CHAPTER XII

DOUBTFUL PHENOMENA OF HEREDITY

1. 'XENIA' AND TELEGONY

ALTHOUGH it is certainly unnecessary in a theory of heredity to discuss all the possible kinds of phenomena which are with doubtful justice included under this head, I should not like to pass over in silence the consideration of certain presumptive observations, as they have so often been discussed, and were considered worthy of notice by so eminent an authority as Darwin. These refer in the first place to the so-called '*xenia*,' and to the phenomenon generally known as '*infection of the germ*,' — which, in case it really exists, I should prefer to speak of as *telegony*.*

Focke has used the term 'xenia' to describe those cases in which 'hereditary characters are supposed to have been transmitted by the pollen to the tissues of the fruit as well as to the fertilised egg-cell and the embryo arising from it.'

Darwin mentioned many instances of this kind, and attempted to account for them by supposing that an emigration of 'gemmules' takes place from the sperm-cells (pollen-tubes) to the surrounding tissue of the fruit. Focke has collected all the known cases, and on reading them, one receives the impression that they may very likely be deceptive. Blue grains occasionally occur amongst the yellow ones in cobs of the yellow-grained maize (*Zea*) after fertilisation with the pollen of a blue-grained species. It is possible that previous crossings of the two species may have produced this result, which might wrongly be ascribed to the immediate influence of the pollen of another species on the fruit. J. Anderson Henry even thought he had observed that all the flowers in an inflorescence of a white *Calceolaria* were reddened by the influence of the pollen from a red kind on a *single* flower of this inflorescence!

* From τῆλε — at a distance, and γόνος — offspring.

As such eminent botanists as Focke,* and more recently de Vries,† have expressed much doubt with regard to these observations — or rather interpretations, — we must wait until these cases have been critically reinvestigated before attempting to account for them theoretically. The chief difficulty we should meet with in any such explanation would be due to the fact that we are here concerned with the influence of the *germ-plasm* of the sperm-cell on a tissue of another plant which only constitutes *a part* of this plant. It would thus be necessary to assume that all the determinants of this germ-plasm are not active, and that only those take effect which determine the nature of the fruit.

The uncertainty of the observations is still greater in the instances of so-called *infection of the germ*. If the case recorded by Darwin — but not observed by him personally — is reliable, and has been accurately described, all doubts must be set aside. A mare belonging to Lord Morton 'bore a hybrid to a quagga,' and subsequently '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 the body plainly marked with stripes,' and the hair of the mane is said to have resembled the short, stiff and upright mane of the quagga, instead of that of the horse.‡ Similar cases of the influence of a previous fertilisation on the structure of subsequent offspring are related of several domestic animals, — viz., of cows, sheep, pigs, dogs, and pigeons, as well as of human beings when crosses occur between white races and negroes.

Up to the present time no experiments have been made with this special object, and it would be necessary to use every conceivable precaution in conducting such experiments, or they would be valueless; they could, therefore, be best made in zoological gardens, not only because of the undoubtedly pure material which might be used for the purpose, but also because

* Focke, 'Die Pflanzen-Mischlinge,' Berlin, 1881, p. 510, *et seq.*

† Hugo de Vries, 'Intracellulare Pangenesis,' Jena, 1889, p. 206.

‡ I have quoted this case from Darwin's 'Variation of Animals and Plants under Domestication,' 2nd ed., Vol. I., p. 435. Darwin does not seem to have known of the drawings of these colts which will be mentioned in a subsequent paragraph. I have not seen them, and only learnt of their existence from Settegast's book.

it would be possible to isolate the animals, and for the keepers to exert a strict control over them for considerable periods of time.

The philosopher Carneri mentions a case which came under his own notice. He kept a herd of cattle of the dark grey Mürzthal breed. On one occasion he put one of the cows to a *'light-coloured Pinzgau' bull instead of to one of the same* breed. The cow threw 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.' The cow was subsequently covered by a Mürzthal bull, and, contrary to expectation, the second calf was also a 'hybrid,' being for the most part grey, 'but possessing *large* brown spots like those of the Pinzgau breed.'

Both the above mentioned cases are not so conclusive as they appear to be at first sight. A drawing by Agasse of the foal possessing the characters of the quagga is to be seen at the Royal College of Surgeons in London, and shows indistinct dark stripes on the neck, withers, and legs. Similar stripes are, however, not very uncommon on purely bred foals, and ordinarily disappear as the animal grows older. No further resemblance to the quagga can, however, be detected in these pictures.*

I must not omit to mention that before having heard of the hypothesis of 'infection,' Carneri accounted for the case of the two breeds of cattle described above by supposing that 'a drop of Pinzgau blood' must have previously got into the Mürzthal herd without his being aware of it.

