THE GRAFT THEORY OF DISEASE

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GRAFT THEORY OF DISEASE

BEING AN APPLICATION

OF

MR DARWIN'S HYPOTHESIS OF PANGENESIS

TO THE

EXPLANATION OF THE PHENOMENA

OF

THE ZYMOTIC DISEASES

BY

JAMES ROSS, M.D.,

WATERFOOT, NEAR MANCHESTER.

LONDON:

J. & A. CHURCHILL, NEW BURLINGTON STREET.

1872.

PREFACE.

The nature of the following work will be best understood by a brief account of the progress of its subject-matter in my own mind. When reading a few years ago Mr. Darwin's great work on "Animals and Plants under Domestication," I was particularly struck with the similarity between the effects produced upon a vegetable stock by grafting upon it a scion of another variety, and the phenomena of the contagious diseases. It was some time after this that I listened with rapt attention to Professor Huxley's brilliant address delivered before the British Association at Liverpool, 1870; and more especially to that part of it which deals with the Germ Theory of Disease. For a time I was inclined to think that the particles which Professor Chauveau and Dr. Sanderson had demonstrated to be the active part of contagious matter were "parasites in the zoological sense," and that my former idea would have to be abandoned.

About this time, however, I became acquainted with the results obtained by M. Reverdin's method of grafting a bit of healthy skin upon a chronic ulcer, and I became more than ever convinced that the three processes of vegetable grafting, animal grafting, and the communication of the zymotic diseases, were essentially one. If, then, the analogy between these processes were real, the contagium particles must be "heterologous growths." That these particles are only modified portions of the individual from which they have become detached now became my dominant idea; it is the central one in the following work, around which all others cluster, and to the establishment of which is directed, either directly or indirectly, all the argumentation it contains. This explains the method I have adopted in discussing the subject. I have not endeavoured to examine the question in the most systematic manner possible, but have accepted the form imposed upon me by the development of the different bearings of the problem in my own mind, and by the state of the controversy with regard to it at the time I began the study. I may also add that I have endeavoured as far as possible to treat the subject deductively, being convinced that pathology is hereafter destined to become a deductive science.

I have made acknowledgments in the body of the work to the authors laid under contribution, and it will be seen that I am principally indebted to the writings of Mr. Darwin, and to the profound works of Mr. Herbert Spencer. In making this avowal, I am fully conscious that I only pay a sorry compliment to my own production.

A word of apology is due from me for writing upon a subject like pathology, with which my acquaintance must necessarily be very limited. Ever since entering the Profession, about nine years ago, I have conducted large country practices, in which I have been obliged to expend more power in muscular than in nervous action, and during that time I have had scarcely any opportunity of becoming practically acquainted with morbid tissues. Under the circumstances my readers must not expect from me great accuracy in the details of pathology. It must not be thought that in mentioning these facts I wish to claim exemption from criticism. On the contrary, I court honest criticism as the only means which will enable me to elaborate still further the theory I have advanced. If the principles I advocate are erroneous, by all means let them be condemned; but my readers who are more favourably circumstanced than I am, should mentally correct any errors in minor particulars, and endeavour to preserve whatever is true in the doctrines advanced.

When I began to study this subject my intention was to write a paper which might appear as a few articles in one of the medical journals. Knowing that Dr. Anstie is not more noted for his courage in exposing whatever is unreal and pretentious, than for his generosity in bringing to the surface whatever is meritorious, especially in the works of the younger members of the Profession, I ventured to send him a few months ago a long paper, with a request that he would advise me how to bring it before the public. While specially guarding himself against the idea that he agreed with all my views, he thought the paper of sufficient importance to warrant separate publication. The paper has since been rewritten, and greatly enlarged, and I hope made more worthy of being submitted to the judgment of the Profession. It is, however, so much altered, for better or for worse, that Dr. Anstie is not even committed to a general approval of it in its present shape. I beg to return Dr. Anstie my sincere thanks for finding time, amidst his many other duties, to peruse a long manuscript, to oblige one who is not even personally acquainted with him.

JAMES ROSS, M.D.

Waterfoot, near Manchester,
May 4th, 1872.

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CHAPTER I.

THE GERM THEORY.

THE theory of fermentation has, from the time of Van Helmont, exercised a very important influence upon both physiological and pathological doctrines. It is necessary, therefore, to allude briefly to the ideas which prevail at the present day with regard to the nature of ferments, in order to trace the influence which these exercise upon pathology. There are two main hypotheses regarding the nature of ferments: according to the one, their action is of a chemical nature; while, according to the other, their action is dependent upon the growth and development of living organisms. The chemical theory was first advanced by Stahl; and, after successive modifications, has received its modern statement from Baron Liebig. The hypothesis of Liebig is more a chemico-physical than a purely chemical one, and embraces several processes which are more or less analogous to fermentation. It is founded upon the physical law enunciated by Laplace and Bertholet, that when a molecule is put in motion by any force, it is able to communicate its motion to another molecule with which it may come in contact. Liebig's theory is stated in the following terms:—"Contact with a substance itself undergoing the process of decay is the chief condition of decay for all organic substances which do not possess the power of combining with oxygen at common temperatures."* This theory embraced not only the process of decay or eremacausis, but also the processes of fermentation and putrefaction, and it was extended by Liebig and others to the explanation of the phenomena of epidemic diseases. According to this theory, the cause of epidemic diseases was a malaria, consisting of organic matter in a state of motor-decay, which, when absorbed into the system, was supposed to be capable of communicating to the fluids of the body the motion it was itself undergoing. The latter doctrine lent itself to the humoral pathology, which regards all the changes in disease as occurring primarily in the fluids; and it also followed that the malaria must be detected, and their effects upon the body investigated mainly by chemical means. So far as fermentation and putrefaction are concerned, this hypothesis is abandoned for the rival vital theory; and, with the gradual growth of solidism in patho-

* "Familiar Letters on Chemistry," p. 237.

logy, it is being rapidly superseded in its application to the explanation of the phenomena of disease. We shall therefore pass on to a brief consideration of the vital theory of fermentation and putrefaction.

The fact that organic liquids undergoing putrefaction become crowded with low forms of vegetable and animal organisms, is familiar to all. For a long time it was supposed that these organisms were generated from the matter undergoing putrefaction; and it was only about two centuries ago that the opposite doctrine, that "all life comes from preexisting life," was first propounded. The experiments undertaken to decide between these conflicting hypotheses with regard to the origin of life led to very important collateral results. Passing over the labours of the earlier experimenters, we come down to the year 1837, when the experiments of Schwann proved that when a boiled infusion is protected from ordinary air and only supplied with such as had passed through red-hot tubes, putrefaction does not ensue. This showed that a material in the air which is necessary to putrefaction could be destroyed at a sufficiently high temperature. A short time previously, Cagniard de la Tour discovered the yeast plant; and this discovery, in so far as it showed that fermentation was accompanied by the development of organic forms, assimilated the processes of fermentation and putrefaction, and suggested the question whether the living organisms were not the cause of these processes. A few years afterwards, Helmholtz conducted a series of beautiful experiments, which showed that the substance which excites fermentation and putrefaction does not pass through membrane, and this proved it to be neither gas nor diffusible fluid. In 1854 Schroeder and Dusch showed that if a boiled infusion is protected from all air except such as had previously passed through a layer of cotton wool, no living forms are developed. From these experiments it was inferred that what was arrested by the cotton wool must have been solid particles, and sometime afterwards all doubt upon this point was set at rest by the labours of Pasteur. He subjected the cotton wool which had acted as a strainer to microscopic examination, and found numbers of solid particles, some of which were clearly recognised as possessed of life. He also showed that these particles when planted in a suitable medium gave origin to living forms, and from this it was inferred that they were germs; and, finally, he proved that no other change had taken place in the air in its passage through the cotton wool, except that it was deprived of germs; and that this was the sole cause of its incapacity to generate life. The labours of Pasteur, therefore, have placed it beyond doubt that the growth and evolution of microscopic organisms are the cause—either directly or indirectly—of fermentation and of putrefaction; and the only question which can arise is, what are the intermediate links by which the cause produces the effect? For our purpose, however, it is not necessary to determine whether these organisms act by transforming the infusion in their substance during the process of nutrition; or simply by their physical properties; or, as is most probable, by giving origin to a substance which acts upon the infusion as emulsin does upon amygdalin, the fact which concerns us most is that the processes of fermentation and putrefaction are dependent upon the growth and reproduction of living forms. We shall now proceed to show the influence which this theory has had upon our ideas of disease, and more especially of the zymotic diseases.

About the middle of the seventeenth century the idea was advanced by Hauptmann and Kircher that epidemic diseases were caused by the presence in the atmosphere of invisible germs, which on entering the body exercise a deleterious influence by the development within it of parasitic life. By the successive labours of Leuwenhock, Spallanzani, Bonnet, Van Siebold, Van Beneden, Kuchenmeister, Leuckart, and others, it is now amply proved that a great many diseases are caused by animal and vegetable parasites. So far, therefore, as the diseases which are termed parasitic are concerned, the germ theory is triumphant. But the question is still open how far this theory can be extended to the explanation of other diseases, and more especially of the contagious diseases. The evidence in favour of the germ theory of the contagious diseases may be usefully considered under two heads—the collateral and the direct evidence. Under the first head I shall notice the argument in favour of this theory, which may be derived from more or less analogous processes which are known to be caused by living organisms; and under the second I shall briefly allude to the special researches which have been undertaken with a view to determine this question.

With regard to the analogical part of the evidence, it is now fully proved that certain diseases of plants and of the lower animals, which are more or less similar to the contagious diseases of man, are of parasitic origin. Numerous examples might be given, but it will be sufficient to mention the smut of wheat and the potato blight amongst plants; and the now well-known case of the pebrine of the silkworm amongst animals. In so far as these diseases are similar to the zymotic diseases of man, and more especially in their contagiousness, the evidence is in favour of the germ theory being extended to the explanation of the latter. On the other hand, these parasitic diseases of plants and animals are very dissimilar in many respects to the contagious diseases of man. We do not find any of them presenting a definite cycle of changes similar to the zymotic diseases. None of them, for instance, is similar to small-pox, a disease which runs a certain course, and leaves the majority of those affected in a state approaching to health, and insusceptible to another attack of the disease. And as it is very probable that the definite course which the zymotic diseases run, and the insusceptibility of the individual who recovers to a second attack, are more characteristic features of those diseases than even their contagiousness, this argument from analogy on the whole tells against the germ theory. But, indeed, it has no great evidential value either way.

It has already been remarked that the chemical theory of fermentation and putrefaction advanced by Liebig had exercised a great influence upon pathological doctrines: let us now notice briefly the extension by analogy of the germ theory of these processes to the explanation of the phenomena of disease. Professor Lister has the merit of being the first who applied the germ theory of putrefaction to explain the formation of putrid matters in the living body. It was well known to surgeons that wounds exposed to the influence of the atmosphere were more liable to inflame and to produce unhealthy pus than those which were protected from it. According to the chemical theory of putrefaction this was supposed to be owing to the deleterious action of oxygen upon the tissues. But a few years ago Professor Lister was led, from a consideration of the germ theory of putrefaction, to think that the formation of putrid pus might be owing to the introduction into the wound of germs floating in the atmosphere; and upon this he founded what is called the antiseptic treatment of disease. The striking results of this treatment, supplemented as they were soon after by the brilliant experiments of Tyndall, which showed that the dust of the atmosphere mainly consists of organic particles, drew the attention not only of the learned, but of every well-informed general reader to the germ theory in its relation to disease, and the danger is that a hasty generalization may be made extending the theory further than the facts of the case warrant. It is necessary to make a distinction between the practical and the theoretical importance of an event or of a discovery. An event of no practical moment, such as a convulsion in a guinea-pig, may bring about a revolution in science; whilst another event of no scientific value, such as the decree of a ruler, may bring about a revolution of an empire. Viewed practically, then, it is almost impossible to exaggerate the importance of Professor Lister's extension of the germ theory to the domain of surgery; but viewed theoretically, and this is the aspect with which we have to do here, its importance is not very great. A similar remark may be made with regard to the experiments of Professor Tyndall. These experiments gave a finish and completeness to the germ theory, and did more probably than anything else to bring it into public favour; but the logical position which must be assigned to them in the chain of evidence is a very subordinate one. Let us now endeavour to estimate the theoretical import of the germ theory, as applied to the formation of putrid pus, with a view to determine how far it justifies us in extending the theory to the explanation of the zymotic diseases. No one, I think, who has read

Professor Lister's writings upon this subject will doubt that, if the germ theory of putrefaction be accepted as true, he has proved that the deterioration which takes place in pus, into which a little air has been admitted, is caused by the germs which pass in along with the air. But this admission, which I make most unhesitatingly, simply amounts to this, that these germs excite putrefaction in an albuminous fluid (that of the pus) which, even when enclosed in a cavity, is practically outside the body; just as they excite putrefaction in an organic infusion which is not in contact with the body at all. When the pus of an abscess begins to putrefy, very important events ensue. The walls of the cavity become inflamed, and great changes take place in the blood and other parts of the body, but the contents of the abscess are so complex that it is impossible to disentangle the share which each component part takes in the production of the succeeding events. It cannot be determined how much is to be attributed to the corpuscles, altered as they no doubt are by the increase of inflammatory action in the surrounding tissues, how much to the absorption of the putrefying fluid, and how much to the numerous vibrios which have descended from the germs which have been introduced into the cavity from without. And if the share which these vibrios take in the production of the events which succeed to the putrefaction of the pus cannot be disentangled, it is quite impossible to argue from such a case to that of the zymotic diseases; in short, the argument from analogy is in this case worthless. It is almost unnecessary to point out that no argument for the extension of the germ theory to the zymotic diseases can be founded upon Professor Tyndall's experiments. He has proved that the greater part of the dust of the atmosphere consists of organic particles, but he has not proved that these particles are living, and even if he had, he has not proved that they are living germs capable of giving rise to organic forms in a suitable medium, nor has he given experimental evidence that these particles are operative in the causation of disease. Other minor arguments from analogy might be noticed, but on examination they will be found to be more worthless, if possible, than those already discussed; and since each of them will be answered, implicitly if not explicitly, in the following pages, we shall proceed to notice the special researches which have been undertaken to determine this question.

The researches which bear most directly upon this subject are those which have been directed to the detection and isolation of the active part of contagious matter in its passage from one individual to another, in order to determine in the first place its physical and chemical characters. The experiments of Mr. Crookes upon Cattle Plague showed that when the breath of an infected animal was passed through cotton wool, the wool retained part of the virus, "a fact which was demonstrated by inoculating a sound animal with the wool which had been thus exposed. The animal received the disease."* Some of this wool was examined by Dr. Beale microscopically, and compared with a portion of similar wool which had not been exposed to the breath of the diseased animal, when particles were detected in the former which could not be found in the latter. Since, however, it was not possible to isolate these particles from other matters, it could not be concluded that they were the cause of the propagation of the disease. But the experiments of M. Chauveau, of Lyons, extended and confirmed by Dr. B. Sanderson, have been attended with greater success with regard to infective fluids. M. Chauveau took as the basis of his inquiry into the nature of the contagious process the physical characters of vaccine. Vaccine lymph consists of three elements —an albuminous liquid, bodies analogous to pus corpuscles, and particles not exceeding the $\frac{1}{20000}$th of an inch in diameter. By a series of beautiful experiments, M. Chauveau proved that the activity of the lymph depends upon the particles. These researches have been extended by M. Chauveau and by Dr. B. Sanderson to other contagious diseases, so that it may be considered as proved that the contagious properties of vaccine, small-pox, sheep-pox, and farcy, depend upon particles less than the $\frac{1}{20000}$th of an inch in diameter. Dr. Sanderson also thinks that, by inference, it is in the highest degree probable that the contagious matter of scar-

* "The Microscope in Medicine," by Dr. Beale, p. 132.

latina, rubeola, cholera, and syphilis, consists of particles of similar physical characteristics, although this has not as yet been determined experimentally. Syphilis, however, is a disease which differs in several important particulars from the contagious diseases. It is propagated by inoculation, and not by contagion, and runs a very chronic course, instead of undergoing a rapid cycle of changes like the contagious diseases; hence it is rather hazardous to infer that the physical characters of the active part of its virus are in all respects similar to that of the contagious diseases. With the exception of syphilis, however, these experiments render it highly probable that the active part of virulent fluids resides in these particles; the physical and chemical characteristics of which are summed up in the following terms by Dr. Sanderson:—"They are spheroidal, transparent, of gelatinous consistence, of density nearly equal to that of the animal liquids in which they float, and are mainly, but perhaps not exclusively, composed of albuminous matter."* Such, then, being the physical characters of the contagium of infective fluids, it seems certain that, when these liquids are dried up by evaporation, the particles may be wafted by currents of air to great distances from the soil upon which they have been generated. It seems, therefore, probable that the active part of effluvia consists of contagium particles which are

* "Twelfth Report of the Medical Officer of the Privy Council," p. 255.

floating in the atmosphere. Having now come to the conclusion, on evidence presenting a very high degree of probability, that contagium is "particulate;" and having determined the physical and chemical characters of these particles, the question which presents itself is, Are they living?

