REMARKS

ON

HETEROGENESIS IN ITS RELATION TO CERTAIN PARASITIC DISEASES.

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IV.

DURING the last fifteen or twenty years, another most important disease has prevailed extensively amongst the silk-worms of France; which, though of the utmost consequence commercially, has seemingly been due to the ravages produced by certain extremely minute vesicular parasites usually known as *Psorospermia*—organisms concerning whose real nature and affinities systematists are not agreed. Some place them amongst the equally obscure group of Gregarinidæ; whilst others, such as Robin and Balbiani, consider them to be more allied to Diatoms or aberrant forms of Alga. Organisms of a similar nature, though rather more complex in structure, were first discovered about thirty years ago by J. Müller, pervading the spleen, kidney, and almost all the organs and tissues (with the exception of the great nerve-centres and the muscles of the trunk) of certain fresh-water fishes. They often follow the line of the blood-vessels, and are occasionally concentrated in such enormous numbers as to form small whitish or yellowish masses visible to the naked eye, although individually the organisms are only a very little larger than the blood-corpuscles of the fish. Subsequent investigations have revealed the fact that these rudimentary organisms are to be met with habitually in the tissues of higher as well as of lower organisms, where, for the most part, they seem to exist without the slightest detriment to the creatures which contain them. Six years ago, when the cattle-plague raged with great virulence in this country, some observers thought for a time that they had detected its cause when they found myriads of these minute corpuscles imbedded in the flesh of animals which had succumbed to the disease. Dr. Cobbold, however, soon made known the fact that such organisms might be met with most abundantly in the healthiest mutton and beef; and that they were always to be found in astonishing numbers in the substance of the heart of sheep, oxen, and other animals. They had, in fact, nothing whatever to do with the cause of the cattle-plague. Similar parasites have been found, on several occasions, in various parts of the human body; and such living units seem to exist habitually in almost all varieties of lower organisms. They have been encountered, for instance, in the majority of insects, and in many other classes of the Articulata; whilst Balbiani has also discovered them amongst the Arachnida, and in many fresh-water Entomostraca. In the latter animals, and also in some serpents, as Vlacovitch ascertained, the corpuscles are of the same simple character as those which are to be met with in certain diseased silk-worms; though in fishes and other organisms* the corpuscles are often more complex, being composed of a mass of granular protoplasm enclosed within a resistant bivalved envelope, having a projecting rim at the junction of the valves, and two internal ovoidal projections at one or at both extremities. They multiply with great rapidity, since some of the corpuscles are capable of growing to fifty or one hundred times their original size, so as to constitute generative bodies, which, by a process of segmentation, resolves itself into a new progeny of *Psorosperms*, just as *pseudo-navicellæ* are produced from *Gregarina*.

These organisms being, therefore, extremely common, and for the most part innocuous to the animals which they frequent, it is somewhat surprising to find them apparently producing a fatal epidemic disease amongst silk-worms. It is, however, absolutely certain that in this affection—named “pébrine” on account of the black spots which it produces on the skin—*Psorosperms* of the simplest description abound in almost every organ and tissue of the affected worms or moths. Whether they are causes of the disease, however, or are mere concomitant products, really occasioned by a previous blood-change (and therefore comparable with the Fungi in muscardine), it is impossible for us to say;† though it is certain, from the researches of Pasteur,

Balbani, and others, that the disease is eminently contagious. Pébrine resembles flacherie, though it is unlike muscardine, in the fact that it may be transmitted hereditarily. In these cases, the affected eggs, according to Balbiani, always yield a slightly acid reaction when

Fig. 5.—Psorosperms and their Mode of Development (Balbiani).

a a, Psorosperms from the Silk-worm; *b b* and *c c*, rare and occasional varieties of these; *x y*, Psorosperms in an earlier stage of development, which are found intermixed with the more perfect forms (*X 1700*). *M S*, Portion of the intestine of a small Caterpillar (*Gastropacha*), showing masses of Psorosperms in different stages of development beneath the serous coat. At first, mere homogeneous masses of matter (*p p*) appear, in the midst of which Psorosperms are developed after the manner of nuclei (*X 250*).

crushed upon litmus-paper, whilst healthy eggs are neutral. Such infected eggs do not at first exhibit any distinct Psorosperms, though these gradually develop from particles which are originally quite indistinguishable from the ordinary granules of the ovum. Such appearances are, therefore, quite consistent with the possibility of the mere hereditary transmission of a morbid quality to the ovum, whose presence, whilst it is already attested by the acid reaction above referred to, soon still further displays itself by a new evolution of Psorosperms.

