains often results in apparent "throw-backs." A dark bantam hen paired with an Indian game Dorking produced, amongst others, a cockerel almost identical with a jungle fowl (*Gallus bankiva*)—that is, with the original wild stock. What occurs when different breeds are crossed may occur on a smaller scale when individuals of the same breed, but of different strains, are crossed. When reversionary phenomena occur they usually spell disappointment to the practical breeder. In search of an explanation, he sometimes thinks that he finds one in telegony; that is to say, gives the blame of the reversion not to the immediately preceding crossing, which was theoretically correct, and should have turned out well, but to some remoter, less careful, or perhaps accidental crossing. In this way the remoter sire is made the scapegoat for the reversion, and the belief in telegony has grown.

§ 9. *An Instructive Family History*

A good instance of the way in which cases of alleged telegony evaporate when analysed has been given by Dr. O. vom Rath. It concerns the somewhat intricate family history of certain cats.

A family who had lived for many years in Tunis migrated in 1888 to Baden, taking with them a beautiful pair of kittens. These were none the worse for the change, except that they grew up very unwilling to leave the house, and more or less vicious. The female cat (F) was grey-brown with black stripes; the tom (M) was pitch-black, except a large white spot on the right breast, and had a naturally half-sized left ear. In each litter which they cast there were some abnormal kittens, with rudimentary ear and tail. All these and all the males were destroyed; the normal females were given away. But as the tom (M) became more and more vicious he was castrated, and became peaceful and lazy.

The she-cat (F) was then crossed with an unblemished German tom, but she still produced abnormal kittens in each litter. Thus a strong suggestion of telegony arose.

Further inquiries showed, however, that a normal daughter of F, crossed with a normal German tom, had borne a red male with rudimentary left ear and rudimentary tail. Inquiries as to the pedigree of F and M showed that *f*, the mother of F, had a rudimentary tail, but no rudimentary ear, and was like F in colour. This *f* had been crossed with a red tom (R), who had a rudimentary ear and tail; there was but one litter, which was destroyed, and R soon afterwards died. Then *f* was paired with a normal black younger brother (S) of the deceased (R). From this normal S and from this *f* with a rudimentary tail, F sprang. But the two parents of *f* and the two parents of R and S were relatives, belonging to a family in which a rudimentary ear and tail were common—all springing from a pair which the owner of F and M had found in a hollow tree near Tunis.

Dr. vom Rath has more to tell, but enough has been quoted to show the correctness of his conclusion that there was no telegony at all. There was a strong family tendency to having a rudimentary ear and tail. But it is evident that if Vom Rath had not had patience to search out the family history, the case for the occurrence of telegony would have been fairly good—at least as good as many others.

§ 10. *A Note on Xenia*

The mysterious name "xenia," which seems to mean "guest-gifts," was applied by the botanist Focke to cases where the pollen from the "male" parent seemed to affect the tissue of the maternal ovary—the substance of the seed, or even the fruit, as distinguished from the embryo itself.

Correns has made careful experiments with maize and estab-

lished that there at least xenia occurs. When the white-grained variety (*Zea alba*) is pollinated from the blue-grained variety (*Zea cyanea*), the majority of the seeds have white endosperm around the embryo, but a few have blue endosperm. The converse is likewise the case. It must be noted that the effect is only on the so-called "endosperm," or nutritive layer around the embryo; the envelope of the seed, for instance, is never affected.

What happens seems to be this. The pollen-tube arising from the pollen-grain contains *two* generative nuclei, which arise by the division of one. Of these two nuclei, one fertilises the egg-cell, the other fuses with what are called the polar-nuclei (a fact discovered by Nawaschin and Guignard). Thus there is a sort of double fertilisation within the embryo-sac; the one results in the embryo, the other gives origin to the endosperm.

Thus we see that xenia (in the well-authenticated case of maize) is no mysterious influencing of maternal tissue by the pollen-tube, and that it does not require Darwin's hypothesis of a migration of "gemmules" from the fertilised ovum into the surrounding tissue. It is a phenomenon *sui generis*, due to the very peculiar "double fertilisation." As Weismann points out, it corroborates the view that the nuclei are the vehicles of the hereditary qualities.

Many of the alleged cases of xenia are cited in Prof. Delage's great work (1903, p. 252), the most picturesque being that of an apple-tree of Saint Valery. "This tree was sterile through the abortion of its stamens. Every year the young girls gathered branches from other apple-trees in flower, and shook them over the flowers of the non-staminate tree to fertilise them. Tillet De Clermont-Tonnerre (1825) relates that the resulting fruits recalled in their size, colour, and taste, those of the trees which had furnished the pollen."