When an infective liquid, such as vaccine lymph, is examined microscopically, the particles are seen to exhibit very active movements, and, according to Dr. Sanderson, tend "to elongate into rod-like bodies endowed with a peculiar progressive and oscillatory movement."* In these respects they are very similar to the organisms in putrefactive liquids termed vibrios and bacteria; hence Dr. Sanderson, following Béchamp, includes all these particles under the general term "microzyme." Mycrozymes are defined by him as "living particles which in their earliest state do not exceed the $\frac{1}{20000}$th of an inch in diameter, but subsequently elongate into rods." A word of caution is necessary in this place lest the term microzyme, under which are grouped together both contagium particles and bacteria, lead us to prejudge the next question which has to be determined with regard to the former. It might be supposed because bacteria are proved to be independent organisms, that the contagium particles are so likewise. But this has by no means been determined as yet, and whatever advantages may

* "Twelfth Report of the Medical Officer of the Privy Council," p. 245.

be derived from the use of a general term to embrace both, we must be careful lest it mislead us. For my own part I think that Dr. Sanderson has come rather too hastily to the conclusion that vaccine particles do elongate into rods. I have frequently watched the movements of these particles in fresh vaccine lymph, and although they were very active, I have not seen them elongate into rods. They appear to me to have a tendency to cluster together into groups of three and four, and sometimes into much larger groups, reminding one of the clustering of the white blood corpuscles, as described by Virchow.* But although the particles of fresh lymph have no great tendency to elongate into rods, so far as I can make out, yet it is possible that these rods are formed if the lymph is kept for a few days. But here another difficulty presents itself. How can we be certain even if these rods appear in the lymph after a few days, that they are the descendants of the lymph particles, and not of germs which have found their way into the lymph from without? Dr. Sanderson figures † a group of microzymes in vaccine: at one part "the corpuscles are spheroidal, at other parts they are more or less staff-shaped." But the lymph was first diluted with distilled water and then kept a week in a capillary tube. He does not

* "Cellular Pathology," by R. Virchow, p. 151.

† "Twelfth Report of the Medical Officer of the Privy Council," p. 245.

say that any precautions were taken to destroy germs which might have adhered to the inside of the capillary tube, or to prevent germs settling upon the lymph before it was taken into the tube. The particles which are still spheroidal may be the vaccine particles, while the staff-shaped bodies may be descendants from germs which have found their way from without. I do not at all wish it to be thought that I am placing my opinion upon a question of fact on a level with that of such an accomplished observer and experimentalist as Dr. Sanderson, but I think that he himself will admit that even if contagium particles exhibit movements similar to bacteria, and like them elongate into rods, this does not prove that, like the latter, they are independent organisms; and he will also probably admit that the caution here given is by no means unnecessary. This digression has caused us to lose sight for a time of the question under discussion, which is, whether the contagium particles are living; of course if they are independent organisms like bacteria, there can be no doubt that they are in possession of life; and if, on the other hand, they are not independent organisms, the fact that they have been recently detached from a living body, and that they exhibit the physical and chemical characters of protoplasm, along with the active movements peculiar to living units, are sufficient indications that they are possessed of life. The question then stands thus: Professor Chauveau, by experiments similar in principle to those of Helmholtz upon putrefactive fluids, has proved that contagium is solid; so far, then, the former has brought the theory of contagium to the same stage of completeness that the latter did the theory of the substance which excites putrefaction. But Professor Chauveau and Dr. Sanderson have advanced the theory of contagium somewhat further than this, or to speak more exactly, they have done so for a certain number of contagious diseases, the experimental evidence for the remainder not being yet completed. They have examined the contagium microscopically, and found it to consist of particles which were recognised as living. This brings the theory of contagium to the same stage to which the first microscopic observations of Pasteur had brought the theory of putrefaction, when he found upon the cotton wool, which had acted as a strainer, particles which were recognised as living. If, now, the second and third steps which Pasteur had taken with regard to the substance which excites putrefaction, could be taken with regard to contagium, the theory of the latter would be as complete as that of the former. In other words, before the germ theory of contagious diseases can be fully accepted, it has yet to be proved that the contagium particles when planted in an organic infusion external to the body are competent to give rise to living forms, and finally to devise experiments which will show that nothing can give rise to a contagious disease except contagium particles. Let us now proceed to notice the researches which have been undertaken to settle these points, and see how far they conform to the conditions of proof just laid down.

The stage of the inquiry at which we have now arrived is to determine whether the contagium particles are independent organisms capable under certain conditions of giving origin to living forms, or are merely detached portions of a more complex organisation. The question is admirably stated by Professor Huxley. "Now," he asks, "arises the question, are these microzymes the results of homogenesis or of xenogenesis; are they capable, like the torulæ of yeast, of arising only by the development of pre-existing germs, or may they be, like the constituents of a nut-gall, the results of a modification and individualisation of the tissues of the body in which they are found, resulting from the operation of certain conditions? Are they parasites, in the zoological sense, or are they merely what Virchow has called heterologous growths?" * The analogies which point to the parasitic origin of the contagious diseases have already been noticed, and it has been found that not much importance is

* See "Address to the British Association at Liverpool, 1870." I wish to express my obligations to this magnificent address, and to Professor Huxley's neat little essay "On Yeast," which appeared in the *Contemporary Review* for December, 1871, for much which appears in this chapter. I am only sorry that I have not been able to borrow more of the exquisite arrangement and lucidity of expression which so eminently characterise all the writings of this gifted author.

to be attached to them. It is evident, however, from some remarks in his address to the British Association, that Professor Huxley is inclined to adopt the theory of the parasitic origin, that is the germ theory of the contagious diseases; but his language is very cautious, and he justly adds, "it appears to me that this great problem will have to be solved for each zymotic disease separately, for analogy cuts two ways." But Dr. B. Sanderson has given his adhesion to the theory of the parasitic origin of the zymotic diseases much more unreservedly. "With reference," he says, "to their (microzymes) mode of action, we have examined into those considerations which seem to render it probable that they are organised beings, and that their powers of producing disease are due to their organic development, and we have adopted this doctrine as the only one which affords a satisfactory explanation of the facts of infection, and in particular of those which tend to show that within the body of the infected individual the particles of contagium rapidly reproduce themselves, while out of the body they are capable of resisting for very long periods the influences of conditions, which if not restrained by organic action, would produce chemical decomposition." * Dr. Sanderson in this passage not only gives his adhesion to the opinion that contagium particles are independent organised beings, but he gives what he

* "Twelfth Report of the Medical Officer of the Privy Council," p. 255.

regards as the principal argument for maintaining this opinion, namely, that this hypothesis affords the only satisfactory explanation of the facts of infection. If, however, it can be shown that another hypothesis will give an equally satisfactory account of the phenomena of infection, this argument will be rendered valueless. But instead of proceeding to construct such an hypothesis at present, I shall proceed to notice very briefly the observations and experiments which have been made with the view of proving that infective liquids owe their activity to living organised beings.

At the threshold of this part of the inquiry two questions present themselves. If these particles are independent organisms, it is necessary to determine whether, in the language of Dr. Sanderson, "they constitute a race of more or less similar individuals, each of which springs from and reproduces its like, or germs in which a specific form is wrapped up, capable of developing to the higher organisms from which they spring." * Professor Hallier, of Jena, adopts the latter view, and all his researches have been directed to show that contagium particles have originated from higher specific forms, and are capable of developing into those forms again under suitable conditions. It is unnecessary to state Professor Hallier's theory at length in this place, it will suffice to say that, so far as cholera is concerned, the

* "Thirteenth Report of the Medical Officer of the Privy Council," p. 48.

theory is discredited by the observations of Dr. Lewis, narrated in the "Report on the Microscopic Objects found in Cholera Evacuations, &c.;" while the experiments of Dr. Sanderson, detailed in the "Thirteenth Report of the Medical Officer of the Privy Council," are equally adverse to the theory in its more extended applications. Hence if the particles of contagium are to be regarded as independent organisms, we are shut up to the conclusion that each particle has sprung from a similar particle, and gives origin to similar particles as its successors. With regard to this last hypothesis, there is no experimental evidence in its favour. I have already cautioned the reader not to be led to think that because contagium particles and bacteria are included under one general name that they must be identical in every respect. Both are about the same size, their physical and chemical constitution are more or less similar, they exhibit similar movements, and, according to Dr. Sanderson, both tend to elongate into rods. Reasons have already been given for believing that the contagium particles do not tend to elongate into rods; but even if they did, it by no means follows that because bacteria are independent organisms, the contagium particles must be so also. A pus corpuscle or a white blood corpuscle, when recently detached from a living body, exhibits movements more or less similar to those of an amœba. One of these corpuscles is about the same size as an amœba, it is also very like it in physical and chemical constitution; yet, with all this similarity, it does not follow because an amœba is an independent organism, that either a pus or a white blood corpuscle is also an independent organism. But we are not shut up to a criticism of the arguments which have been adduced to prove that contagium particles are independent organisms with unlimited powers of self-multiplication in a suitable medium, since strong arguments can be advanced to show that this opinion is not true. When fresh vaccine lymph is examined it is found to contain a certain proportion of microzymes; but if the lymph be diluted with water and exposed to the atmosphere the microzymes soon become a thousandfold more numerous; but just in proportion as these microzymes multiply does the lymph lose its specific properties. This proves that the microzymes of fresh lymph and those found in it after dilution with water and exposure for a few days to the atmosphere have widely different properties. Again, the fearful disease caused by poisoned dissection wounds occurs soon after the somatic death of the subject, but before molecular death has taken place; but when bacteria begin to be developed in myriads, the virulence of the contagium abates. Dr. Beale * discusses this part of the subject very ably, and it is therefore unnecessary for me to pursue it further at present. My object is gained if I have succeeded in showing that it has not yet been proved that con-

* "Disease Germs: their Supposed Nature," p. 62, *et seq.*

tagium particles are independent organisms; and that until an experimentum crucis is devised to decide the question, there is as much antecedent probability in favour of the truth of the opposite view, namely, that the particles are living only in the sense of being portions detached from a living body. Until, then, a crucial experiment can be devised to decide between these conflicting hypotheses, our only plan is to explore both to the utmost, and to give our adhesion provisionally to the one which affords the readiest explanation to the phenomena of disease. I shall, however, in the first place endeavour to remove the objections urged by Dr. Sanderson against the hypothesis that contagium is alive only as a part of a living body.

After stating that if contagium is alive it must be so in either of the two senses already indicated, and if not alive its action must be chemical, Dr. Sanderson says, "the distinction which this mode of stating the question implies, between chemical on the one hand and living on the other, if not entirely meaningless, is, at all events, too vague to be used for scientific purposes; for inasmuch as no vital function is known to be performed without chemical change, and as many chemical changes are constantly attended with vital manifestations, neither term can be applied to any process to the exclusion of the other. In short, the only character by which the living can be separated from the not living is that of organic development; so that the question may be considered to lie between those who hold that the particles of contagium are living organisms, and those who attribute their infective properties to their chemical composition." * But this mode of disposing of the third hypothesis is far too summary. It is true that the distinction between chemical on the one hand, and vital on the other, is vague in the sense of being with difficulty definable; but the distinction between organic development and chemical change is equally vague, inasmuch as no organic development can take place without being accompanied by chemical changes, and many chemical changes are accompanied by organic development. The distinctions between physical, chemical, and vital are equally vague. For instance, according to one definition of the physical a distinction is drawn between it and the chemical; according to another definition the physical includes the chemical; while, according to a third, the physical embraces not only the chemical, but the vital also; and, lastly, if by a physical cause is merely meant an invariable and unconditional antecedent, it can be applied to the subjective phenomena of the mind. But although these distinctions are artificial and vague, they are not on that account useless for scientific purposes; on the contrary, a great part of the preliminary operations

* "Twelfth Report of the Medical Officer of the Privy Council," p. 243.

of any science consists in making such distinctions. If this argument had any validity a great many of the inferences made from physiological experiments would be worthless. Suppose, for instance, that a muscle, along with the nerve which supplies it, is detached from a living body, and that certain experiments are carried on with a view to determine the physical and chemical agents which will irritate the nerve so as to cause the muscle to contract. No inferences could be made from these experiments to what occurs in the living body, unless it be admitted that the fact that the muscle has been recently detached from a living body, and that it exhibits movements similar to those it exhibited when an integral part of that body, are sufficient indications that the muscle with its attached nerve are still alive; but in this instance there has been no organic development. Again, when fresh pus is examined under suitable conditions of temperature, its corpuscles exhibit amœboid movements; and these are held to be a sufficient indication that the corpuscles are still alive, although they do not evince any organic development.

Another objection urged by Dr. Sanderson against the hypothesis that contagium particles are alive only in the sense of being portions recently detached from a living body, is that it "is evidently equivalent to the assumption that each species of contagium contains an 'immediate principle' (in the chemical sense) proper to itself, to which its specificity is due."* But if the hypothesis under consideration is equivalent to the assumption that each species of contagium particle contains an "immediate principle," the same assumption will require to be made with regard to the particles concerned in reproduction; since it is only after the concourse of the male and female element that the ovum constitutes a germ; and whatever possibilities may lie dormant in these particles prior to their concourse, they are only alive in the sense of being either integral, detached, or semi-detached portions of a living being. No person, however, thinks himself under any necessity to assume an "'immediate principle' (in the chemical sense)" to exist in the spermatoza of a mouse to which the specific character of the mouse is due; nor do I see that there is any necessity to make such an assumption with regard to a contagium particle on the hypothesis under discussion. These, then, are the arguments by which Dr. Sanderson endeavours to dispose of the hypothesis that the contagium particles are alive only in the sense of being portions detached from a living body, and I think that they have been fairly answered. Thus we see that the third step which Pasteur took with regard to the theory of putrefaction has not been taken relative to the theory of contagium. It has not been proved that the particles which give rise to contagious diseases are germs

* "Twelfth Report of the Medical Officer of the Privy Council," p. 243.

having independent powers of reproduction in a suitable medium; and therefore the other hypothesis that these particles are portions detached from a living organism is still a possible one.

In the foregoing pages the chemical theory of fermentation and of putrefaction was briefly noticed, not on account of its intrinsic value, but as an introduction to the consideration of the germ theory which is gradually superseding it. In the next place it was noticed that the experiments undertaken to decide the question of the genesis of the low forms of organisms which appear in putrefactive infusions led to important collateral results. Schwann proved that some material in the atmosphere necessary to putrefaction could be destroyed by heat, hence it was surmised that that material must be organic. Cagniard de la Tour and Schwann had, independently of each other, discovered the yeast plant; this led to the assimilation of the two processes of fermentation and putrefaction, and suggested the question whether these organisms were not the cause of both. The experiments of Helmholtz proved that the substance which excites putrefaction must be solid; and some time afterwards Schroeder and Dusch showed that air strained through cotton wool does not excite putrefaction. The observations and experiments of Pasteur gave a satisfactory explanation of this by proving first, that the cotton wool arrested from the air in its passage organic particles, some of which were recognised as living; second, that these particles were germs, and thirdly, that no other change had taken place which would account for the phenomena but the arrest of the germs.

After tracing the germ theory of fermentation and putrefaction, it was then noticed that some forms of the germ theory had been applied to the explanation of disease for some centuries, and that this theory is admitted by all to be true with regard to a certain number of diseases. The proposed extension of this theory to the explanation of the phenomena of the contagious diseases was then discussed. The analogical argument derived from the parasitic diseases of plants and animals in favour of this extension of the theory was found to be unreliable. And it was also found that the extension of the germ theory by Professor Lister, to the formation of putrid pus, even when taken along with the brilliant experiments of Professor Tyndall, did not warrant us in extending this theory to the contagious diseases.

The experiments of M. Chauveau and of Dr. Sanderson were then glanced at, and it was shown that they brought the theory of contagion to the same stage of completeness that the first observations of Pasteur brought that of putrefaction—that is, they proved that the active part of the infective fluids of some contagious diseases consists of particles, and that these particles are living, and by inference it is considered highly probable that this is the case with all contagious diseases, although the experimental evidence is not yet completed. It was finally noticed that the attempt made to complete the germ theory of contagium, as Pasteur had done that of putrefaction, had hitherto not been successful. It has not yet been proved that the particles of contagium are living germs capable of independent maintenance in a suitable medium, nor has the attempt to dispose, by indirect evidence, of the opposite hypothesis, that these particles are portions detached from a living body, been successful. It follows, therefore, that two hypotheses are open to us, the one that contagium particles are independent organisms, and the other that they are detached portions of a living organism; and until an experimentum crucis can be devised to decide between them, our only alternative is to adopt provisionally the one which explains the most readily the phenomena of disease. I shall attempt to develop the latter hypothesis more fully in the following pages, and my readers must decide whether they will adopt it or the germ theory.

CHAPTER II.

THE GRAFT THEORY.