On this theory the disease would be contagious, owing to the corpuscles acting after the manner of mere dead ferments. They would incite certain changes in the fluids with which they came into contact; and these changes would culminate in the re-evolution of new and independent organisms in the blood and tissues generally, or in some parts of them—just as the poisonous organic particle from the skin of a small-pox patient incites spreading changes in the body of a person with whom it comes into contact, which changes terminate in the evolution of a similar poison. The organism incites the disease by setting up molecular changes, rather than by its direct multiplication as a living thing.*

The opposite notion—viz., that the communicated disease is caused by the direct multiplication of the organisms which act as contagious agents—is beset with difficulties. Seeing how exceedingly common Psorosperms are in the bodies of all kinds of animals in their natural or healthy condition, why should they not multiply to an undue extent, and produce disease in them? Such organisms are probably swallowed by most of us during each meal in which meat is taken, and yet no harm ensues—they are probably just as nutritious as the meat in which they are contained. Speaking of their almost universal prevalence, Dr. Cobbold says: “Altogether, at two meals, I could not have swallowed less than eighteen thousand of these *Psorospermæ*,” and he adds that “lovers of beef, mutton, and pork, eat these bodies every day, but take no harm.” Again, silk-worms probably contained *Psorospermæ*, and swallowed them at times with their food just before the outbreak of pébrine, and yet they were not affected by a fatal epidemic disease; though at last a particular change or morbid condition of their tissues may have been initiated, in which *Psorospermæ* were especially prone to be developed—characterised molecularly by some peculiar properties which were capable of initiating similar changes in other silk-worms. The rational conclusion concerning pébrine, therefore, seems to be very similar to that which

indeed says: “It appears to me in no way made out whether the *Psorospermæ* are to be considered as the result of a special animal development; whether they, like *pseudo-navicellæ*, are the nuclei of gregariniform productions; or whether they are the final products of pathological metamorphosis.” And again, in his *Untersuch. über Trichina Spiralis* (1860), in speaking of the enteritis which is occasionally induced by these Nematoïd parasites he says that “croupy masses,” which may be thrown off in flakes, may at other times resolve themselves into pus-corpuscles, or in dogs may be “converted into *psorospermæ*” (Quoted by Dr. Cobbold in his *Entozoa*, p. 344).

* I have previously (BRITISH MEDICAL JOURNAL, Oct. 7, 1871) expressed the belief that the germs of existing morbid growths also operate in this manner when they are the causes of “secondary growths.”

* Including some insects, such as *Pyralis*, according to Balbiani.

† According to Robin, it appears that they are frequently developed in the midst of an amorphous and homogeneous “masse sarcodique génératrice,” which insinuates itself or lies between the elements of the tissues in all directions, so that they appear to be imbedded in this amorphous substance. Further researches concerning the mode of origin of this substance are much to be desired. Professor Leuckart

we were compelled to adopt concerning the mode of origin and propagation of flacherie.

Looking, then, broadly at the various facts which have been detailed in this communication, how are they to be explained by those who refuse to believe in the occurrence of Archebiosis and Heterogenesis?