Thus even the best of these 'cases' are not reliable and actually convincing. We may, however, at any rate suppose that this so-called 'infection,' if not altogether deceptive, only occurs in rare instances, and by no means regularly, or at most only in some cases. Experienced breeders, like Settegast and Kühn of Halle, do not believe in it; for although they have frequently crossed various domestic animals, they have never observed an instance of it. Such cases could only be accounted for from our point of view by supposing that spermatozoa had reached the ovary after the first sexual union had occurred, and had penetrated into certain ova which were still immature. The

* According to Settegast ('Thierzucht,' Breslau, Bd. I., 1878, pp. 223–234).

immediate fertilisation of the latter is rendered inconceivable by the fact of their immaturity, and the sperm-cell must have remained in the body of the ovum until the maturation of the latter, with the nucleus of which it then united in the process of amphimixis. If this occurred sometime after the first of the offspring was born, it might easily have coincided approximately with the second *coitus*, from which the fertilisation would then apparently be due. If the 'infection' were proved beyond a doubt, a supplementary fertilisation of an egg-cell in this manner must be considered possible; we certainly might then reasonably ask why mares, cows, or sheep, should not occasionally become pregnant without being covered a second time. *But this has never yet been known to occur*, and I incline to Settegast's view that *there is no such thing* as an 'infection' of this kind, and that all the instances which have been recorded and discussed critically by him are based upon a misconception.

2. The Influence of Temporary Abnormal Conditions of the Parents on the Child

Although I do not consider that the cases which come under the above heading have anything to do with heredity, I should not like to leave them entirely on one side.

It has often been supposed that drunkenness of the parents at the time of conception may have harmful effect on the nature of the offspring. The child is said to be born in a weak bodily and mental condition, and inclined to idiocy, or even to madness, &c., although the parents may be quite normal both physically and mentally.

Cases certainly exist in which drunken parents have given rise to a completely normal child, although this is not a convincing proof against the above-named view; and in spite of the fact that most, or perhaps even all, the statements with regard to the injurious effects on the offspring will not bear a very close criticism, I am unwilling to entirely deny the *possibility* that a harmful influence may be exerted in such cases. These, however, have nothing to do with heredity, but are concerned with an *affection of the germ by means of an external influence*.

The experiments of the brothers Hertwig show that the development of the fertilised egg in lower animals may be considerably

retarded by the action of various chemical substances, such as chloral, quinine, and morphia; and we also know that the ova of sea-urchins, if kept too long in the sea-water before being fertilised, tend to lose their vital energy, and consequently many spermatozoa, instead of a single one, are likely to enter each of them. A similar result may follow from the effects of the above-mentioned chemical reagents, and in both cases an abnormal development of the egg, such as a duplication of parts, may be the consequence.

It does not appear to me impossible that an intermixture of alcohol with the blood of the parents may produce similar effects on the ovum and sperm-cell. According to the relative quantity of alcohol, either an exciting or a depressing influence might be exerted, either of which would lead to abnormal development. A depressing influence exerted on *both* germ-cells would certainly retard, or even quite prevent, the process of fertilisation; while if the egg-cell were *alone* affected, super-fertilisation (polyspermy) might result; and the same might occur by an excitation of the sperm-cells alone. The entrance of several spermatozoa into the small human ovum, which contains only a small amount of yolk, might produce an abnormal development just as much as in the case of the eggs of the star-fish or sea-urchin. A high degree of excitation in both germ-cells might, on the other hand, cause the complicated processes of the increase of the germ-plasm in the ovum and the subsequent conjugation of the two germ-plasms to take place in an inexact manner, owing to their being passed through too quickly, and would then produce an irregular development.

New predispositions can certainly never arise owing to such deviations from the normal course of development, and therefore a modification of the process of heredity itself is out of the question. It is, however, conceivable that more or less considerable abnormalities may affect the course of development, and either cause the death of the embryo, or else produce more or less marked deformities. The question as to whether such deformities really result in consequence of the drunken condition of the parents can only be decided by observation.

3. The Supposed Transmission of Diseases

There is no doubt that some diseases are passed on from one generation to another. All such cases are not, however, con-

nected with heredity, and many of them are in all probability to be explained as the result of infection of the parental germ-cell with microscopic parasites, and ought consequently to be described as *infections of the germ*.

In man such a transference of disease has only definitely been proved to occur in the case of syphilis.* The father, as well as the mother, is capable of transmitting this disease to the embryo, and the only possible explanation of this fact is, therefore, that the specific bacteria of syphilis can be transmitted by the spermatozoon. Amongst the lower animals the 'pebrine' of the silkworm is an example, which has been well known for several decades, of the transference of a fatal disease from one generation to another through the egg: the germs of the fungus which produces the disease penetrate into the yolk. It is not known why these germs do not develop and multiply within the egg, and thus destroy it, but this is, however, the case. The fungi only begin to multiply in the young caterpillar † when it is half- or full-grown, or the disease may, again, only be fatal in the butterfly stage.

As we now know that many diseases of man and other mammals are due to such low forms of parasites, it is natural to suppose that the transmission of such diseases results from infection of the germ-cell with microbes, and not from inheritance in the true sense of the word — that is, from the transmission of an anomalous state of the germ-plasm itself.