The theory which I shall endeavour to elaborate in the following pages is, as already stated, that contagium particles are living in the sense of being portions detached from a living being, that they are not germs capable of giving origin either to higher forms of life or to organisms like themselves in an organic infusion, but anatomical units modified and individualized by a diseased process, and capable of impressing upon the healthy organism with which they come into collision a succession of changes similar to that which preceded their own modification in the body from which they were detached. It is well known that the more active contagious diseases are characterized by the innumerable epidermic and epithelial cells which are cast off from the body. The least observant cannot have failed to notice that this is the case with such diseases as small-pox and scarlet fever, and microscopic examination shows that in typhoid fever and cholera the alvine discharges contain a great many epithelial cells along with myriads of granules and particles;

while the experiments of Mr. Crookes upon cattle plague render it highly probable that in the contagious diseases which affect the respiratory organs, the breath carries off innumerable particles from the body. Syphilis is a communicable disease which is an apparent exception to this rule, since it does not appear that many cells or particles are cast off a syphilitic person; but upon further examination, the apparent exception really confirms the rule, because inoculation with the secretions of a diseased person appears to be necessary for the propagation of the disease. No one will doubt that the particles which are detached from the body in contagious diseases are small enough to be wafted by currents of air; but it can be proved that epithelial cells and epidermic scales are floating in the atmosphere. This has been proved by Dr. Angus Smith, and can easily be verified by any one, by merely shaking a glass slip, upon which a drop of glycerine has been placed, two or three times near a person convalescent from scarlet fever, when the glycerine becomes covered with epidermic scales and minute particles. I had a striking illustration of the fact that epidermic scales may be wafted in atmospheric currents in a case of confluent small-pox which I attended in November, 1868. When the patient was convalescent I called one evening to see him, and a beam of sunlight coming through the window, fell upon his back as he sat by the fire. The light was scattered by myriads of epidermic scales. On the slightest movement clouds of these scales were seen to arise from his head and face, the heavier settling on his clothes, while the lighter danced in the sunlight. This observation made a deep impression upon me at the time, but I did not recognise its full significance until I read Professor Tyndall's experiments with the electric beam. If, then, contagious diseases cast off innumerable epithelial cells and particles, and if these cells and particles are found floating in the atmosphere, the spread of contagious diseases are as satisfactorily accounted for by the theory under consideration, as by the one which regards contagium particles as independent organisms.

The next step we must take is to adduce evidence to show that the particles concerned in the production of contagious diseases are only alive as parts of a living body; and since, as has already been seen, the direct or experimental evidence upon this point fails us, our only alternative is to develop, as far as possible, the indirect or analogical evidence. Is there, then, any order of facts in the whole field of biology, to show that particles apparently so insignificant, and which are living only in the sense of being detached portions of a living body, can initiate changes so extensive as those which occur in the zymotic diseases?

The events which succeed the union of the germ-cell and the sperm-cell in ordinary reproduction will strike every one as more or less analogous to the case in hand, and a closer examination will disclose agreements between the two orders of facts where they might be least expected. I shall now proceed to trace out those analogies more minutely.

The pathological changes which precede the detachment of the contagium particles are such as to diminish the supply of nourishment to the epidermis; hence its cells lose even the small degree of specialization which enable them to take a share in the operations of the body of which they form a part; and hence they are cast off. But it has been supposed that the detached and semi-detached portions which are operative in the genesis of new individuals are highly "vitalized"* products; and if this is the case, the analogy fails in one essential particular. For if the evolution which takes place after the union of the germ-cell and the sperm-cell depends upon these cells being highly vitalized, this could not be used as an argument to show that the collision with another organism, of a particle characterised by a low degree of vitality, could give rise to such marked phenomena as are presented by the zymotic diseases. But Mr. Herbert Spencer has shown that the assumption that the reproductive cells are highly vitalized in the sense of being highly specialized or differentiated is very doubtful. The following are a few of the facts which render it in the highest degree probable that the reproductive cells are characterised by being unspecialized, or by

* "The Genetic Cycle," by Professor Ogilvie, 1861.

having undergone very little modification. In many plants and the lower members of the animal kingdom, a small fragment of tissue is capable of reproducing a new individual. A hydra may be divided into fifty pieces, and each piece will grow into a new and perfect individual. In plants, the reproductive particles arise at the extremity of the axes where the degree of organization is least; and in the higher animals, the reproductive centres are merely modified epithelium cells—cells which are remarkable for the simplicity of their structure. During the development of the embryo, the nervous tissue is the first to show signs of organization, but of "all the principal organs, the genital are those that are latest recognisable in their rudiments, and distinguishable in their future special forms."* This rule is not absolute throughout the whole animal kingdom, but it is so nearly absolute that it may be taken to indicate that as the first tissue to show signs of organization is the nervous—the most highly specialized—so the last organ recognisable in the order of development is the least specialized. But not only do the reproductive centres spring from a tissue which is undifferentiated; but the power of reproduction of these centres appears to vary inversely according to the degree of the organization of the individual. In unicellular organisms their only function appears to be reproduction; while in some of the inferior animals a fragment of tissue

* "Agassiz and Gould's Comparative Physiology," p. 328.

may give rise to a new individual. Ascending somewhat higher in the scale of complexity of organization, we find not only very active reproduction, but rapid repair of injuries, and restoration of lost limbs; but as we ascend still higher in the scale of organization, both the power of reproduction and of the repair of injuries greatly diminishes. In this respect there is a striking analogy between the reproductive and the contagium particles. As already stated, the latter have lost even that small degree of specialization which enabled them to take part in the operations of the living body from which they have become detached; but the considerations just stated appear to show that in proportion as they become less differentiated do their power of initiating changes in another organism increase; and, indeed, detached epithelial cells are known to possess the power of reproduction in a very eminent degree, even without concourse with another organic particle. Mr. Herbert Spencer says that he has been informed by Dr. Hooker, "that the *Begonia phyllomaniaca* habitually develops young plants from the scales of its stem and leaves—nay, that many young plants are developed by a single scale."*

It has already been said that contagium particles are detached from the body at a time when they are sparingly supplied with nourishment, and in this respect also there is an agreement between these and the reproductive particles. The paral-

* "Principles of Biology," by Mr. H. Spencer, vol. i. p. 221. 1864.

lelism in this particular is best seen in those organisms where asexual reproduction takes place. When a polype is freely supplied with nourishment it multiplies by a budding process, but when the supply of nourishment begins to fail, reproductive cells are formed, and sexual genesis takes place. A similar fact is well known to gardeners with regard to fruit trees. Trees which are freely supplied with nourishment "run into wood," and gardeners adopt the method of cutting the roots, and sometimes go even the length of "ringing the bark," so as to diminish the flow of sap to the tree. After this expedient is adopted, asexual gives rise to sexual reproduction, the terminal buds become floral, and finally the tree bears fruit. A great mass of evidence is brought forward by Mr. H. Spencer* to show that during the transition from asexual to sexual reproduction, from agamogenesis to gamogenesis, there must be relative innutrition of the reproductive particles. These particles which, by their union, initiate the most wonderful process in nature —the evolution of a new individual—are prior to their union sparingly supplied with nourishment; and, as has just been remarked, the contagium particles are detached from the body from a similar cause.

But there are other points of agreement between the reproductive and contagium particles. It has already been said that the reproductive particles are

* "Principles of Biology," by Mr. H. Spencer, vol. i. p. 224, *et seq.*

modified epithelial cells, and if such is the case there cannot be a profound difference between the male and the female element. "In the common polype," says Mr. H. Spencer, "sperm-cells and germ-cells are developed in the same layer of indifferent tissue; and in Tethya, one of the sponges, Professor Huxley has observed that they occurred mingled together in the general parenchyma."* On the other hand, the contagium particle is according to assumption a modified portion of an epithelial cell, and produces its action by coming in contact with the epithelial cell of another organism; the difference between the two processes being that the latter cell is an integral part of the organism, while the germ-cell is either detached or semi-detached, and therefore enabled after fertilization to enter upon changes more or less independent of the parent organism.

But if such serious consequences result from the union of the epithelial cells of one organism with epithelial particles detached from another, it might be supposed that we are at every instant exposed to disease, since there can be little doubt that the body in a state of health is constantly throwing off particles from its exterior covering. But some well-ascertained facts in connection with the fertilization of the germ will help us to solve this question. It has already been noticed that the germ-cell and the sperm-cell are not unlike each other; but a certain

* "Principles of Biology," by Mr. H. Spencer, vol. i. p. 222.

degree of unlikeness, either in the matter or in the molecular motion of these cells, must exist before the evolution of a new individual is initiated. Mr. Darwin says, that "with all hermaphrodite animals and plants, which it might have been thought would have perpetually fertilized themselves, and thus have been subjected for long ages to the closest interbreeding, there is no single species, as far as I can discover, in which the structure insures fertilization;" and he has shown that in a great number of cases there are "manifest adaptations which favour, or inevitably lead to an occasional cross between one hermaphrodite and another of the same species." * It has also been ascertained that some plants are absolutely infertile with their own pollen. "They are sometimes," says Mr. Darwin, "so utterly self-impotent, that, though they can readily be fertilized by the pollen of a distinct species, or even distinct genus, yet, wonderful as the fact is, they never produce a single seed by their own pollen." † From these and a great many facts adduced by Mr. Darwin, it is apparent that although the germ-cell and sperm-cell must on the whole have a considerable degree of similarity, yet these particles must also have a certain degree of dissimilarity, either in the matter or molecular motion before the evolution of a new individual is initiated. If, therefore, the lesson which these facts appear to show with regard to the

* "Animals and Plants under Domestication," vol. ii. p. 117.
† *Ibid.*, vol. iii. p. 139.

production of new individuals may be carried forward to the explanation of contagious diseases, it is in the highest degree probable that a particle detached from a healthy body would produce no result by coming into contact with another healthy body; and that it is only after the detached particle has been previously modified and individualised by what we call a diseased process, that its collision with a healthy body would produce any marked result.

It has already been noticed that the contagium particles are detached from the body because the supply of nourishment fails them; hence it may be inferred that they are on the verge of reaching that state of molecular equilibrium which constitutes death. And the same may be said with regard to the reproductive particles. "The next general fact," says Mr. Herbert Spencer, "to be noted is, that these cells whose union constitutes the essential act of gamogenesis, are cells in which the developmental changes have come to a close; cells which, however favourably circumstanced in respect of nutrition, are incapable of further evolution. Though they are not, as many cells are, unfitted for growth and metamorphosis by being highly specialized, yet they have lost the power of growth and metamorphosis. They have severally reached a state of equilibrium." * After noticing that when the sperm-cells and germ-cells which are not brought into contact disappear, Mr. Spencer adds: "But the

* "Principles of Biology," vol. i. p. 222.

important fact which it chiefly concerns us to notice is, that on the union of these reproductive elements there begins, either at once or on the return of favourable conditions, a new series of developmental changes. The state of equilibrium at which each of them had arrived is destroyed by their mutual influence, and the constructive changes which had come to a close recommence, a process of multiplication is set up, and the resulting cells presently begin to aggregate into the rudiment of a new organism."*

But although both the contagium and reproductive particles are on the verge of reaching a state of equilibrium, yet, like all lowly endowed tissues, they are able to retain their vitality for a long time. Strong evidence could be adduced to show that the virus of the contagious diseases retains its activity for many months, and, according to Mr. Darwin, the male element "is enabled to keep alive for even four or five years within the spermatheca of a female insect."†

On the supposition, therefore, that contagium particles are portions separated from the living body undergoing the disease, and that they produce their action by union with the cells which constitute the envelope of another body, there is a very close resemblance between these organic particles and the organic particles which, by their union, give rise to

* "Principles of Biology," vol. i. p. 223.

† "Animals and Plants under Domestication," vol. ii. p. 363.

new individuals. Both sets of particles are unspecialized; they are, whether detached or yet an integral part of a living individual, sparingly supplied with nourishment; they are either epithelial or modified epithelial cells or particles, and hence are so far similar to each other. In both sets, however, the fertilizing element must have acquired a slight degree of difference of matter or of motion from the fertilized element; they have also both arrived at a condition approaching to molecular equilibrium, and have the power of retaining their vitality for comparatively long periods. In one particular, however, the analogy fails. In the genesis of new individuals, there is, if we except the unicellular organisms where there is a fusion between two individuals, a union between two portions detached or semi-detached from two distinct individuals; but in the genesis of a contagious disease a union takes place between a distinct individual and a detached portion of another individual. But another series of facts is known to biologists where the analogy even in this respect is complete. I allude to the phenomena of grafting, where a portion detached from one individual is grafted upon another, and I shall now proceed to show that some of the phenomena which result from this process closely resemble those of contagion.

It may be remarked in the first place that we must look for the agreements between the phenomena of grafting and those of contagion, in the effects which are produced by the scion on the stock, since these are the most prominent features in the contagious diseases. In a very interesting article on "Grafting: its Consequences and Effects," in the *Popular Science Review* for April, 1871, p. 149, Dr. Maxwell T. Masters says:—"Adverting now to the effect produced by the scion on the stock, it may be said that we have here an operation something akin to vaccination." The similarity between the phenomena of grafting and those of the zymotic diseases has not escaped the sagacity of Mr. Darwin. "It is certain," says Mr. Darwin, "that when trees with variegated leaves are grafted or budded on a common stock, the latter sometimes produces buds bearing variegated leaves; but this may perhaps be looked at as a case of inoculated disease."* Both Dr. Masters and Mr. Darwin give many instances in which the stock is affected by the scion. Dr. Masters states on the authority of Mr. Rivers that "an unhealthy or feeble stock has been restored to health by the imposition of a healthy graft."† This fact has its parallel in what takes place after the surgical operation of grafting a bit of healthy skin upon an ulcerated surface. "Again," says Dr. Masters, "cases have been observed where from the stock *below* the graft fruits and flowers of the same appearance as those borne on the scion have made their appear-

* "Animals and Plants under Domestication," vol. ii. p. 365.
† *Popular Science Review*, April, 1871, p. 149.

ance. This has been observed in the case of the pear grafted on the mountain ash, and in other cases."* But the effect produced by grafting a scion of a variegated plant upon a stock of a common kind affords the most striking illustration of the phenomena of the zymotic diseases. Many such cases are mentioned by Mr. Darwin and by Dr. Masters, but only one or two of the most apposite instances can be mentioned here. "It is notorious," says Mr. Darwin, "that when the variegated jessamine is budded on the common kind, the stock sometimes produces buds bearing variegated leaves; Mr. Rivers, on the authority of a trustworthy friend, states that some buds of a golden variegated ash, which were inserted into common ashes, all died except one; but the ash stocks were affected, and produced, both above and below the points of insertion of the plates of bark bearing the dead buds, shoots which bore variegated leaves. Mr. Brown, of Perth, observed many years ago, in a Highland glen, an ash tree with yellow leaves, and buds taken from this tree were inserted into common ashes, which in consequence were affected, and produced the blotched Breadalbane ash."† I shall now quote one striking instance related by Dr. Masters in the article already referred to. "The effect produced," says he, "even by a temporary contact with the variegated bud, is confirmed by a case that fell

* *Popular Science Review*, April, 1871, p. 149.

† "Animals and Plants under Domestication," vol. i. p. 394.

under our own observation. A year or two since a beautiful *Abutilon*, with leaves mottled with yellow, was introduced into our gardens. It was very desirable that this should be propagated as largely and speedily as possible. Propagation by means of cuttings was easy enough, but naturally the plants were small, and took a considerable time to grow bigger. Grafting was therefore had recourse to. The scions of the variegated *Abutilon Thomsoni* were grafted on to green-leaved stocks of other *Abutilons*. This was done by many nurserymen on the Continent as in this country, and it was soon found that the grafted plants were apt to produce variegated leaves from the stock; in other words, that the peculiar qualities of the scion were manifested throughout the entire organism. To show that the variegation was really due to the influence of the scion, we may mention a curious fact communicated to us by M. Van Houtte, the well-known nurseryman of Ghent. Like his compeers he had plenty of illustrations of the fact that a variegated scion of this particular *Abutilon* will communicate its properties to the stock on which it may be grafted, but he further ascertained that if by some accident the graft were separated from the stock, the leaves subsequently produced from the latter were wholly green, as before the grafting, and even the variegated leaves originally produced lost their mottled character."* These facts show that a

* *Popular Science Review*, April, 1871, p. 150.

scion from a plant which has acquired a certain variation, when grafted on a stock which has not undergone such variation, can communicate its own special characteristics to the latter, and that this change in the stock not only takes place at the point of junction but is manifested throughout the entire organism. Similarly with regard to a contagium particle. It has already been said that I regard a contagium particle as a portion of what Dr. Beale calls the "germinal matter" of an epithelium cell, which has undergone some modification of matter or of motion. When this particle comes in contact with a healthy individual it not only communicates its own motion to the body at the point of contact, but it profoundly modifies the entire organism. In these respects, therefore, there is a very close agreement between the phenomena of grafting and those of the zymotic diseases.

But there is another series of facts which is closely related to the phenomena of grafting. I refer to the direct action of the male element, not in the ordinary way on the ovules, but on certain parts of the female plants, or in the case of animals on the subsequent progeny of the female. It is now well ascertained that pollen from one species or variety, when applied to a distinct form, frequently modifies the coats of the seeds, and the fruit of the mother plant. With regard to animals Dr. Carpenter says, "Attention has recently been directed to a very curious class of phenomena, which show that where the mother has previously borne offspring the influence of its father may be impressed on her progeny afterwards begotten by a different parent, as in the well-known case of the transmission of Quagga marks to a succession of colts, both of whose parents were of the species Horse, the mare having been once impregnated by a Quagga male, and in the not unfrequent occurrence of a similar phenomenon in the Human species, as when a widow who marries a second time bears children strongly resembling her first husband."* These facts are adduced here because they are related to sexual reproduction, and to the facts of grafting already mentioned, and therefore related to the phenomena of contagion; and something is gained if the latter phenomena are brought into proper relation with the wider problems which present themselves in the study of life. This is true even if no explanation of these problems can be given, since when a special difficulty is resolved into a general difficulty, which meets us throughout the whole biological series, there is a much better chance of attaining at some time to a solution of both the general and special difficulty. These facts also show, as Mr. Darwin has remarked, "in how many extraordinary modes one organic form may lead to the modification of another; and often without the intervention of seminal reproduction."†

* "Carpenter's Principles of Human Physiology," 7th edition, p. 863.