The only answer which can be adduced may be given in the words of Dr. Beale, who says (*Disease-Germs*, 1870, p. 64):—"The higher life is, I think, interpenetrated, as it were, by the lowest life. Probably there is not a tissue in which these germs are not; nor is the blood of man free from them. They are found not only in the interstices of tissues, but they invade the elementary parts themselves." This represents, in general terms, the doctrine or mere assumption which is made by many at the present day, who cannot otherwise account for the numerous phenomena to which we have just been referring. It is, however, in direct opposition to the experiments of Dr. Burdon Sanderson, and is antagonistic to the theory and practice of those who believe in antiseptic surgery. It is, indeed, an hypothesis of Panspermism very similar to that which was started by Bonnet more than a century ago. But under what different auspices and prospects is this old view still sought to be maintained! Originally supported by no independent observations, and based only upon the occurrence of phenomena which could be otherwise explained, it was thought to derive support from those views concerning development which were at the time most prevalent. But these old notions of development advocated by Haller and Bonnet—the doctrine of the "pre-existence of germs" of all organs and tissues in the ovum itself—has faded away like a cloud before the more correct and light-giving notions of Harvey, Von Baer, and the modern evolutionists. With the disappearance of these old doctrines of development, vanished all that could ever have been said to give countenance to the doctrine of Panspermism. The hypothesis, however, now stands in even a more sorry plight: it is condemned and undermined by many of the recent and positive testimonies of scientific workers.

Even independently of all this, however, it will be found, as we have already hinted, that an exclusive doctrine of contagion is singularly unsupported, and inadequate to account for the spread and perpetuation even of undoubtedly parasitic diseases. In this respect, the history of these diseases is essentially similar to the history of those non-parasitic but general contagious diseases which frequently prove so disastrous to the human race. A comparison of the respective phenomena which characterise them tends to show that the parasitic and non-parasitic affections are more intimately allied than might be supposed. Both sets of diseases are due to the occurrence of spreading changes of a general character throughout the body, leading to grave alterations in the character of its principal fluids. These changes in the ordinary exanthemata are not such as to lead to a birth of independent organisms within the blood and tissues;* whilst in other affections they are such as to entail the birth of a multitudinous progeny of independent units. Partly because the changes which lead to this birth are probably more complete, and more antagonistic to the ordinary vital processes—though even more on account of the enormous powers of reproduction possessed by such new-born independent units—the higher organisms which are thus affected are sure to die. This fatality of the general parasitic diseases is precisely what might have been anticipated.

It would, therefore, follow from such a view (1) that the development of the parasites in these latter affections is altogether secondary, in order of time, to the blood-changes by which they are produced; and it would seem to indicate (2) that, even in cases of the spread of these diseases by contagion, the contagious element, whether living or not living, operates by its power of initiating similar changes—which in their turn may or may not cause the evolution of new organisms. That is to say, we have, at first, always to do with a mere contact-action, and not, even in the case of general parasitic diseases, with direct multiplication of the infecting agent itself.

Already existing evidence is thoroughly harmonious with such views.

In the first place, it is admitted even by those who are pure contagionists, that a blood-change is the primary and necessary initiator of such a disease. Thus, M. Robin says:—"Les circonstances qui paraissent favorables au développement de la Muscardine sont celles qui ont pour premier résultat une altération des humeurs ou des organes de l'animal vivant, et c'est à la suite de cette altération que le parasite se développe. *** Le développement du *Botrytis* est donc bien plutôt consécutif aux modifications des humeurs que cause de celles-ci." And then he continues:—"Les faits d'introduction artificielle des spores ne sont pas en contradiction avec ce qui précède, car la pique pratiquée pour introduire le mycélium et les spores est suffisante pour déterminer ces modifications des humeurs, d'abord localement, puis peu à peu dans toute l'économie."†

We know, moreover, that the blood invariably yields an acid reaction by the time that the first organisms appear in it.

The second point is best established with reference to the phenomena of that fatal epizootic disease amongst cattle, which is commonly known by the name of the "blood" or "sang de rate." In connection with this subject, the researches of M. Davaine are of the highest value. Although this disease is characterised by the presence of multitudes of Vibrio-like organisms in the blood, still these differ from the Vibrios of ordinary putrefactions. Whilst the disease is always capable of being reproduced in a previously healthy animal by the inoculation of some of the fresh blood of an animal that has recently died of the disease, the blood of such an animal loses its powers whenever it becomes putrid.* This has been established by multitudes of experiments. A drop of the diseased blood inserted beneath the skin of rabbits was found to be always sufficient to reproduce the disease; whilst a drop of ordinary blood, after it had become putridified and swarming with *Bacteria*, when similarly introduced, generally produced no effect. Again, whilst rabbits which had been fed upon the fresh organs (livers) of animals which had recently died of the "blood", almost invariably became affected by the same disease, and soon showed myriads of the characteristic organisms in their blood; of other animals which were made to swallow similar quantities of liver, after it had become quite fetid (and therefore swarming with *Bacteria*), only one out of eight died. This latter animal was found to have suffered from an inflamed lung, and even in it no trace of organisms was to be met with in the blood.