I have elsewhere attempted to trace the 'heredity' of 'epilepsy,' produced artificially in guinea-pigs, by supposing that in this case a similiar process occurs. The slow development of this form of 'epilepsy,' resulting from an injury to the spinal cord or one of the larger nerves, seems to me, indeed, to support the conclusion that its symptoms, which resemble those of true epilepsy, are due to the migration of microbes, which advance from the injured part along the nerves in a centripetal direction until they reach the brain, where they set up the state of irritation characteristic of the disease. The great inconstancy of the symptoms, and the variety of forms of nervous diseases which the offspring exhibit, also indicate that a true heredity is

* *Cf.*, *e.g.*, Dohrn, 'Zur Frage der hereditären Infection,' Deutsche med. Wochenschrift, Sept. 15, 1892.

† *Cf.*, F. Haberlandt, 'Der Seidenspinner des Maulbeerbaums u. seine Krankheiten Wien, 1871.

not concerned in the process, and that the transmission is in this case due to infection of the germ with the microbes by which the disease is induced.*

The 'transmission' of carcinoma might be accounted for in a similar way, — if, as has recently been supposed, this disease is really due to microbes.

It is, however, also conceivable that both causes — the transmission of abnormal predispositions, and infection of the germ — might combine to bring about the transference of a disease from one generation to another. Without desiring to encroach upon the domain of pathology, I am inclined to suppose that this is the case as regards 'hereditary' tuberculosis: there is no doubt about the occurrence of a 'tuberculous habit,' — that is, a certain complication of structural peculiarities which is commonly connected with the disease, such as a narrowness of the chest, for instance. These peculiarities must result from the structure of the germ-plasm, in which a definite variation of certain determinants and groups of determinants must have taken place, and they are therefore certainly transmissible. The disease itself, however, is not due to this 'habit,' but is caused by the presence of specific parasites, the tubercle-bacilli, which have a harmful effect upon the various living tissues. They may be introduced artificially into the blood, and then produce the disease even in perfectly normal individuals. They may, moreover, enter the body 'spontaneously,' *e.g.*, by some natural means, and will then also give rise to the disease. But in the latter case the probability of infection seems largely to depend upon the susceptibility or power of resistance of the individual, and at the present day pathologists are of opinion that persons exhibiting the 'tuberculous habit' already referred to have a much slighter power of resistance to the parasites which have passed into the body than strongly-built people. The inheritance of the disease would accordingly depend on the transmission of a constitution very liable to infection.

Without wishing to deny the existence of such a predisposition to infection, I do not believe that the transmission of tuberculosis is due merely to the inheritance of a greater degree

* A more detailed account and proof of this view concerning the infectious nature of traumatic epilepsy in guinea-pigs is contained in my essay on 'The Significance of Sexual Reproduction,' Appendix iv.

of susceptibility. A large number of facts seem to me, on the contrary, to support the view that *infection of the germ plays the chief part* in the process. It would be out of place to enter into particulars and attempt to prove this view here—the question belongs to the province of the pathologist: I merely wished to point out in this connection that a combination of hereditary transmission and infection of the germ is perfectly conceivable. The *phyletic origin* of such constitutional diseases is presumably to be explained as being due to the occurrence of certain individuals possessing constitutions which were abnormally susceptible to a certain kind of microbe. Such persons would be more readily attacked from without by this particular disease. If, however, it once attacked them, and were it of such a kind as to cause death only after some time, a further and much surer opportunity was offered to the microbes for transferring themselves to other hosts than was previously the case when they passed into the body from without: — they settled in the germ-cells of the individual affected, and were thus transferred to the descendants of this individual. Although the presence of parasites in the germ-cells has not yet actually been proved in the case of tuberculosis, in my opinion it by no means follows that such an infection does not nevertheless take place: we do not even know whether such microbes are of the ordinary form and size. In any case they must possess different vital qualities; for did they multiply in the egg- or sperm-cell in the same manner as in the tissues in which they are known to occur, the germ-cells would soon be destroyed. Numerous adaptations to the host may have occurred in this case as in that of other parasites; and, moreover, *latent periods of development* may have arisen during which the parasite does not undergo multiplication. It seems improbable that such arrangements should not be met with, and that the parasite should not make use of the favorable opportunity of becoming distributed with the greatest certainty. Latent periods very commonly occur in the germs of animals and plants whenever they are useful, and hence this arrangement must come about without any great difficulty.

Even although our most eminent pathologists, such as Ernst Ziegler, are now of opinion that tuberculosis is not transmitted by infection of the germ, because such a transmission has not been directly proved, and because, on the other hand, an in-

fection from without cannot be conclusively disproved in any individual instance, I am inclined to believe that they have been too cautious in their conclusions, of which only a negative proof is furnished by either factor. For neither of these in the least proves that infection of the germ does *not* take place: from a more general, biological point of view, indeed, it seems to be far more probable that it *does*.

It will, I think, at any rate be conceded that a 'constitutional' disease cannot be taken as a proof that the processes of heredity are therein concerned until we can determine whether we are actually dealing with heredity, — *i.e.*, the transmission of a constitution, — and not only with a transference of microbes; and the main object of this section was to make this clear. But at the same time I have stated my reasons for using so few facts from the domain of pathology in support of my theory.