† "Animals and Plants under Domestication," vol. i. p. 405.

And if this consideration does not prove the theory of contagion advanced in these pages, it at any rate helps to make it more credible.

It has now been shown that there is a very close analogy between the phenomena of contagion and those of reproduction; and still closer between them and those of grafting. Therefore, in speaking in future of contagium particles, I shall call them, not disease *germs*, but disease *grafts*. Nor is this by any means a new theory. The very term "inoculation" suggests that the idea of producing the communicable diseases artificially was adopted from the gardeners; and that the analogy between the two processes was early noticed is rendered evident, from a passage in one of Lady Mary W. Montagu's letters, dated from Adrianople, 1717. "The small-pox," she says, "so fatal and so general amongst us, is here entirely harmless, by the invention of *grafting*, which is the term they give it."*

The question now to be determined is how far this assumption will account for the phenomena of the zymotic diseases: and in framing a hypothesis which will explain these phenomena it is better, if possible, to gain a central stand-point from which to view the facts at a glance; instead of having to walk round and examine them in detail. In choosing a hypothesis that one is to be preferred which

* "The Letters of Lady Mary Wortley Montagu," edited by her great-grandson, Lord Wharncliffe, 2nd edition, vol. i. p. 393.

includes, and helps to explain the greatest number of facts; and therefore any hypothesis constructed to account for the phenomena of the zymotic diseases will command our assent just in proportion to the number and extent of analogous processes which can be brought within its scope. The analogy between some of the phenomena of the zymotic diseases and of grafting has already been noticed, and the transition is easy from grafting to budding, and from budding to asexual and sexual reproduction, the repair of injuries, the restoration of lost parts, and to the growth, maintenance, and development of the individual. Again the genesis of new individuals cannot be considered apart from the great questions of inheritance, reversion, and spontaneous variations; while lastly, the consideration of useful variations, whether spontaneous or not, cannot be clearly separated from that of injurious variations; nor can the inheritance of beneficial be considered apart from that of detrimental variations—in short, the discussion of inheritance, in its widest aspect, includes that of diseased as well as of healthy variations. All the processes enumerated graduate into each other; and, as already remarked, the hypothesis which embraces and helps to explain all these, recommends itself to our acceptance much more than if it were constructed specially to account for one or two of those analogous processes considered apart from the rest. To those of my readers who are acquainted with the writings of Mr. Darwin, his theory of Pangenesis will immediately suggest itself. The hypothesis of Pangenesis, in Mr. Darwin's own words, "implies that the whole organization, in the sense of every separate atom or unit, reproduces itself. Hence ovules and pollen-grains—the fertilized seed or egg, as well as buds—include and consist of a multitude of germs thrown off from each separate atom of the organism."* Mr. Darwin remarks that nearly similar views have been propounded by other authors, and more especially by Buffon, Bonnet, Professor Owen, and Mr. Herbert Spencer; but the fact is, that the fundamental conception of the hypothesis was advanced upwards of 2000 years ago, as the following quotation from the "Hippocratic Treatise on Airs, Waters, and Places" will show. I quote the passage at length, because it is interesting in itself, and especially so in relation to some of the great questions opened up by the speculations of Mr. Darwin. Hippocrates says:—"I will pass over the smaller differences among the nations, but will now treat of such as are great, either from nature or custom; and, first, concerning the Macrocephali. There is no other race of men which have heads in the least resembling theirs. At first usage was the principal cause of the length of their head, but now nature co-operates with usage. They think those the most noble who have the longest heads. It is thus with regard to the usage immediately after the child is

* "Animals and Plants under Domestication," vol. ii. p. 358.

born, and while its head is still tender, they fashion it with their hands, and constrain it to assume a lengthened shape, by applying bandages and other suitable contrivances whereby the spherical form of the head is destroyed, and it is made to increase in length. Thus, at first, usage operated, so that this constitution was the result of force; but in the course of time it was formed naturally, so that usage had nothing to do with it; *for the semen comes from all parts of the body—sound from the sound parts, and unhealthy from the unhealthy parts.* If, then, children with bald heads are born to parents with bald heads, and children with blue eyes to parents who have blue eyes; and if the children of parents having distorted eyes squint also for the most part; and if the same may be said of other forms of the body, what is to prevent it from happening that a child with a long head should be produced by a parent having a long head?"* It is possible that the length of the head of the Macrocephali was more owing to sexual selection than to the usage of applying bandages to the heads of the infants; but what concerns us most at present to notice is that the facts of inheritance were well known to Hippocrates, and that to account for them he assumed that the semen comes from all parts of the body. To understand the full significance of

* "The Genuine Works of Hippocrates." Translated from the Greek by Francis Adams, LL.D. (Sydenham Society). Vol. i. p. 207.

this hypothesis, it ought to be remembered that, as the learned translator says, in a foot-note, "Hippocrates, and after him most of the ancient authorities, held that the fœtus is formed from the male semen."* But although the fundamental conception of Pangenesis is to be found in the writings of Hippocrates, the hypothesis has assumed a much more definite form in Mr. Darwin's hands, and has also been enlarged so as to embrace, not only the genesis of individuals and inheritance, but all the great operations carried on in living beings. In constructing his hypothesis, Mr. Darwin makes the following assumptions:—

1. That during all stages of development the cells of the body throw off gemmules, which circulate freely throughout the system.

2. That the gemmules multiply by self-division, and subsequently become developed into cells, by union with other gemmules, or partially developed cells which precede them in the regular course of growth.

3. That the gemmules are transmitted from the parents to the offspring—are developed in the succeeding generation, but often are dormant during many generations.

4. That the gemmules, in their dormant state, have a mutual affinity for each other, leading to

* "The Genuine Works of Hippocrates." Translated from the Greek by Francis Adams, LL.D. (Sydenham Society). Vol. i. p. 214.

their aggregation either into buds or into sexual elements."*

Mr. Darwin adduces a great many facts, which show that these assumptions, extreme as they may at first appear, are supported by many analogies in the admitted truths of biology; in short, he shows that he is only exercising the "scientific imagination" in forming new combinations with facts admitted to be true in a less extreme form. These assumptions being granted, Mr. Darwin applies them with wonderful skill to the explanation of the most complex processes of living beings. By this hypothesis he brings under one point of view all the forms of asexual and of sexual reproduction—the development of each being including all the forms of metamorphosis and metagenesis—the growth of organisms, repair of injuries, and restoration of lost parts—inheritance, reversion, and individual variations. Now, the fact of bringing all these processes under one point of view, and thus showing the relation which the one bears to the other, is of very much greater importance than the hypothesis which is used as a means to this end. Mr. Galton may at this moment be conducting experiments which will prove the hypothesis to be erroneous, but the inductive marshalling of the facts will remain untouched. If, however, this hypothesis is useful in enabling us to comprehend the great

* "Animals and Plants under Domestication," vol. ii. p. 357 *et seq.*

operations which take place in healthy living beings, it will be equally useful in the study of diseased beings; and, in applying it to the elucidation of pathological states, I attach much greater importance to the order to which the facts of disease are reduced, and to their being brought into proper relation to the great processes of genesis, inheritance, reversion, and variations, than to the hypothesis which is used as a means to that end. Before proceeding, however, to apply this hypothesis to the explanation of the phenomena of disease, I shall make a few remarks upon the assumptions on which it is founded.

The theory of Pangenesis can, in my opinion, be considerably simplefied, although I do not consider myself competent to the task. I shall, however, throw out a few hints which may be taken for what they are worth.

Mr. Darwin's first assumption is, that during all the stages of development the cells of the body throw off gemmules which circulate freely throughout the system; and he also assumes that representatives of all these gemmules are found in the reproductive cells. I shall not insist upon the great improbability of the reproductive cells being such complex aggregates of gemmules, since the main question is to find the simplest physical mechanism by which we can represent to ourselves the facts; and if the facts can be represented by a simpler mechanism, the latter is much to be preferred. Mr.

Darwin instances a simple case. "If one of the simplest protozoa," says he, "be formed, as appears under the microscope, of a small homogeneous gelatinous matter, a minute atom thrown off from any part and nourished under favourable circumstances, would naturally reproduce the whole; but if the upper and lower surfaces were to differ in texture from the central portion, then all three parts would have to throw off atoms or gemmules, which when aggregated by mutual affinity, would form either buds or the sexual elements. Precisely the same view may be extended to one of the higher animals, although in this case many thousand gemmules must be thrown off from the various parts of the body."* But Mr. Darwin appears to me to neglect entirely the action of the environment upon living beings. It is well known to physiologists that a change in the incident forces does produce a modification of the tissues of a living body. The thickening of the skin upon workmen's hands may be mentioned as a familiar example. It is certain, therefore, that even the simplest protozoa could not live long without the external and central portions being subjected to different incident forces, and as certain that this would lead to the external covering being rendered slightly different from the central portion. In this instance, if the gemmule is homogeneous at first it cannot long remain so, and therefore, if a gemmule from the central por-

* "Animals and Plants under Domestication," vol. ii. p. 376.

tion of one of the protozoa maintains its existence for a short time and grow, it is certain that it will become differentiated into an envelope and central portion, whether our means of observation enable us to detect the difference or not. In this case, then, one gemmule was sufficient to give origin to a protozoa which, in what may be called its adult condition, becomes differentiated into an outer and central portion. If we now ascend to those organisms which consist, not of a single cell, but of an aggregation of cells, it is certain that the difference between the forces which will fall upon the outer and central portions will be greater than in the unicellular organisms, and therefore that the differentiation between the outer and central portions will also be greater. But another factor may now come into operation, which will produce a still further modification. One of the other assumptions of Mr. Darwin is that the gemmules, which circulate freely throughout the body, become developed into cells by union with other gemmules or partially developed cells, which precede them in the regular order of growth. If, then, the outer portion of an organism which consists of an aggregation of cells casts off gemmules, these will come into contact with the central portion, or with gemmules from the central portion, and a third cell may result, differing from both the outer and central parts to a slight extent. This being the case, then, an organism may become developed by the continued growth of a single gemmule, first into a cell, and then into an aggregation of cells, and this organism may have in its fully developed state three tissues slightly differing from each other,—an outer and a central tissue, and another of an intermediate character, but slightly differing from both. That the formation of a third tissue from the concourse of cells or gemmules from other two tissues really does occur in the higher animals is proved by one of Cohnheim's interesting experiments. Cohnheim scarified the corner of a frog's eye, and, according to him, repair was immediately set on foot by the transportation thither of lymph-corpuscles from the neighbouring lymph heart, a fact which was ascertained by introducing finely-powdered aniline blue into the latter. But the tissue by which the repair was effected was, when completed, unlike either the tissue of the part prior to the injury, or to the lymph cells;—in short, a third tissue was produced, differing slightly from the tissues whose concourse had produced it. It is evident, therefore, that a homogeneous gemmule may by assimilating pabulum to its substance develop into a cell, and that this cell may, by the action of the environment, become differentiated into an outer and central portion. It is also evident from what has been said that this cell may, by self-division and growth, become developed into an organism consisting of an aggregate of cells, and that this aggregate will, by the action of the forces of the environment, become more distinctly differentiated into an outer and inner portion; and that the concourse of gemmules from each of these may give rise to a third tissue differing from both. But the process does not stop here: the more the organism acquires bulk by growth, the greater will be the difference between the forces to which its different parts are subjected, and as a consequence the greater will be the modifications produced in its tissues by the action of the environment; and as a further consequence the more numerous will be the tissues produced by the concourse of gemmules from different parts of the organism. This statement, although true in a general sense, would require to be modified in many directions before it could be accepted as universally true; but it is sufficiently near the truth for our present argument. That purpose is to show that it is not necessary to postulate the existence of such myriads of gemmules in the fertilized germ as Mr. Darwin's theory demands. One of the higher organisms as it exists at any one moment is the effect of the same organism, and of its environment at the preceding moment; hence the life of the organism as a whole is a series of which the terms are changing at every instant of time. If, therefore, a fertilized germ is surrounded by conditions nearly similar to those by which its parent was surrounded at the initial stage of its development, all that it is necessary to account for at first is, how the first term gives origin to a second similar to that of the parent, and the conditions in the environment continuing similar, how the second gives origin to the third, and so on. It has already been traced how a gemmule may by growth give rise to a differentiated cell, and how a cell may give rise to an organism consisting of an aggregate of cells differentiated into three tissues differing from each other, and it has also been hinted that an indefinite number of terms of higher and higher degrees of complexity might be accounted for by the same principles; therefore it appears to me that it is unnecessary to assume that the fertilized germ consists of such a complex aggregation of gemmules as Mr. Darwin supposes.

Another consideration which strengthens this opinion is, that although spontaneous variations tend to be inherited, yet the scars which result from slight mechanical injuries have no such tendency. If the gemmules of the cells of those scars had representatives in the reproductive cells, we might expect that occasionally the progeny would exhibit copies of the scars of either parent. But Dr. Brown-Séquard has shown that a section of one-half of the spinal cord of the guinea-pig produces a liability to epileptic attacks, and that this acquired epilepsy tends to be inherited. This shows that when the injury produces a profound modification of the system, the latter may be inherited; but the paralysis which results from the injury being ephemeral is not inherited. It is in my opinion impossible to account for these facts on the supposition that the reproductive cells contain gemmules from the cells of the parent during all the stages of development.

Another assumption which Mr. Darwin makes is that the gemmules are transmitted from parent to offspring, are developed in the succeeding generation, but often are dormant during many generations. This assumption has been made in order to account for the phenomenon of reversion and atavism. But what happens in many pathological processes will give us a clue to the simplification of the theory in this direction. According to the doctrine of organic evolution all organisms have been developed from one or a few germs by successive modifications. But besides developments from the lower to the higher, from the least specialized tissue to the most specialized, pathology discloses a reverse process in which a degeneration takes place from the higher to the lower, from the most specialized to the least specialized tissues. Take for instance a mucous membrane. Its cells, though not highly specialized, must have relations which are more definite and special than the unspecialized tissue which constitutes an amœba. But when a mucous membrane becomes inflamed, its cells, after a time, become imperfectly nourished and degenerate into pus; in short, they lose all the special properties which fitted them to take an orderly part in the vital operations of the organism as a whole, and only retain such vital movements as are exhibited by amœba. Similarly with regard to the movements of the particles of vaccine lymph, when they are considered as detached portions of a living body, they lose the special properties which enabled them to take a part in the healthy operations, but retain the movements which characterize bacteria. Suppose, then, a germ-cell or a sperm-cell to be either from general, or from local causes, imperfectly nourished, it may lose some of the properties which its recent predecessors had acquired, and thus revert to the characters of the reproductive cell of a remote progenitor. On the other hand, if the nutrition of the reproductive cell is unusually great, a higher degree of specialization may be attained, and the line of least resistance to the acquisition of this specialization will be in the direction of qualities which were highly developed in a remote progenitor. If this opinion is correct, the assumption that the fertilized germ contains gemmules in a dormant state from remote progenitors may be dispensed with.

But if I object to some of the assumptions of the theory of Pangenesis, it may be asked which of them I would retain. What appears to me to be the most valuable assumption of the hypothesis is that the cells of the body cast off gemmules which subsequently become developed into cells by union with other gemmules, or partially developed cells which precede them in the regular course of growth. Of course, underlying all these assumptions is the assumption, or I may say, the proved law, of the unity of life—"Of life with its several powers, having been originally breathed by the Creator into a few forms or into one,"* and the gradual evolution of the higher organisms by successive modifications and developments from the lower. This being the case, it may be expected that the phenomena presented by the lower organisms can be traced, however much entangled, throughout the whole biological series. This is admitted to be true with regard to some of these phenomena, and we may infer it to be true of the rest.

In the lower forms of life each cell is an independent organism, and it is admitted by all physiologists that every cell of the body of the higher organisms has a certain degree of independence. "Now," says Schwann,† "as all cells grow according to the same laws, and consequently the cause of growth cannot in the one case lie in the cell, and in another in the whole organism, and since it may be further proved that some cells, which do not differ from the rest in their mode of growth, are developed independently, we must ascribe to all cells an independent vitality, that is, such combinations of molecules as occur in any single cell, are capable of setting free the power by which it is enabled to take up fresh molecules. The

* "Origin of Species," by C. Darwin, p. 490.

† "Schwann and Schleiden's Researches." Sydenham Society's Translation, p. 192.

cause of nutrition and growth resides not in the organism as a whole, but in the separate elementary parts, the cells." And again, Virchow says:—"Every animal presents itself as a sum of vital unities, every one of which manifests all the characteristics of life. The characteristics and unity of life cannot be limited to one particular spot in a highly developed organism (for example, to the brain of man), but are to be found only in the definite, constantly recurring structure, which every individual element displays."*

Again the unicellular organisms show a certain elective affinity for the organic substances upon which they feed, and for the infusions to which they communicate chemical change. "Each cell," says Schwann, "is not capable of producing chemical changes in every organic substance contained in solution, but only in particular ones. The fungi of fermentation, for instance, effect no changes in any other solutions than sugar, and the spores of certain plants do not become developed in all substances. In the same manner it is probable that each cell in the animal body converts only particular constituents in the blood."† This selection by the cells of the higher organisms of particular constituents in the blood is strongly

* Virchow's "Cellular Pathology." Translated by Dr. Chance, p. 13.