These experiments seem only explicable on the assumption that, in the cases where the "blood" was communicated to other animals by inoculation (either subcutaneously or by the stomach), the disease was communicated not so much by the direct multiplication of the stock of inoculated organisms and their spread throughout the body, as because some of the inoculated matter (either fluids or organisms) had the power of setting up certain changes of a spreading character, which soon sufficed to produce a condition of blood similar to that usually preceding the development of organisms in this disease. If the organisms acted *quâ* organisms, and not as ferments or producers of spreading chemical changes in the fluids of the body, then we should necessarily expect that other more or less similar organisms would also be capable of multiplying themselves, and of producing general parasitic diseases. This, however, is notoriously not the case, as we pointed out when speaking of flacherie, and as we have now further attempted to prove. We can only conclude, therefore, that in "the blood", in flacherie, and other similar affections, the contagious element acts as a mere dead ferment might do in inciting blood-changes; and that the organisms which are subsequently found in the infected animal are, for the most part, the products of a new birth which has taken place in the altered fluids.†

Again, all that has been said with reference to these general diseases is abundantly borne out by what we know of local parasitic affections. Here, too, the general or predisposing causes represented by the state of the parts themselves, seem to be just as important as the action of the special contagion—indeed, in multitudes of cases, the action of the special contagion is assumed rather than proved to exist. Just as muscardine can be apparently "spontaneously" engendered in almost any caterpillars which are placed under certain conditions, so does the Fungus-growth which is characteristic of "thrush" manifest itself on the tongue and adjacent parts of children under certain conditions, or even on that of adults in the last stages of lingering illnesses. The thoroughly healthy silkworm is, moreover, almost proof against contagion, just as the thoroughly healthy child or man is proof against the

induced. Thus M. Guérin-Ménéville rubbed some *Botrytis* against a chrysalis; and, on examining the blood of the same animal on its first appearance as an imago, he found multitudes of the characteristic elongated bodies in its blood; and, on its death several days afterwards, these were observed to have become ramified and much increased in size (p. 580).

* M. Davaine says: "Dans les grandes chaleurs de l'été, lorsque le thermomètre marquait de 28 à 32 degrés centigrades, j'ai vu disparaître la faculté dont il s'agit en quarante ou cinquante heures, une fois en trente-cinq heures." (*Compt. Rend.*, 1864.)

† Other instances of a similar nature are known. Thus it has been ascertained by M. Vulpian (*Journal de la Physiologie*, vol. i.), that the insertion of a small portion of cyclamen-root beneath the skin of frogs produces a local irritation and putrefactive process, which after a few days is followed by signs of languor and great debility in the animals operated upon, and is succeeded by a speedy death. Both during life and after death, such animals possess myriads of *Bacteria* in their blood. And yet the disease is not produced by the mere presence of *Bacteria* in the local seat of irritation. I have several times injected one or two drops of a fluid swarming with *Bacteria* beneath the skin of frogs, without producing any such effects, or leading to the subsequent presence of *Bacteria* in the blood. The cyclamen doubtless sets up peculiar local changes, which are capable of spreading, so as to produce a general disease in which the blood as well as other parts of the body are affected; and so far there is an agreement between such a process and pyæmia in the human subject after wounds or operations. In the latter case, however, the blood-changes are not such as to lead to the evolution of *Bacteria*; whilst, in the case of frogs inoculated with cyclamen-root, the changes in the blood do lead to the evolution of organisms.

* See *Trans. of Pathol. Soc.*, 1869, p. 426.