It is to be feared, however, that many of the alleged cases of xenia will not stand examination. Thus the records in regard

to peas do not seem to be relevant, since the two halves of the pea-seed are of course the cotyledons and part of the embryo. Some of the phenomena seem simply ordinary cases of Mendelian inheritance (see Chapter X.).

Some of the cases where it is said that the whole fruit is affected —*e.g.* in grapes and oranges—well deserve further investigation.

§ 11. *Maternal Impressions*

It is a time-honoured belief that the mental states—especially vivid sense-impressions and strong emotions—of a pregnant mother may so affect the unborn offspring that structural changes result which have some correspondence with the maternal experience. The belief was hardly doubted till Blondel began to criticise it early in the eighteenth century.

Every one allows that the mother's health in the widest sense may react on the offspring, within what limits we hardly know; but it is a very different matter to believe in definite and specific structural effects. There can be no doubt that the firmly rooted theory is in the main quite unscientific, except in the sense that it expresses the instinct to discover some cause for peculiar phenomena. A child has hypertrichosis: did not the mother look too long at a picture of John the Baptist in a hairy robe? A white mother has a dark child: what can she say but that she was frightened by a Moor?

The abundant literature on the subject has been carefully studied by Dr. J. W. Ballantyne, and it need hardly be said that his general verdict is wholly against the tenability of the theory, except in a very refined form.

The mental experiences of the mother have been held to explain peculiarities of colour, abnormal hairiness, birth-marks, malformations, and even conception itself. The *post hoc ergo propter hoc* argument has never been more wildly used, and the result has been a retardation of the study of ante-natal pathology.

Jacob's trick of using peeled wands to influence the colour of

his stock is still practised in modified form. A famous breeder of cattle has assured me that to obtain a particular colour of calf from a cow which persistently refused to produce what he wanted, he followed the patriarch's prescription with success. He had her covered blindfold ; after the sire had gone he brought to her a heifer of the desired colour, and that was the first object she saw when the bandages were removed ; she was left with the heifer as a companion to occupy her mind, and the result in due time was a calf of the desired colour. Nor was this an isolated case.

What can one say—the credibility of the witness being secure—except the unsatisfactory word " coincidence " ? One requires to know in what direction, as regards colour, the sire was prepotent. One requires to know how many failures are forgotten in proportion to the successes remembered ?

It is admitted that shock and distress and the like may have prejudicial effects on the unborn offspring. It is stated that after the Irish famine and after the siege of Paris there were many children born with stigmata of various sorts, and these were sometimes referred back to particular experiences instead of to the general state of malnutrition and nervous exhaustion. But to associate a particular structural defect with a particular mental impression seems an untenable position. The *modus operandi* is difficult to conceive of. Sometimes, indeed, the maternal-impression theory is demonstrably untenable, when the impression occurs late in pregnancy, for most of the great events in development occur very early. We have also to remember the multitude of cases in which, in spite of very startling maternal experiences, the offspring is quite normal. In comparison with this multitude of cases where nothing happens, the number of really puzzling cases is very small, and may be dismissed as coincidences.

At the same time it is always unwise to speak of impossibilities in regard to matters which are inadequately known and imperfectly understood. That we cannot imagine the nature of a

physiological nexus does not prove its non-existence. Thus, as in regard to the transmission of acquired characters and telegony, we may be scientifically sceptical and give a verdict "non-proven," without dogmatically saying "impossible."

We can understand how contact with a puzzling case gives the observer pause. A medical practitioner of keen scientific intelligence told me of a patient who, during pregnancy, had seen her husband suffer a serious accident. His arm was cut open by a falling block. As the impression seemed to weigh on the woman's mind in its relation to the unborn child, the doctor was asked to reassure her—which he did, with confidence and no doubt with skill. He was rather startled, however, when the time came, to find that the child he ushered into the world *had* a mark on the arm suggestive of the father's wound, and on the same arm.

We must remember that for a prolonged period the unborn child is part and parcel of the mother—almost an integral part of herself—and we are beginning to know enough of the influence of mind upon body to make us cautious in dogmatising as to the possibilities of what Ballantyne * finely calls "the mysterious wireless telegraphy of ante-natal life."

* While expressing his disbelief in the potency of maternal impressions to cause conditions in the fœtus resembling the impression, Dr. J. W. Ballantyne cautiously adds ("Discussion on Heredity in Disease," *Scottish Med. and Surg. Journ.* vi. 1900, p. 310) that "to whatever extent we believe the mind capable of influencing the state of a part of the body, to that same extent, or to a degree rather less, the mother's mind might influence her parasitic growth—*i.e.* the fœtus *in utero*. But this amount of belief would of course vary very much in accordance with the elasticity of our belief regarding the influence of the mind over the body."