† "Schwann and Schleiden's Researches" (Sydenham Society), p. 200.

insisted upon by Virchow.* "We are irresistibly compelled," says he, "both by the consideration of simply pathological, and particularly by that of pharmaco-dynamical, phenomena to admit that there are certain affinities existing between definite tissues and definite substances, which must be referred to peculiarities of chemical constitution, in virtue of which certain parts are enabled in a greater degree than others to attract certain substances from the neighbouring blood."†

The unicellular organisms multiply with great rapidity, by giving off minute buds, which soon attain the size of their parent, and a similar mode of multiplication is exhibited by the individual cells of the higher organisms. Even if we do not sanction Virchow's opinion that all structures arise from the proliferation of pre-existing cells, there can be no doubt that the individual cells are short-lived, and that before each dies it gives origin to one or more to take its place. Dr. Beale, in commenting on the movements exhibited by pus-corpuscles says, "So far from the corpuscles being spherical, as usually figured and described, in many specimens not a single corpuscle of this form is to be detected. Every corpuscle exhibits little 'buds,' 'offsets,' or 'protrusions,' at every part of its cir-

* See also Paget's "Surgical Pathology," 2nd edition, by Turner, p. 13, *et seq.*

† Virchow's "Cellular Pathology." Translated by Dr. Chance, p. 123.

cumference, and attentive examination, even under moderate magnifying powers, will convince the observer that the corpuscles are slowly undergoing alterations in form." "Movements also occur in the most minute of these buds or offsets which have been detached." "One of the smallest particles detached from a pus-corpuscle is capable of absorbing nutrient material and growing into a corpuscle having all the properties and powers of that from which it was derived."* It is also highly probable from the observations of Dr. Beale and of others, that the lymph, chyle, and mucus corpuscles, young epithelial, and other actively growing cells exhibit similar movements, and multiply by the growth of detached buds. But although the unicellular organisms multiply by a budding process, close observation has demonstrated that the individual multiplication does not proceed indefinitely, but that occasionally two individuals coalesce—a process which is justly considered analogous to sexual reproduction in the higher organisms. But it has also been seen that the cells of the higher organisms give origin by a budding process to other cells, and it seems a very probable assumption to make that these cells cannot go on multiplying indefinitely, but that there must be an occasional concourse of two cells or detached portions of two cells from different parts of the organism. Many facts could be adduced to

* "The Microscope in Medicine," by Lionel S. Beale, 3rd edition, p. 64.

strengthen this assumption, but in the mean time it may be allowed to rest entirely on analogy. It has been seen that the cells of the higher organisms exhibit a certain degree of independence like the unicellular organisms; both cells have an elective affinity for certain organic substances, both multiply by a budding process, and exhibit similar movements, and this alone renders it highly probable that as genesis by the concourse of two individuals takes place in the unicellular organisms, the same occurs with the cells of the higher organisms, and that they are occasionally fertilized by gemmules cast off from different but correlated parts of the body; and if this is true, it gives high probability to the truth of the assumption of Mr. Darwin which is under consideration at present. With these remarks I shall now proceed to apply the hypothesis of Pangenesis to the explanation of the phenomena of disease.

CHAPTER III.

LIFE, HEALTH, AND DISEASE.

THE GENERAL DISEASES.

BEFORE proceeding further it is necessary, if not to define, at least to advance a few considerations which will enable us to make a practical distinction between the correlative couple—*Health and Disease;* and since these are only different modes of *Life*, our starting-point must necessarily be from a definition of the wider process which embraces both. The following remarks are not intended as a scientific discussion of the subject; since, as already stated, my object is to arrive at a practical result which will enable us to proceed with the consideration of disease. As the basis of further procedure I shall accept Mr. Herbert Spencer's definition of life; since, whatever objections may be made to it, it is the best yet advanced, and it is certain that I cannot pretend to improve upon it. Mr. Spencer defines life, as "the definite combination of heterogeneous changes, both simultaneous and successive, in correspondence with external co-existences and sequences;"* or in a more abstract form, as, "the continuous adjustment of internal relations to external relations."† When these definitions are viewed from Mr. H. Spencer's standpoint, they are in my opinion as nearly perfect as any definition of such complicated processes can be. His object is to exhibit the phenomena presented by living beings as conforming in the highest degree to the principles of evolution in general. On the other hand, my object is to adduce considerations which will enable us to make a practical distinction between a healthy living organism, and a diseased living organism; indeed it is narrower still, since in the generality of cases I only wish to distinguish between a healthy living man and a diseased living man; and, therefore, I must bring into greater prominence elements which are only included by implication in Mr. Spencer's definitions. Mr. Spencer‡ shows that the degree of life varies as the correspondence between the outer and inner relations. The life is high as the changes in the environment are met by corresponding and counterbalancing changes in the organism, the life is perfect when the correspondence is perfect, and ceases when the correspondence ceases. From this it might be thought that disease would be a state in which the adaptive changes in the organism would only imperfectly counterbalance the changes in the

* "Principles of Biology" (1864), p. 74.

† *Ibid.* p. 80.

‡ *Ibid.* p. 82, *et seq.*

environment. It is an undoubted characteristic of a diseased individual that its reactions to the actions of the environment are less perfect than in health; but as this is a characteristic which may be employed to distinguish between the different degrees of life as exhibited in the lower and in the higher organisms; between the life of a child and that of an adult; between that of an adult and an aged individual; and between that of an educated man and that of a savage, it follows that it is only an imperfect test when employed to distinguish between a healthy and a diseased individual. For our purpose it will be more convenient to view the subject in a more concrete form. All living beings exhibit a threefold unity—a unity of structure, a unity of function, and a unity of composition. The cell has already been noticed as the morphological unit, but since the days of Schwann the theory of the cell has undergone considerable modification. Neither the cell wall nor the nucleus is now considered an essential constituent of the structural unit; as Professor Huxley expresses it, "Protoplasm, simple or nucleated, is the formal basis of life."* There is also a fundamental unity in the actions of living bodies. All young cells exhibit contractions of their substance on the application of certain chemical and physical agents termed *irritants* or *stimuli*. This property of responding to the action of stimuli is termed

* "Lay Sermons, Addresses, and Reviews," by Professor Huxley (1870), p. 142.

irritability, and is no doubt a derivative property depending upon antecedent changes in the protoplasm; but as no account can yet be given of the *irritability* which does not pre-suppose its existence, it is possible to argue from it as if it were an underivative and ultimate property. In doing so, we are not reverting, as some have supposed, to the vitalistic theory of an immaterial essence; but simply argue provisionally as from an ultimate property, until the progress of discovery shall enable us to assign to it a physical or chemical cause. Besides this contractility of their substance, a circulation of the granular contents of young cells is also seen to take place, which is in all probability closely connected with their nutrition. But along with this unity of form and of function young cells also exhibit a unity of composition. All the forms of protoplasm which have yet been examined contain carbon, hydrogen, oxygen, and nitrogen in complex union. The organic substances which contain these elements are termed by chemists protein-principles, but the exact composition of these has not yet been determined. There can be no doubt, however, that an almost endless variety of those principles may result from the different manner in which the atoms of the elements may group themselves so as to form the complex atom of the proteinaceous substance.

But this fundamental unity is associated with an almost endless variety of special modifications. In the lowest organisms every part of the individual may perform the functions of nutrition, locomotion, and reproduction; but in the highest organisms each function is only performed by the definite combination of a great many parts each of which has undergone a special differentiation. As we ascend, therefore, from the lower organisms to the higher, and more especially to the higher animals, we find an ever increasing complexity of structural arrangements, an ever increasing specialization of function, and an ever increasing diversity in chemical composition. A similar increasing differentiation is traceable in their historical progress as individuals. One of the most prominent features of the higher animals is their *individuality*. They do not exist in large and continuous masses like inorganic matter, but are separated into distinct individuals, each having a special history of its own—a special cycle of existence. The most obvious characteristics of this cycle are, that the organism derives its existence from others more or less similar to itself, that its growth and capacity for action increase for a time, and that it terminates its existence after a definite career. But the simple fact of growth involves other changes in the organism, changes which are partly necessitated by the aggregation, and which on the other hand make further aggregation possible. The cell being the morphological unit, growth can only proceed by cell multiplication, and the fact that an organism can exist as an aggregation of several cells makes it necessary that the cell-wall shall have some degree of consistence, and as the aggregation of cells increases the consistence of the inter-cellular substances must also increase so as to give the organism the necessary cohesion. As aggregation proceeds the different parts of the organism cannot be equally nourished, and if there were no special arrangements for the supply of nourishment to the different parts of the aggregate, cell multiplication, which, as will be hereafter shown, depends on the amount of nourishment, would soon diminish and the aggregate would reach its maximum size. This special arrangement is brought about by what Milne Edwards calls "the physiological division of labour." Part of the organism becomes adapted for the accumulation of nourishment, part for the transfer of that nourishment to the different parts of the organism, and part for the expenditure of the nutriment in the activities of the individual. But along with this increasing specialization of parts of the organism for special functions, there is also an ever increasing dependence of each part upon the due performance of the functions of another part. In the lower organisms a fragment of tissue will reproduce the whole, but in the higher animals the severing of a small thread like the pneumogastric may terminate the existence of the individual. With these general remarks let us now confine ourselves to the consideration of what constitutes the healthy cycle of the life of an individual man.

As we have already seen, the healthy life of an individual is an orderly progression in the directions of size, structure, and function. Each individual, during the first stages of life, increases in bulk, acquires a more complex structure, with increase of functional activity. During middle life each of these processes continues more or less stationary, although, when the body ceases to increase in size, more of the food taken for its maintenance can be expended in functional activity; and after middle age is passed the functional activity declines manifestly, and this is accompanied by corresponding changes of structure; and if the bulk of the body does not always decrease there is a gradual decrease of its more active tissues, such as the nervous and muscular. Hence, looking at the body as a whole, in so far as the path of its healthy life can be represented by a single line, it may be compared to that described by a projectile or to a parabola. The body takes its origin in pre-existing life, increases in the directions indicated, and finally its motions become feebler and feebler, until the equilibrium is overthrown in death. But when I say that the course of a healthy body may be represented by a parabola, it must be understood with not only an equal, but even a greater latitude than is given to the astronomer when he says that a planet describes an ellipse in its course round the sun. The real path of the planet is an oscillatory movement, of which the circumference of the ellipse represents the mean position. Similarly in the case of a healthy living body, neither its weight nor its vigour is stationary for a single instant of time, but is constantly oscillating from side to side of an average position, which may be pretty accurately represented by a parabola. We have already noticed the physiological division of labour by which special organs are adapted for special functions; and also the increasing dependence of every part of the body upon the due performance of the function of every other part. Now each part of the body has a life of its own, and it is upon the orderly progress of the life of each organ that the harmonious action of the whole depends. It has just been said that the life of the body as a whole might be compared to a parabola, and, with the qualifications then noticed, the same is true of the life of each organ. And not only must the course of the life of each organ be a parabola, but the course of each must bear a certain definite proportion to that of every other, and to the course of the life of each individual as a whole; just as the orbit of each planet bears a constant relation to that of every other, and to the solar system as a whole. But this mode of viewing the healthy life dwells more upon the uniformities of succession which characterize the individual, but the facts of co-existence must not be entirely neglected. At each stage of its progress a healthy individual should possess a definite size and form, a definite chemical composition, a certain degree of structural complexity; and should also exhibit a definite combination of actions in conformity with simultaneous and successive changes in the environment. But how are we to distinguish the size, form, composition, structure, and actions which constitute health, at a certain stage of the evolution of the individual? Evidently not entirely from the diseased individual, since the fact of his being diseased at a certain time shows that at that particular stage of his progress the condition of health has never been realized in him. What constitutes our idea of the healthy life of an individual is an ideal conception derived partly from our knowledge, either by direct or indirect inference, of the condition at the same stage of evolution of the individuals of the species which have been able the best to combine their actions so as to balance the actions of the environment for the longest time, partly from the condition of the parents at the same age, and partly from a special study of the peculiarities of the individual. A study of the past history and present condition of the individual will assist us in carrying his ideal curve of health forward into the future, and in estimating whether his condition at any particular moment presents a deviation from that curve. It may be objected that this mode of viewing health and disease takes no account of mental phenomena. But setting aside all metaphysical speculations with regard to the relation which exists between body and mind, there can be no doubt that all mental phenomena are accompanied, if not caused, by correlative changes in the organization. In the language of Professor Huxley, "all the multifarious and complicated activities of man are comprehensible under three categories. Either they are immediately directed towards the maintenance and development of the body, or they effect transitory changes in the relative parts of the body, or they tend towards the continuance of the species. Even those manifestations of intellect, of feeling, and of will, which we rightly name the higher faculties, are not excluded from this classification, inasmuch as to every one but the subject of them they are known only as transitory changes in the relative position of parts of the body. Speech, gesture, and every other form of human action are, in the long run, resolvable into muscular contraction, and muscular contraction is but a transitory change in the relative positions of the parts of a muscle."*

Since, then, the healthy body is an equilibrated system of parts having a definite cycle of existence, it is evident that the perturbation of this cycle which constitutes disease may be caused either by excess or deficiency of the forces in the environment: excess or deficiency of nourishment, excess or deficiency of the physical forces, heat, light, and electricity, may each and all cause a perturbation of

* "Lay Sermons, Addresses, and Reviews," by Professor Huxley (1870), p. 135.

health. The diseases which are caused by general changes in the incident forces of the environment may affect any healthy individual, and therefore they may be conveniently termed general diseases. But frequently a special cause in the environment may produce a definite perturbation of health which can be recognised and defined as a distinct disease. We may therefore term the special perturbations which are caused by special agents in the environment specific diseases. I may here remark, however, that I use the term specific in this manner only provisionally, and that a fuller discussion of its meaning will hereafter be entered into. All men, however healthy, are liable to those diseases which arise from general or special changes in the environment. But, as Sir Thomas Watson says, "We must distinguish between *susceptibility* of disease, and a *tendency* to disease. In one sense all persons are born with a predisposition to most forms of disorder. No person is protected by nature against inflammation when the causes of inflammation come into play. Poisons of various kinds, and specific contagions, which indeed are poisons, operate with tolerable uniformity upon all men alike. But there are certain other complaints which we may separate in this respect from the others, which complaints some have a tendency to and some have not. The tendency is sometimes strong and evident, sometimes feeble and faintly marked; sometimes it displays itself in the midst of circumstances the most favourable to health, sometimes it requires for its development conditions the most adverse and trying."* Those diseases which require an inherited tendency in the organization for their development may be called constitutional diseases. With these general remarks we shall now proceed to consider more particularly those perturbations of health which we have called *general diseases.*

The healthy life has been regarded as an orderly progression in a definite direction, and it has also been seen that the organism at each stage of this progress must possess certain uniformities of co-existence. It is necessary to regard health in this light in order to be able to distinguish between it and disease, and also to discriminate between the various diseases; but both the progression and the uniformities of co-existence are not causes but effects, and it is now necessary to consider the causes from which these effects have resulted. We are therefore irresistibly driven back to consider once more the structural unit, since the orderly progress of the life of an individual, which we term health, must depend in the ultimate analysis upon the harmonious action of the units of which the body is composed, just as the order and progress of a society must depend upon the intellectual and moral qualities of its individual citizens; and it may be added that the perturbations of disease must also depend upon

* "Watson's Lectures on the Principles and Practice of Physic." Vol. i. 5th edition (1871), p. 115.

changes in those units and their relations to each other. It has already been noticed that the structural unit is the cell, and that all cells exhibit a unity of form, a unity of function, and a unity of composition; but there can be no doubt that these are three different forms of the same unity, and closely connected together as cause and effect. Neglecting what takes place in vegetables, which does not directly concern us at present, it is evident that when an animal cell exhibits contractile movements its proteinaceous contents must fall into less and less complex combinations, otherwise motion would be generated without a cause; and if the cell maintains its integrity it must discharge the less complex, and absorb from the environment more of the proteinaceous compound of the higher complexity; so that the actions of the cell are closely connected with its nutrition, and if contraction were the only function of the cell its action would vary directly as the nutrition. But it has already been seen that, either by a process of budding or of self-division, each cell gives origin to other cells more or less similar to itself. Now in whatever way this process of cell-multiplication is carried on, each particle detached from the cell towards the production of a new individual is a deduction from the nourishment of the parent cell, which might otherwise be expended in action. The expenditure in action and the expenditure in genesis are derived from a common stock of nourishment; hence it follows that, other things being equal, increase of the one involves decrease of the other, and *vice versâ*. From this it results that a tissue whose elements exhibit a very high reproductive activity cannot expend much in action; and, on the other hand, that a tissue which expends much in action cannot have such a large balance for the rapid reproduction of its elements. It has just been seen that the rate of multiplication must depend, other things being equal, upon the amount of nourishment absorbed by the cell; but it is not enough that the cell is surrounded by nourishment; even when that nourishment is highly elaborated and fit for absorption there must be nothing about the cell itself which will prevent the nourishment being taken into its substance. It is clear that if there is a dense cell wall the nourishment will not permeate the substance of the cell so readily as when the wall is thin and transparent; hence cells which have a high reproductive activity must either have a very inconspicuous envelope, or none at all. But another consequence will follow from the fact that the reproductive activity will, other things being equal, be in proportion to the facility with which nourishment can permeate the substance of the cell. If the individual consists of an aggregation of cells nourishment will permeate its cells less readily than when the individual consists of a single cell. This will result, because in the individual consisting of the aggregate of cells the nourishment has to permeate the peripheral before it can gain access to the central cells, but in the one-celled individuals their entire exterior may be surrounded by nourishment; and again, because the fact of the individual consisting of an aggregate of cells will render it necessary that the cell wall should have some degree of density to give the necessary cohesion to the cells, while when each cell is independent the cell wall may be absent. From this it follows that when the reproduction is very high the growth must be discontinuous. Lastly, the rate of multiplication will depend upon the rapidity with which each cell throws off portions of its substance for the production of new individuals; but when a cell begins at an early age to throw off portions of its substance, or if at an early age it merges its own individuality in that of two or more offspring, it is clear that the size attained by each cell can only be very small, and that the life of each cannot have any degree of permanence. Hence it is evident that when there is a very rapid production of units the size and permanence of each can only be small. To sum up, then, the reproductive activity will be at its maximum when the supply of nourishment is large and the expenditure in action small, the cell wall thin or absent, the growth discontinuous, and the size attained by each unit very small. And it also results from what has been said, that the life of each unit of a tissue having a high degree of multiplication has only a small degree of permanence.