† *Végétaux Parasites*, p. 585. Mere contact of the spores with the skin, when the animals are at all predisposed, seems to suffice. General blood-changes are soon

attacks of *Oidium albicans*. When from lowered health, however, the blood of the silkworm tends to acquire an acid reaction, or the mucous membranes of the infant or of the man assume such a condition, then the respective organisms may appear, and general or local morbid conditions may be established.*

The attempts which have been made to inoculate healthy plants with the various Fungus-parasites to which they are liable, cannot be regarded as being other than very unsuccessful as compared with what it should have been if "contagion" were the only means of spreading and perpetuating these parasitic productions. Thus many cruciferous plants, such as garden-cress, mustard, and shepherd's purse are liable to be affected by a white rust, named *Cystopus candidus*; and De Barry made an extensive series of experiments with the view of ascertaining the means by which plants became affected with the parasite. These experiments showed very conclusively, as De Barry himself thought, that plants are not infected either through their spongioles or through their leaves or stems;† though a subsequent series of experiments induced him to believe that it occasionally took place through the seed-leaves or cotyledons of the embryo plant. The results of these experiments may be given in the words of Mr. M. C. Cooke, who says:‡—"In two series of plants cultivated at different periods from good seeds, one hundred and five plants which had not received the water impregnated with zoospores upon their cotyledons, vegetated without any indications of the parasite. Amongst the eighteen plants which were inoculated by watering the cotyledons, four only were not attacked by the parasite, fourteen bore the 'white rust.' In six of these it did not extend beyond the cotyledons; in the others it also appeared on the stem and leaves." Thus eight only out of eighteen plants which were placed in the most favourable conditions which could be devised for bringing about inoculation were found to have been affected—since it does not seem fair to include those cases in which the contamination did not spread beyond the seed-leaves. Moreover, De Barry's own experiments tend to show that, if a plant has not been infected in the first stages of its growth, such parasitic disease cannot be subsequently induced by inoculation. So that if the disease can be determined almost at will by submitting previously healthy plants to unhealthy conditions, it would tend to show that such affections can originate *de novo*, by reason of pathological changes taking place in the plant. Nay, more; unless De Barry can show that the tissues of plants, when once infected through their seed-leaves, subsequently continue to present evidences (which they do not) of their tissues being permeated with a rudimentary mycelium, afterwards capable of bursting out into more active growth in all parts of the plant, his experiments cannot be said to have given much real support to the supposition that such diseases are propagated only by means of contagion and pre-existing spores. In fact, the advocates of inoculation are themselves compelled to give up the notion that the parasitic fungi are produced from ordinary spores, since in the majority of cases these are far too large to enter through the stomata or through the spongioles of the roots. A determined upholder of the doctrine of the spread of these diseases by contagion (Mr. C. Cooke, in *Microscopic Fungi*, p. 9), whilst admitting that "the spores themselves have certainly not yet been traced traversing the tissues of growing plants", says that the difficulty may be lessened "when we remember that what are termed the spores which are discharged from the cups are not the true spores, but bodies from which smaller seed-like vesicles are produced." Yet, as the same writer most candidly adds, "even then there will be much need of an active imagination to invent hypotheses to cover the innumerable difficulties which would encounter their passage through the vessels of the infected plants."

The experiments of Dr. Spring, § moreover, show a similar failure in many attempts to produce a direct inoculation of hen's eggs, even though the inoculation was made with portions of a fungus which had been produced in an uninjured egg. Very rarely, indeed, did a growth start from the seat of inoculation, though frequently an entirely different form sprang up at the opposite extremity of the egg, in a region which was apparently quite beyond the reach of contamination. And, again, such facts are rendered all the more significant by reason of the researches of Harless (Robin, p. 559), showing that fungi may be made to appear, almost at will, in eggs which are exposed for a few days in the chamber

of an incubator saturated with moisture, and maintained at a temperature of 100-104 deg. F. Under these conditions, the customary exhalations and evaporations of fluid cannot take place from the egg, so that the embryo dies whilst lower organisms appear.