It has now been seen that the nutriment absorbed by a cell is expended either in action or in reproduction, and that the expenditure in function and in reproduction must vary inversely; also that the amount of nourishment absorbed, and the mode in which it is expended depends upon, or is accompanied by, certain morphological characteristics of the cells, both as individuals and when aggregated into organisms of the first degree of complexity. From this we shall proceed to notice very briefly and generally what must be the morphological characteristics of the tissues of the higher animals, and the relations which must exist between those characteristics and the amount of absorbed nutriment, and the manner in which it is expended. It has already been mentioned incidentally that when the individual consists of an aggregation of cells, the cell wall must possess some degree of consistence, so as to hold the different units together; but as aggregation proceeds this necessity becomes greater, so that in the higher animals some, if not all the tissues must possess a great degree of strength. But when the cell wall is dense, the nourishment cannot permeate through it so readily; and since the protoplasm of the cells cannot obtain ready access to nourishment, the cell can neither exhibit a high reproductive activity, nor expend much in the display of function. The function which the tissue produced by the aggregation of these cells will perform, must therefore be of a passive character, and the individual cells will possess a great degree of permanence. Cartilage is a good example of the tissue under consideration. The intercellular substance is dense, the individual cells do not undergo rapid changes, and have only a low degree of fertility, and the tissue does not perform any active function. But there are other tissues, such as the skin, which do not require to possess a great degree of strength, but whose functions are of a passive character; and although the intercellular substance is not so dense in those tissues as in cartilage, yet it bears a much larger proportion to the contained protoplasm than in tissues which either perform an active function, or whose units have a high degree of reproductive activity. But the physiological differences of structure can only be alluded to here in the briefest possible manner, and we must therefore pass on to the consideration of the other tissues. It is evident that when a tissue is freely supplied with nourishment, and the walls of its cells are very thin, and when it is not performing any active function, the reproductive activity of the cells will be great. The substance of the lymphatic glands is a good example; and if, as is highly probable, the white blood cells multiply in the circulation their reproductive activity will be very great, since in their case not only are the walls of the units thin, but the growth is discontinuous. Again, tissues which expend much in action must be soft, so as to allow free access of nourishment; while, on the other hand, they must have a considerable degree of cohesion. The muscular system, for instance, could not convert the motion of molecule into that of mass unless its different elements were bound together by a substance possessing a great degree of cohesion. This double requirement of cohesion to give strength, and slight density to allow free access of nourishment, is met by an intimate blending of the dense and soft tissues, the former being employed to pack the latter into a definite structure. But this intimate blending of the dense with the soft tissues renders it necessary that there should also be an elaborate structure for the distribution of nourishment, otherwise the latter could not gain access to the soft through the dense tissues. The tissues of which we are speaking cannot be characterised by high reproductive activity, since they expend much in action, and, as already remarked, high expenditure in action and reproduction of units must bear an inverse ratio to each other. It may, however, be remarked that, since those tissues are supplied by a definite system of vessels containing a highly elaborated nutriment, it is possible that they should expend much in function, while a considerable margin would remain for reproduction. The higher organisms must, therefore, consist of three great classes of tissues—the *passive expenders*, which give strength and protection to the body; the *reproducers*, which are mainly engaged in the growth of the organism; and the *active expenders*, which are engaged in controlling and producing the motions of the individual. With these remarks upon the healthy tissues we must now pass on to the consideration of diseased organisms.

As previously remarked, the life of a healthy individual may be represented as oscillations on both sides of an ideal curve, and the life of a diseased individual as presenting larger deviations and oscillations from this curve; and from this consideration alone it is evident that there cannot be a clear and distinct line of demarcation between health and disease, whether the case be looked at from the point of view of the individual as a whole, or of the different parts of the individual, or whether the case be viewed from the chemical, structural, or functional stand-point. It is convenient here to make the structural unit the basis of our further consideration of disease, since this unit was adopted as the basis of our remarks upon healthy life, and also since the chemical changes which take place in living tissues are too refined for our means of research; and structure being the correlative of function, the consideration of the former will be the best preliminary to the study of the latter. At the same time it ought to be remembered that disease cannot be studied to advantage by simply attending to the structural deviations which take place, apart from the functional deviations, nor can the study of disease be prosecuted by attending to the structural and functional deviations of a part of the body apart from the influence the disease exerts upon the individual as a whole. As well might we expect to arrive at a good zoological classification of animals by a study of the histological characteristics of their tissues, as expect to get a good classification of disease from purely histological considerations. If, then, there is no clear line of demarcation between health and disease, it may be inferred that this graduation from the one to the other will reveal itself in the structural deviations which take place in the latter. "There is no other kind of heterology in morbid structures," says Virchow, "than the abnormal manner in which they arise, and that this abnormity consists either in the production of a structure at a point where it has no business, or at a time when it ought not to be produced, or to an extent which is at variance with the typical formation of the body. So then, to speak with greater precision, there is either a *Heterotopia*, an aberratio loci, or an aberratio temporis, a *Heterochronia*, or lastly, a mere variation in quantity, *Heterometria*."* Such then being the case, let us inquire more particularly how these deviations arise. The property of irritability, by which the tissues respond to certain chemical and physical agents termed stimuli or irritants, has already been mentioned. When an irritant is applied to a living

* "Virchow's Cellular Pathology." Translated by Dr. Chance, p. 63.

tissue, the skin, for instance, the cells become more active, and a greater flow of blood to the part results. The cells at first take up more nourishment, and this produces the appearance of what Virchow calls the "cloudy swelling;" and if the process stops here, the only result will be a slight increase of size from the absorption of a greater quantity of nourishment by the individual cells, constituting what Virchow calls simple hypertrophy. If, however, the irritation is greater, so as to lead to a larger amount of nourishment being absorbed by the cells, the diseased process will proceed further than simple hypertrophy. The relation which exists between the amount of nourishment on the one hand, and expenditure in action, and in genesis, on the other, has already been dwelt upon. If, then, the tissue which has absorbed an excess of nourishment is one of the active expenders, the nourishment may be expended in function and the part still maintain its integrity; but if again the tissue is a passive expender like the skin, the excess of absorbed nourishment must be expended in the production of new individuals, and this constitutes what Virchow calls numerical hypertrophy or hyperplasia. The cells may, however, as the result of the primary stimulus, absorb a still greater amount of nourishment, and a still higher rate of multiplication will ensue. But a high rate of multiplication is associated with a decrease of the thickness and density of the cell wall: hence in dense tissues this high reproductive activity will lead to the destruction of their structural integrity by the disappearance of the intercellular substance. And this high rate of multiplication will also lead to other consequences of equal importance to those just mentioned. When the cell proliferation proceeds to some extent there must result a higher competition for nourishment, the available positions for growth become occupied, the blood vessels are pressed upon from without, and coincident changes are taking place in their relations with the blood from within, which causes some of the capillaries to be closed, and the flow of blood through others to be retarded, so that the supply of nourishment is cut off to a very considerable extent. The formative impulse has, however, been given, and all the available nourishment is applied to the production of new individuals, so that there cannot be any balance left for the arrangement of the new units into a definite structure: the consequence is that all structure is lost, the intercellular substance disappears, and a tissue results whose units are as independent of each other as the cells of the unicellular organisms. As multiplication proceeds the disproportion between the number of units and the amount of nourishment becomes greater, and the cells instead of consisting of protoplasm become filled with elements of a less degree of chemical complexity, the cell degenerates, and may finally become dissolved. The degeneration which most frequently occurs in pus—the tissue under consideration at present—is called the fatty, and by its means the pus may become gradually dissolved, while changes are taking place in the surrounding tissues which cause the absorption of this fluid, and finally the product may disappear; and if there has been no loss of tissue, the part may nearly regain its former integrity.

Hitherto we have spoken of the diseases which arise primarily from an excess of irritation and of nourishment, but other diseases may arise from a deficiency of the action of the environment and of nourishment. The morbid processes which originate from deficiency of irritation are of a passive nature, and do not exhibit such a tendency to cell multiplication as those which arise from the opposite cause. The first degree may merely consist of a diminution of the protoplasm, or what Dr. Beale calls the "germinal matter" of the tissue. In another degree the protoplasm may be mixed up with elements of a less degree of chemical complexity—such as fat and starch, while the intercellular substance may soften, or become hardened by a deposit of inorganic salts.

Let us now take a wider view of all these diseases, so as to enable us to distinguish the relations in which they stand to each other. The hypertrophies consist of a deviation from the normal nutritive changes of the part by way of excess; while the atrophies and degenerations consist of deviations by way of defect. There are two ways in which these diseases may proceed in their further evolution. Either they may gradually rejoin the normal curve of health, or may continue permanently changed; and in the latter case the body, as a whole, must form a new moving equilibrium round the diseased organ. Inflammation again occupies a position intermediate between these two sets of diseases. It is on the one hand very difficult to distinguish the first stage of a slight inflammation from hypertrophic and hyperplastic conditions; while, on the other hand, it is equally difficult to distinguish between the second stage of a slight and chronic inflammation from degenerative changes. My opinion is, to quote words used by myself elsewhere, "that the first stage of inflammation is an excess of the normal nutrition of the part; that this excess produces changes in the relations between the tissues of the locality affected and the blood and capillaries, which cause an oscillation by way of defect of nutrition to take place. The secondary oscillation may be so great as almost instantly to overthrow the equilibrium in death; or it may only be to an extent which is compatible with the gradual return of the locality to the line of health. Between these two terminations—technically called gangrene and resolution—there may be various terminations, from the gradual death, which constitutes ulceration, to the formation of a new moving equilibrium nearer to death, than the line of health as the formation of degenerate tissues, like pus and chronic exudations."*

From these considerations it is evident that when there is a large and rapid multiplication of the cellular elements of the part, inflammation can be readily distinguished from hypertrophy on the one hand, and from atrophy on the other; but when the irritation is such as only to lead to small multiplication of units, it is difficult to draw a line of distinction between the first stage of inflammation and numerical hypertrophy; and again, when the irritation is slight; but, either from external or internal causes, prolonged, it may be difficult to distinguish the degenerative changes which arise from deficiency of irritation from the degenerative changes which arise as a secondary result of a primary excess of irritation.

We shall now notice one or two points connected with the spread of irritation, that are of the greatest consequence to our future argument. One of the most prominent characteristics of inflammation is that the morbid process does not confine itself to the point which has been the primary seat of irritation. Suppose that a pustule results upon the skin, from the irritation of a hair follicle; while pus may be found in the centre, the margin immediately in contact with the pus will be found to contain a much larger proportion of cellular elements than in health, but it is still able to maintain

* *Vide The Practitioner*, August, 1871, p. 68.

more or less of its structural integrity; somewhat further out will be found a condition of numerical hypertrophy; while at a degree still further removed from the centre will be found simple hypertrophy, which gradually merges into the sound tissue. Whatever the explanation may be, there can be no doubt that here there is the spread of inflammation from a morbid to a sound tissue with which it is in contact; and more marked cases might be given, such as the spread of inflammation from the pleura costalis to the pleura pulmonalis. In the widest meaning of the term contagious there can be no doubt, therefore, that this process would be embraced by it, and that it is essentially related to contagion, in its narrowest meaning. Mr. Simon is therefore right, when he says that "there is much reason to question the popular impression that only 'specific' inflammations are communicable; much reason for suspecting it, on the contrary, to be a generic and essential property of inflammation that its actions (or some of them) are always in their kind, to some extent, contagious."* But not only does disease spread from one tissue to another by contact, but health spreads in the same way. In a pustule, for instance, when the pus escapes, and the violence of the first action subsides, the inflammatory process ceases to spread, and the healthy process begins soon to encroach upon the morbid part, which becomes healthy from the circumference to the

* "Holmes' System of Surgery," vol. i. p. 47.

centre, and not from the centre to the circumference. But this fact will, hereafter, receive a further confirmation, and acquire a greater significance.

But this contagiousness of inflammation not only declares itself at the primary seat of irritation, but it is equally manifest when the products of inflammation are conveyed from one part of the body to another. "When the infective entrance of pus," says Mr. Simon, "is into some vein of the digestive system (as, for example, into a mesocolic or inferior mesenteric vein, in connection with the ulcers of dysentery) the first stoppage of the inflammatory product is made by the capillaries of the liver, and hepatic abscess is the immediate result, the lung suffering only in proportion as pus passes onward to it from the liver, and other organs suffering only in proportion as the left ventricle transmits to them a purulent blood from the pulmonary circulation. Here then primarily, secondarily, and tertiarily are facts of inflammatory contagion: whithersover the pus is carried, there unfailingly it produces suppuration, so unfailingly that, as has been said, 'if, by way of experiment, you inject a little pus into a vein, you are able to foretell the result just as certainly as if you transferred a handful of frogs' spawn from one ditch to another.'"*

This statement requires a certain degree of modification, since it does not necessarily follow that pus

* "Holmes' System of Surgery," vol. i. p. 71.

always results from the entrance of pus into the circulation. The pus may only cause a certain amount of irritation where it has been arrested, sufficiently to cause a slight degree of simple or numerical hypertrophy, or only an amount of inflammation which falls short of that necessary for the production of pus. Mr. Simon's allusion to 'frogs' spawn' reminds one of the experiments of Newport with the ova and spermatozoa of Batrachians. "Newport," says Mr. Darwin, "established by numerous experiments, that, when a very small number of spermatozoa are applied to the ova of Batrachians, they are only partially impregnated, and the embryo is never fully developed: the first step, however, towards development, the partial segmentation of the yelk, does occur to a greater or less extent, but is never completed up to granulation. The rate of segmentation is likewise determined by the number of the spermatozoa—with respect to plants, nearly the same results were obtained by Kölreuter and Gärtner."*

There can be no doubt that the degree of contagiousness of pus depends very much upon its quality. In the mean time, however, the question of the morphological characteristics with which a high degree of contagiousness is associated, may be left undetermined until the properly contagious diseases are under consideration. But it may be remarked here that the experiments of Newport render it highly

* "Animals and Plants under Domestication," vol. ii. p. 363.

probable that the quantity of pus which gains entrance into the blood is also a factor in determining whether pus is or is not generated in the remote parts of the body. But another point of interest in connection with the genesis of pus from pus may be mentioned here. As already pointed out, the first stage of inflammation is an excess of the normal activity of the part, while the second is characterized by a diminished nutrition, and it is in this second stage that pus is formed. When, therefore, pus is arrested in the capillaries, and gives origin to pus, the formation of the latter is only an indirect consequence of the arrest of the former. The first effect of the pus is to irritate the cells of the part so as to produce an excess of their normal activity, and as an indirect consequence of this activity pus is finally produced. What I wish to point out here is the analogy which the evolution, or if it be preferred, this deviation from the normal evolution of the part, bears to the evolution of an individual. When, for instance, the two reproductive particles of the higher organisms come together, and when the fertilized germ is kept under suitable conditions, very rapid changes begin to take place. For months, nay even for years, growth and development proceed, but no reproductive particles are formed; and when finally they reappear, under what circumstances are they produced? They are produced when the developmental changes and growth of the individual are about to close, that is, when the amount of nourishment taken by the individual is not much in excess of the expenditure in function; in short, the reproductive elements which began the evolution do not reappear till the tissues of the body are so sparingly supplied with nourishment that there is only a small balance left for further growth and development. The connection between a small supply of nourishment, the cessation of all structural development, and the formation of pus, has been already noticed; so that there is a considerable degree of analogy between the series of phenomena which occur in inflammation and the evolution of an individual, a fact which is interesting in itself, and which confirms the analogies already noticed between the reproductive cells and contagium particles. In these remarks I have been led to anticipate what I shall have to say immediately, when applying the theory of Pangenesis to the explanation of the phenomena of the general diseases. In the mean time I shall return to the consideration of the facts which indicate the contagiousness of inflammatory products. The contagiousness of pus when it gains entrance into the circulation was under consideration when the experiments of Newport led us to make a digression from the subject. All I have to say upon that part of the subject at present is, that the contagiousness of the healthy tissues shown by their encroaching upon diseased tissues is as marked, other things being equal, in those secondary inflammations as in the primary inflammations caused by external irritation.

But not only does inflammation spread at the primary seat of irritation, and not only is it caused by the transference of inflammatory products through the medium of the circulation to remote parts of the body; but it may also be propagated by the direct transference of inflammatory products from one part of the body to another, or from one individual to another. A little gonorrhœal discharge, for instance, coming in contact with the mucous membrane of a healthy urethra, causes gonorrhœa. That the gonorrhœal discharge has no peculiar action upon the mucous membrane of the urethra, is shown by the fact that the same discharge may cause inflammation of the conjunctiva; and this is rendered still more manifest when it is considered that inflammation of the conjunctiva may be caused by inoculation with discharge from ordinary ophthalmia. In these, and other instances which could be mentioned, a little pus detached from a diseased tissue is grafted upon the similar tissue of another body, and it initiates changes similar to those which preceded its own formation in another individual. In the former instances of the contagiousness of inflammatory products noticed, it was remarked that the healthy tissues exhibited a counter-contagiousness in their tendency to encroach upon the diseased tissue; and a parallel fact can be adduced with regard to the cases of contagiousness under consideration at present. It is now very well known to all surgeons that, when a small bit of healthy skin is placed upon an ulcerated surface, it soon becomes incorporated with the part, and not only does it hasten the healing process, but the cicatrix produced is more like healthy skin. In this case a healthy graft has become incorporated with a diseased part, and the modification produced is to deflect the nutrition towards health. Here the contagiousness of health is as prominent a feature of the process as the contagiousness of disease is in other instances; and if this contagiousness is somewhat obscured by attending circumstances in the other cases alluded to, it is not the less certain that it exists.

It has now been found that inflammatory contagion pronounces itself in three prominent directions; first, at the seat of the primary irritation, whatever may be the cause of that irritation; second, by transference of inflammatory products from the primary seat of irritation to other parts of the body through the medium of the circulation; and lastly, by direct transference of inflammatory products from one part of the body to another part of the same body, or to another individual. It has also been found that, in each of the modes in which inflammatory contagion exhibits itself, the healthy tissues exhibit a counter-contagiousness by which they tend to encroach upon the diseased tissues. What has been said here with regard to the contagiousness of inflammation is little more than a statement of observed facts, and therefore true, whatever may be the explanation of these facts. It now remains for us to regard the general diseases from another point of view, and, with as much brevity as is possible, to apply the theory of Pangenesis to their explanation.

As an introduction to the application of Pangenesis to the explanation of the phenomena of the general diseases, the views which now obtain with regard to the formation of pus may be glanced at. The observations of Redfern and of Virchow led to the overthrow of the theory which regarded pus as consisting of a development of cells from lymph, coagulated blood, and fibrine. Instead of this theory was substituted that which regards the formation of pus as depending upon a rapid proliferation of tissue cells, especially those of connective tissue, with partial softening and disintegration of the intercellular substance; and it was also supposed that all this multiplication was quite independent of the vessels and of the morphological elements of the blood. The language employed in the preceding pages in discussing the inflammatory process is more in accordance with this theory; but the truth or falsity of the fundamental principles advanced is by no means bound up with the propriety or otherwise of the language employed. But the theory which ascribes the formation of pus to the proliferation of tissue cells is being gradually superseded by another. A few years ago Von Recklinghausen discovered that the cellular elements of connective tissue are of two kinds. The one kind are flat, nucleated cells, with long processes, and were long known; but besides these he found wandering cells identical in every respect with white-blood and lymph cells. Some time afterwards Conheim demonstrated that at an irritated point white-blood corpuscles wander from the blood into the tissues; hence it is concluded that all the young cells which are found in the tissues during the inflammatory process are wandering white-blood cells. It is proper, however, to notice that many able pathologists dissent from this theory; even while admitting the trustworthiness of the observations upon which the theory is based. It is very difficult to explain the increase of cells in the cartilage cavities, after irritation, by this theory; since it is highly probable that a white-blood cell could not penetrate through the dense intercellular substance. The views of Dr. Beale upon this subject are deserving of attention. He thinks that a pus-corpuscle may be derived from a white-blood corpuscle; but he also thinks that it may be derived from epithelial and other tissue cells. "There can be little doubt," he says, "that pus may be derived by very rapid growth from any germinal matter in the body."* This is, on the whole, the theory which will be adopted in the following pages; but in addition it will be assumed,

* "Disease Germs: Their Real Nature," p. 42.

in accordance with the theory of Pangenesis, that both in health and disease there is a constant interchange, not only between the fluids of the tissues and of the blood, but also of their morphological elements. The observations of Dr. Beale and of others show that pus-corpuscles and other young growing cells exhibit protrusions of their substance, which may become detached. Some of these detached particles are less than the $\frac{1}{100000}$th of an inch in diameter, and yet each particle may grow into a distinct cell; and the recent observations upon the structure of the capillary walls render it very evident that particles much larger than these could pass through with facility. In accordance with this view, therefore, when a part is irritated the whole of the active changes which ensue is not to be ascribed to the white-blood cells and to the capillary walls. The cells of the proper tissue of the part become more active, and in consequence the buds or gemmules of the white-blood cells which pass through the capillary walls become more numerous, and probably larger, which of course increases in a very high degree the cell multiplication going on in the part. But not only will a greater number of gemmules pass from the blood-cells to the part, but a greater number of gemmules will be absorbed from the cells of the part into the blood. The facts of inflammatory contagion, which have already been noticed, would be readily explained, if it be assumed that the gemmules cast off from the cells of the inflamed part inoculate the cells of the surrounding tissues, and that the gemmules absorbed into the lymphatic vessels and blood inoculate the correlated parts of the organism. The special affinities of cells for certain substances has already been noticed as a property which undoubtedly exists, but which has not yet been explained. It may therefore be assumed that the parts of the body which stand more immediately correlated to the primary seat of the irritation would suffer most; but gemmules would be absorbed from these parts which would affect other parts, so that finally a general effect would be produced which would reverberate throughout the entire system; the effect becoming more and more faintly marked as the part became more and more remotely correlated with the locality originally affected. The consequence is that in inflammations which are purely local a general effect is produced; and Dr. Beale has shown that, even in a common cold, there is an increase of the "germinal matter" throughout the entire organism. This increase of growth over the whole body involves, from the principles already laid down, a decrease of structural development, a decrease of functional power, and a destruction of intercellular substance; and as less of the force expended is devoted to structure and to function, a higher genesis of heat will result. When, from the changes already noticed, growth is checked by a decrease of nourishment, a reverse process takes place, in which the remotely affected parts of the organism cast off gemmules, by which the nearer correlated parts are assisted in their return to health, until finally the part originally affected, unless there has been great change or destruction of tissue, may return to a condition approaching to health. This application of Pangenesis to the explanation of the phenomena of the general diseases is very imperfect; but since it will be applied in greater detail to the zymotic diseases, the reader may, by a process of after reflection, supply the links which are missing at present.

CHAPTER IV.

THE ZYMOTIC DISEASES.

THE SMALL-POX GROUP.

Having now considered the general diseases, the next great division which should be discussed, if due logical order were observed, is the Specific. I shall, however, limit my remarks to a small portion of the latter, since my object is, not to treat disease in its widest aspects, but only in so far as the graft theory is directly, or indirectly applicable to it. The signification which has, provisionally, been attached to specific is, that a disease be caused by a special agent in the environment, and presenting an *ensemble* of symptoms capable of definition. When morphia, for instance, is absorbed into the body, it causes a more or less constant group of symptoms susceptible of definition; and since the correlative of symptoms are molecular changes in the organisation, it follows that the constancy of symptoms indicate constancy of change in a definite tract of tissue. It is impossible to tell why certain agents act upon certain tracts of tissue, and not upon others: the specific affinities of cells for certain materials, is to us at present an ultimate fact. But, as Virchow says, "nearly everywhere do we find specific relations or affinities to exist. If we cast our eyes upon the glands, it is a well-known fact that there are specific substances, by which we are enabled to act upon one gland, and not upon another; to rouse the specific energy of one gland, whilst all the rest remain unaffected."* The fact of the affinities of certain tracts of tissue for certain substances is generally made the basis of our classification of poisons, and frequently of therapeutical agents, and this alone is sufficient to show that the diseases produced are capable of more or less strict definition. But I do not wish to discuss those diseases which are caused by the introduction of chemical agents into the body; my remarks shall be confined to the zymotic diseases, and even the denotation of this term shall be limited in one or two directions. Dietic diseases, such as scurvy and rickets, are frequently included amongst the zymotic diseases; but great confusion must result from grouping together diseases which arise from something implanted in the body from without, and those which arise from a deficiency or excess of some element of the food. Again, if the views which are advocated here are correct, it is impossible to group together with advantage the known animal and vegetable parasitic diseases, and those which arise from the inoculation or grafting

* "Virchow's Cellular Pathology," by Dr. Chance, p. 293.

of portions of diseased tissue upon the bodies of the healthy. I shall, therefore, for the sake of present convenience, even though many theoretic objections might be urged against such a proceeding, exclude the known parasitic diseases from the zymotic class. But if I use the term zymotic to embrace all contagious diseases which are not of known parasitic origin, I must guard myself from misconception in another direction, before applying the graft theory to the explanation of the phenomena of those diseases. Evidently the graft theory will not apply to any disease unless its contagion consists of portions detached from the solids of the body, however minute those portions may be. But although it is proved that the contagium of small-pox is "particulate," it has not been proved that the contagium of influenza consists of particles. The contagium of influenza may hereafter be proved to be gaseous, and if it is, the phenomena of influenza cannot be explained by the graft theory. But until this point is determined by a special investigation for each disease, I shall assume that the strictly contagious diseases are propagated by minute particles, and shall include the contagious and inoculable diseases under the general term of zymotic; merely remarking that if the progress of discovery should decide against this view, the terminology adopted here must be modified accordingly.

By contagious diseases are meant, not those which exhibit contagious properties, or which are able to propagate themselves by contact, since we have seen that those properties are exhibited by most inflammatory products, but the contagious diseases are distinguished by being propagated only by contagion. But it may be objected that this is taking for granted one of the principal points which ought to be proved, since it is assumed that the contagious diseases do not arise *de novo*. It must be remembered, however, that my aim at present is not to arrive at scientific definitions, but to make practical distinctions which will enable me to proceed with the argument; and since the question of the spontaneous generation of contagious diseases has only an indirect bearing upon my subject, I decline to hamper myself with its consideration. It will be more convenient for my purpose to limit the denotation of contagion so as to exclude the inoculable diseases. The contagious diseases, therefore, are those in which the contagium takes effect upon the healthy by settling either upon the external or internal surface without any obvious abrasion of the cuticle, while the inoculable diseases are those in which there is a direct transference of the contagious matter to an abraded portion of the envelope of another individual. It is obvious, however, that this latter distinction is not of much consequence, and a little latitude will be allowed in the use of the terms, contagious and inoculable, where the context is such as to indicate the meaning which attaches to them. With these remarks upon the meaning of the terms employed, I shall pass on to the consideration of the zymotic diseases. In order, however, not to be lost in too great generalities, I shall devote this chapter to the application of the graft theory to the explanation of the phenomena of a well-marked group of diseases, and shall for various reasons select for that purpose the small-pox group.

Let us attend to what takes place after a little vaccine lymph is placed upon the arm, when the skin is slightly scratched. It is not necessary for me to enter into a minute description of the changes which ensue at the seat of puncture, since they are familiar to all. These changes are more or less similar to what takes place in an ordinary inflammation when a pustule is formed; but, besides those characters which are common to it, and to ordinary inflammation, the vaccine vesicle has characters of its own, and it affects the body in a much more profound manner than anything which occurs in ordinary inflammation of the same extent. Can any explanation be given of the characteristics in which the vaccine vesicle differs from ordinary inflammation? I do not know of any explanation of this phenomenon, but it can be brought into relation with a great many other well-ascertained facts, and this will merge a special into a general question. The most remarkable series of facts which I know having a bearing upon this subject is the production of galls. The greater number of galls are produced by a species of Cynips, and the opinion which Mr. Darwin sanctions is, that the poisonous secretion of the insect causes the gall. The following facts are taken from Mr. Darwin's great work. No less than fifty-eight kinds of galls are produced on the several species of oak; and one American species of willow bears ten distinct kinds of galls. But notwithstanding the great number of different kinds of galls, they "afford good, constant, and definite characters, each kind keeping as true to form as does any independent organic being."* "The fact," adds Mr. Darwin, "becomes still more remarkable when we hear that, for instance, seven out of the ten different kinds of galls produced on *Salix humilis* are formed by gall-gnats (*Cecidomyidæ*), which, 'though essentially distinct species, yet resemble one another so closely, that in almost all cases it is difficult, and in some cases impossible, to distinguish the full-grown insects one from the other.' For in accordance with a wide-spread analogy we may safely infer that the poison secreted by insects so closely allied would not differ much in nature; yet this slight difference is sufficient to induce widely different results. In some few cases the same species of gall-gnats produces on distinct species of willows galls which cannot be distinguished; the *Cynips fecundatrix*, also, has been known to produce on the Turkish oak, to

* "Animals and Plants under Domestication," vol. ii. p. 282, *et seq.*

which it is not properly attached, exactly the same kind of gall as on the European oak. These latter facts apparently prove that the nature of the poison is a much more powerful agent in determining the form of the gall, than the specific characters of the tree which is acted on." These facts and inferences are so much to the point and so important that they have been given at length in Mr. Darwin's own words. If, therefore, upon the same plant a variety of different galls are produced by a minute drop of poison from insects, which are so nearly alike as to be almost undistinguishable, and if a drop of poison from the same insect will produce a similar modification of plants which differ very considerably from each other, it need not surprise us that vaccine lymph produces vesicles which differ from those of ordinary inflammation, and that the contagious particles of the other contagious diseases give origin to products and symptoms which "afford good, constant, and definite characters, each kind keeping as true to form as does any independent organic being."

But besides the characters in which the vaccine vesicle differs from an ordinary vesicle or pustule, there are other points in connection with the course and progress of cow-pox which require consideration and explanation if possible. About the fifth day after the lymph has been inserted into the arm febrile phenomena become established, and this disturbance is well expressed by the tenth or eleventh day. About the time the fever begins the lymphatics of the axilla become tender, and when the fever is at its height they are much engorged and painful. About the eighth day an inflamed ring or areola begins to form round the base of the vesicle, which continues to extend till about the tenth or eleventh day; that being the time in which the fever is at its height, and the swelling of the axillary glands is greatest. It will be as well to carry on the deductive interpretation hand in hand with our description of observed facts. It is very evident that the vesicles, the areola, the swelling of the glands, and the fever, are bound together by some casual connection; and, indeed, there can be little doubt that the vaccine vesicle is either directly or indirectly the cause of the areola and of the glandular enlargement. But the areola and glandular irritation are only other instances of facts which have met us already; the areola is an instance of the propagation of disease by local contact, and the glandular irritation an instance of the propagation of disease by contact with the products of disease in the circulation. According to the theory of Pangenesis the cells of the vaccine vesicle at the different stages of its progress cast off gemmules, which in the first place affect the cells with which they are in contact, the cells becoming less and less affected as the circumference affected widens. But some of those gemmules are absorbed by the lymphatics, and these come in contact with the cells of the nearest glands. The cells are fertilized by union with the gemmules, become more active, a greater flow of blood takes place to administer to their demands, and engorgement of these glands results. As already remarked, the vesicle, the areola, and the irritation of the glands are only facts with which we were familiar when discussing common inflammation; and now we find them presenting themselves under new circumstances. But it has just been noticed that the vaccine vesicle presents certain special characteristics; and some of these characteristics are communicated to the tissues surrounding the vesicle and to the lymphatic glands. This circumstance is not so manifest in the case under consideration, but it is very pronounced in many other diseases. The syphilitic infecting sore, for instance, at first runs a course not unlike an ordinary pustule, but after the pustule bursts the tissues surrounding the ulcer become indurated, and the ulcer shows no tendency to a reparative process. The chief characteristic of the sore is chronic induration, and this is the exact character of the affection of the neighbouring lymphatic glands in syphilis. In this case the primary sore has impressed its own characteristics upon the disease it communicates to the glands; and this fact may be noticed in other diseases—as in the glandular enlargements in the neighbourhood of scrofulous eruptions, and in that of cancer. The most noticeable characteristic of the glandular enlargement after vaccination is, that it is, like the course which the vesicle pursues, very evanescent; if there is no other taint of the constitution the swelling of the gland soon subsides. Another important point is, that the vaccine lymph is much more contagious than anything which characterizes the products of ordinary inflammation. This is seen in the facility with which lymph is inoculated upon a healthy individual, and in the greater amount of local disturbance produced than in the case of a common inflammation of the same size. Now this intensity of action at the primary point of insertion also expresses itself in the surrounding areola, and in the glandular enlargement; showing that, in the language of Pangenesis, the gemmules cast off by the cells of the primary vesicle have more actively contagious properties than those cast off by the cells of an ordinary pustule. And this intensity of action also expresses itself in the accompanying fever; since it is out of proportion to the local disturbance if it were ordinary inflammation. The connection between the fever and local inflammation has already been noticed, and an explanation was given of the fact that in inflammation there was a general increase of cell growth through the greater part of the body; and the apparent disproportion between the local disease and the general disturbance after vaccination may find its interpretation in the fact that the gemmules of the vaccine vesicle are more actively contagious than those of an ordinary vesicle, and thus they are enabled to infect a comparatively large tract of tissue in a short time, and are also able to produce a higher action in the tissue affected.