The facts narrated in this communication, and multitudes of others of a similar nature that might have been quoted, constitute a body of evidence which is thoroughly harmonious with the results of my previous observations and experiments. It is not a little contradictory to see many of those who have wholly cast on one side the old developmental doctrines of Haller and Bonnet, still confidently pin their faith to a derivative doctrine, which always found its principal support from these now repudiated developmental theories. The inconsistency is, however, increased when we find the doctrine of Panspermism advocated not only by professed vitalists, but by leading evolutionists who do believe in the original occurrence of an independent production of life, though they reject the notion of the continuance of such evolution. They have never attempted to explain why these natural life-evolving laws, whose original existence they postulate, should have ceased to operate in the present day. They assume the omnipresence of germs; they assume that such hypothetical germ can exist for an indefinite period in a latent state; that they can resist degrees of heat which are fatal to all known germs, and that they possess powers of penetration such as known germs do not possess—they will make all these assumptions upon assumptions in order not to believe that which is in accordance with the general teachings of science and with their own doctrines in particular, that which is testified by experiment, and also that which almost every unbiassed microscopist may learn by direct observation.

DEATH FROM SUFFOCATION WHILE INHALING CHLOROFORM: IMPACTION OF FALSE TEETH IN LARYNX.

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I FEEL constrained to offer to the profession, through the JOURNAL, a report of the circumstances attending a fatal event that happened during the administration of chloroform, as I deprecate the recurrence of such a lamentable and unfortunate casualty; and am not personally aware that an accident similar in character and result has been recorded in the practice of any other surgeon.

A patient of mine, aged 46, was about to undergo the operation of ovariectomy, which I arranged to perform in conjunction with Messrs. Pridgin Teale and Oglesby of Leeds, on whose co-operation I congratulated myself. The usual precaution was taken of insuring an almost empty stomach, by ordering her to have some beef-tea and brandy about 5.30 A.M., and a second dose of brandy immediately before the time fixed for the operation, at 9.30 A.M. Chloroform was being administered by Mr. Oglesby; but, before the patient had arrived at the stage of insensibility warranting our proceeding with the operation, the breathing became laboured, the face livid, the pulse failed, and unintelligible symptoms of suffocation supervened, differing considerably from those attending chloroform-asphyxia. She made one or two faint efforts to cough, as is usual previously to the accession of the vomiting commonly produced by the inhalation of chloroform; and this was accompanied by a discharge from the mouth of frothy, bloody mucus, which we attributed to a slight bite of the tongue. Chloroform was instantly withheld. Mr. Oglesby drew out the tongue with a pair of forceps which he had at hand for the purpose, and Mr. Teale and I applied Silvester's method of artificial respiration. As this treatment failed to effect any manifest improvement in the patient's condition, Mr. Teale inserted his finger into the pharynx, and found a hard intrusive substance lodged in the glottis. This was immediately extracted, and proved to be a metal-plate, to which false teeth were attached. The plate, having fallen from the roof of the mouth, was drawn into the glottis by inspiration, and could not be ejected from its position, as the patient was not sufficiently conscious to exert any voluntary effort, or to communicate to us the cause of obstruction in breathing. We continued artificial respiration for forty minutes, being induced to persevere in our resuscitative efforts by the encouraging gasps which the patient made at short intervals during the first ten minutes; but the heart ultimately failed to respond to the stimulus given by the inspiratory movements, and death ensued.

Post Mortem Examination Twenty-three Hours after Death.—The heart was thin-walled and flabby; the right ventricle was dilated, and contained a small quantity of fluid blood. The mitral and aortic valves were slightly altered by deposit, but not sufficiently so to give rise to

* How important such a modification may be, in enabling evolutionary changes to occur in certain fluids, is to be surmised, as we have previously hinted, from the often quoted experiments of Dutrochet, who found that white of egg diluted with distilled water remained for more than a year without becoming covered by mould; whilst, by rendering it slightly acid, a crop of these organisms became developed in less than eight days.

† See, on this subject, also M. Fée, *Sur les Cryptogames des Ecorces Exotiques Officinales*, p. 13.

‡ *Microscopic Fungi*, 2nd ed., 1870, p. 134.

§ *Bullet. de l'Acad. Roy. de Belgique*, 1852, t. xix, p. 573; or abstract in Robin's *Végétaux Parasites*, pp. 545-54.