We see, therefore, that the primary vesicle impresses some of its own characteristics upon the disease which it propagates to other tissues; and on the supposition that the disease is caused by contact of the cells of the tissues affected with gemmules from the primary vesicle, the facts just stated are no more inexplicable than the fact that a sperm-cell impresses the characteristics of its ancestor upon the individual which results from its union with the germ-cell. Here, again, a special difficulty has been resolved into a general difficulty; and there we must leave it.

But it is not probable that all the gemmules cast off by the cells of the primary vesicle are arrested in the surrounding tissues, or taken up by the lymphatics of the part. Mr. Darwin assumes that these gemmules are so minute as not only to circulate in the fluids of the body, but also to be thoroughly diffused through its solids. Are there any facts which tend to show that vaccine gemmules which have not passed through lymphatic glands are diffused through the system? A very curious fact has come to light which has been turned to practical account by Mr. Bryce in testing whether vaccination has exerted its protecting influence upon the system. When another part of the body is vaccinated four or five days after the first vaccination, in the generality of cases the second vesicle will appear and go through its changes so rapidly, that it will be at its height, will decline, and disappear as early as the vesicles of the first vaccination. This shows that as early as the fifth day the tissues at the point where the lymph is inserted the second time have already undergone a modification. What the nature of that modification may be we can only guess. Suppose that gemmules are absorbed by the capillaries, and that these on entering the circulation begin to act on the tissues of the body, for which they have an affinity, two or three days before the second operation. The cells of those tissues will in that case have acquired the motion communicated to them by those gemmules, although this motion, if not supplemented, would not of itself be sufficient to cause an outward lesion. The lymph of the second vaccination comes in as this supplemental force, and a vesicle is produced. This vesicle, however, presents one or two peculiarities which deserve consideration. It is noticeable that this vesicle is much more rapid in the first stages of its evolution than the primary vesicle, and yet it does not equal the latter in intensity of action, whether measured by the local or secondary disturbance produced. It, however, attains to its maturity at the same time with the primary vesicles, and observes a similar gradation with them in its order of decline and disappearance. Other things being equal, rapidity is associated with intensity of action; but the first stage of the evolution of the second vesicle exhibits rapidity of action, and yet the intensity of that action does not equal that of the first vesicles. This shows that the circumstances in which the first and second vesicles are formed are not similar, and therefore amongst those differing circumstances must an explanation be found. As just assumed, the gemmules absorbed by the capillaries have already communicated their motion to the tract of tissue for which they have an affinity, and this motion is in a direction similar to that of the first stage of the vaccine vesicle; hence, when the second lymph is inserted, the tissues have undergone part of the evolution already, and the new molecular motion being in the same general direction both the intensity and rapidity of the action already begun is increased, so that a vesicle is produced which will arrive at maturity in a short time. On this supposition, however, the cells of the tissues in which the second lymph is inserted have already acquired a motion similar in kind to that which is communicated to them by the lymph particles; so that in the second vaccination there is less difference between the molecular motion of the vaccine particles and that of the cells of the tissues than there was in the first vaccination. But it has already been mentioned that, although there must be a general similarity between the germ-cell and sperm-cell, there must also be a considerable degree of difference in their molecular motions, otherwise no evolution results; and it has been seen that some plants are absolutely infertile with their own pollen, while in other cases the self-impotence is not absolute, but the seedlings produced are weakly. If, then, there is a less degree of difference between the molecular motion of the lymph and the cells of the epidermis in the second, than in the first vaccination, this would account for the second vesicle and its areola being smaller, and showing a less intensity of action when compared to that of the primary vesicle; and it has already been seen that the rapidity of the primary stages of the second vesicle is accounted for on the supposition that the cells of the epidermis have undergone a certain part of the evolution prior to the insertion of the lymph.

But the evolution of cow-pox is not completed when the primary vesicle, the areola, and the enlargement of the neighbouring glands begin to subside. About the eleventh day another event of great importance occasionally ensues. Dr. Aitken says, "If the weather be hot, children of full habit not unfrequently show on the extremities, and less copiously on the trunk a lichenous, roseolar, or vesicular eruption, which commonly continues for about a week."* The eruption which occasionally appears about the eleventh day is, in my opinion, abortive vaccine vesicles, and several considerations can be adduced to confirm this opinion. In the first place, it begins about the eleventh day, which is just the time when the general eruption begins in

* Dr. Aitken's "Practice of Medicine," vol. i. p. 295.

inoculated small-pox; this of itself is sufficient to suggest the idea that the roseolar eruption of cow-pox is only a different degree of the secondary eruption of inoculated small-pox. This idea is much strengthened when it is considered that occasionally the secondary eruption of true small-pox presents a graduated series from roseolar rash to the matured small-pox pustule. In some cases the secondary eruption of small-pox is preceded by a red rash, which is with difficulty distinguishable from the eruption of scarlatina; and the rash which appears secondary to vaccination also presents a certain graduation which is very suggestive. Sometimes the rash is roseolar, while at other times it is vesicular, and this, taken along with the other considerations adduced, appears to indicate, on the one hand, that the vesicular eruption is only a degree which if more violent would graduate into the stages of pustule, scale, and desiccation; while, on the other hand, it is equally suggestive of the conclusion that in the cases in which no eruption appears that the whole tract of tissue which is liable to the eruption of small-pox has been profoundly modified. If the latter conclusion be accepted it will merge the question of the immunity of small-pox afforded by vaccination into the general question of the immunity afforded by one attack of a contagious disease to a subsequent attack. Underlying this argumentation is the assumption that cow-pox and small-pox are essentially the same disease, and reasons will hereafter be adduced for believing that this is the case. If, then, this eruption consists of abortive vaccine vesicles, according to the theory of Pangenesis, the cells of the tissues which have an affinity for the variolous poison must have been fertilized by union with vaccine gemmules diffused through the system. Whence are these gemmules derived? It is not probable that they are the direct descendants of those originally cast off by the cells of the primary vesicles. It has already been noticed that during the progress of the primary vesicles coincident changes are taking place in what I may briefly term the variolous tract of tissue. There are good grounds for believing that these changes begin before the fifth day, so that it is not probable that the same or similar gemmules could produce a new modification of the same tissue on the eleventh day. But according to the theory already advanced some of the gemmules cast off by the cells of the primary vesicles have been absorbed by the lymphatic vessels, and have impregnated the cells of the nearest lymphatic glands. The affection of the glands becomes apparent about the fifth day, and about the tenth or eleventh the engorgement is at its height. If, then, we assume that the cells of the lymphatic glands cast off gemmules about the tenth day, are there any grounds for believing that these would act on what I have already called the variolous tract of tissue? The rete mucosum is supposed to be the seat of the active changes in the variolous eruption. But Dr. Burdon Sanderson has described a tissue similar in morphological characteristics, and probably in function to the proper tissue of lymphatic glands. He calls this tissue adenoid,* and he finds that it is scattered in minute patches through almost every tissue of the body, and that it plays a very important part in the development and propagation of tubercle. Some of this tissue is found in the connective tissue in which the hair bulbs and cutaneous glands are embedded; and I would suggest the question whether this may not be the tissue in which the active changes which lead to the small-pox eruption commences. If the adenoid tissue in the vicinity of the hair bulbs and cutaneous glands is the centre of active change during the small-pox eruption, this would explain why small-pox appears as an eruption instead of a diffused inflammation affecting all the parts of the epidermis alike. I do not, however, lay much stress upon this point, since the affinities of the tissues for certain materials are so refined that it is probable we may never, or at any rate not for a long time, be able to connect a special affinity for a certain substance with definite morphological characteristics. But to return to the point more immediately under consideration. If it be assumed that the active changes leading to the small-pox or vaccine eruption take place in this

* "Eleventh Report of Medical Officer to Privy Council" (1868), p. 159, *et seq.*

adenoid tissue, does it in any way strengthen the opinion that the eruption is caused by gemmules cast off from the cells of the lymphatic glands? As just stated, the lymphatic and adenoid tissues are closely allied to each other in morphological characteristics, and probably in function; but owing to the different position each of these tissues occupies in the organism, the forces incident upon the one must be different to those incident upon the other; hence, with a general agreement, these tissues must also possess a considerable degree of difference in molecular motion. But it has more than once been pointed out that the evolution of a new individual, and the vigour of the individual when evolved, depend upon the sperm-cell and germ-cell having, along with a general agreement, acquired a considerable degree of difference of molecular motion. Hence, upon the assumption that the general rash which frequently occurs after vaccination is caused by the union of the gemmules cast off by the cells of the lymphatic glands with the cells of the adenoid tissue of the skin, we not only gain the requisite time, but this supposition helps us to explain why the general affection appears in the form of an eruption, and it is also more in accordance with the analogies afforded by reproduction. With the disappearance of the general rash it is generally supposed that the evolution of cow-pox is completed, at any rate we cannot trace it further; and having now endeavoured to account for the primary vesicles, the areola, the glandular enlargement, the febrile symptoms, and the secondary rash, we may pass on to the consideration of small-pox.

Before proceeding further, however, it will be better to adduce the evidence which tends to prove that cow-pox and small-pox are essentially the same disease; since, as already stated, this assumption underlies some arguments previously advanced. When a cow is inoculated with small-pox, all that occurs is the production of a small reddish papule at each point of insertion. These papules are not followed by much general disturbance, nor by a general eruption; in short, the effects both local and constitutional are so insignificant that it might be supposed that the papule at the point of insertion was merely the result of the slight local injury, and the insertion of a little common pus, instead of being produced by any special action of small-pox contagion. Professor Chauveau * conducted experiments to determine this, but the results of these can only be mentioned here in a very general way. Liquid taken from the papules, resulting from the inoculation of an ox with variolous matter, was employed for the inoculation of a child. The general result of this and similar experiments was that at a certain proportion of the punctures a vesicle appeared which absolutely resembled those of

* See "Twelfth Report of the Medical Officer of Privy Council," Dr. Sanderson's Essay, p. 237.

cow-pox, and this was followed by a general eruption similar to, but probably on the whole milder than what occurs after inoculation with small-pox matter. He also found that it was impossible to produce the variolous papule on cows which were once vaccinated. On the other hand, cows which had not had cow-pox nor been vaccinated were readily variolated; but subsequent to this they were insusceptible of being vaccinated, showing that, slight as the papule had been, it was sufficient to produce a permanent protective influence upon the cow. It is evident, therefore, that variola passes through a very modified course in the cow, and it is probable that if the variola were passed from cow to cow for generations, the modification produced would be still greater. This is rendered more probable by some of the modifications produced on races by crossing. "Lord Orford," says Mr. Darwin, "crossed his famous stud of greyhounds once with the bull-dog, which breed was chosen from being deficient in scenting powers, and from having what was wanted, courage and perseverance. In the course of six or seven generations all traces of the external form of the bull-dog were eliminated, but courage and perseverance remained."* In this instance the most marked effect was produced in the first cross; but from repeated breeding with the greyhound one character after another was lost, and only one or two remained permanent. Similarly

* "Animals and Plants under Domestication," vol. ii. p. 95.

with regard to the variola, it is probable that the greatest effect would be produced by the first inoculation, and that by subsequent inoculations from cow to cow some characteristics would be lost, until finally a modified pox would be produced which would become natural to the cow. The phenomena of grafting also present more or less analogous facts. I allude to those of what is called double-grafting. "It has already been mentioned incidentally," says Dr. Masters, "that some pears will not graft readily on the quince, and, consequently, that mode of enhancing fertility could not be adopted were it not for the ingenious process of double-grafting, which is thus effected. In the first place, some free-growing pear is grafted on to the quince in the usual manner, and then the scion so obtained is, in its turn, grafted with a variety that will not unite readily with the quince in the first instance. In this indirect manner the quince stock is made to affect the scion, throw it into bloom more quickly, and enhance its fertility."* The object of the gardener is, not to experiment in the scientific sense, regardless of the practical utility of the result, but to increase the fertility of his fruit-trees; if, on the other hand, his object were to diminish the fertility of his fruit-trees, I have no doubt that by a little experimenting this object could be attained. The object of the physician, on the other hand, is to produce a disease graft, with diminished fertility, using this

* *Popular Science Review*, April, 1871, p. 149.

latter in a wide sense so as to embrace the incitement to continuous as well as to discontinuous growth. So far, then, as small-pox is concerned, the physician has succeeded in producing, or rather in discovering a graft of the required fertility; and if experiments could be carried on with disease grafts as readily as with vegetable grafts, I have no doubt, that by a process of experimentation grafts of the other contagious diseases could be produced of the requisite modified fertility.

What the nature of the modification may be upon which the increase or diminution of the fertility depends it is impossible to say; but there can be no doubt that the most opposite results may ensue from a modification so slight as to be inappreciable to our most refined means of observation. The fact that many plants are absolutely infertile with their own pollen has already been mentioned. "It is interesting to observe," says Mr. Darwin, "the graduated series from plants which, when fertilized by their own pollen, yield the full number of seed, but with the seedlings a little dwarfed in stature—to plants which, when self-fertilized, yield few seeds—to those which yield none; and lastly, to those in which the plant's own pollen and stigma act on each other like poison."* In these cases the different kinds of pollen may be so similar in morphological characters as to be nearly undistinguishable, as in the experiments of Mr. Darwin upon dimorphic and

* "Animals and Plants under Domestication," vol. ii. p. 141.

trimorphic plants;* and yet the one kind produces the maximum of motion, while a second has no appreciable effect, and a third arrests the motion which constitutes life. Nor are we without more or less analogous facts in physical science. "Extraordinary effects," says Professor Tyndall, "are produced by the accumulation of small impulses. Galileo set a heavy pendulum in motion by the well-timed puffs of his breath. Ellicot set one clock going by the ticks of another, even when the two clocks were separated by a wall."† But by altering the rhythm of his puffs of breath so that each would act on the pendulum when swinging towards him, Galileo might have arrested the motion; and similarly the tick of one clock may be arrested by that of another by a slight alteration in the periods of their motion. These facts are not adduced as an explanation of the diminished fertility of cow-pox; but something is offered to the human mind to rest upon, when facts which are very mysterious are brought into some relation with facts which are easily explained. These remarks render it probable that contagious tissues may differ both in the degree and in the kind of the effects produced when our means of observation do not enable us to connect this difference with morphological characteristics. An attempt will be made hereafter to connect the

* "Animals and Plants under Domestication," vol. ii. p. 181, *et seq.*

† "Fragments of Science for Unscientific People," by Professor Tyndall, p. 427.

major differences of degree in contagiousness with definite morphological changes in the tissues; but the morphological differences are accompanied and caused by differences in the molecular motion of the units; and since, as has just been noticed, the molecular changes of motion do not always declare themselves by producing appreciable changes in the form of the units, it is impossible to predict what will be the full contagiousness of a tissue from morphological characteristics alone. The full significance of this remark will become more apparent hereafter; in the mean time we must return from this digression to the subject more immediately under discussion. An attempt has been made to show that cow-pox and small-pox are essentially the same disease, and now we must pass on to apply the theory of Pangenesis to the explanation of the latter disease.

If, then, cow-pox is merely small-pox modified by having passed through the bovine species for a succession of generations, and if the general theory adduced here to account for the phenomena of the former be admitted, it can be applied with scarcely any variation to the explanation of the latter. So far as inoculated small-pox is concerned, the principal circumstance in which it differs from cow-pox is, that in the former a general eruption appears which passes through all the changes of the primary pustules, while in the latter there is either no eruption at all or only a slight vesicular one. All that requires to be said in explanation of this difference is implied in what has been stated already. When speaking of the want of a general eruption in cow-pox, it was said that the gemmules had not the requisite fertilizing power to produce an outward lesion, or at most only a very slight eruption; and all that can be said in the present instance is that the gemmules have the requisite fertilizing power to produce an eruption which goes through all the changes of the primary pustules. In neither instance, however, are we able to define the molecular constitution of the gemmules upon which the infertility of the one and the fertility of the other depends; although, from what has been said, we may assume that a certain difference in their molecular motion must exist.

From inoculated small-pox we may pass on to the consideration of the natural variety. Although the inoculated and natural small-pox are undoubtedly the same disease caused by the same morbid poison, yet there are several very important differences between them. The first is, that in the natural variety there is no primary pustule on the skin such as appears after vaccination and inoculation. This in all probability arises from the fact that the contagium particles in natural small-pox gain entrance into the body through the mucous membrane of the respiratory or of the digestive organs. It is not known whether any local effect is produced upon the mucous membrane; or whether the particles are absorbed at once into the circulation. Judging from analogy the former is the case. One or more pustules might form on the mucous membrane of the lungs, or of the digestive canal, without producing symptoms sufficiently marked to attract attention. If this is the case the first part of the latent period of the natural small-pox differs in no essential particular from the stage of the primary pustule of the inoculated variety. But the most remarkable difference between the natural and inoculated small-pox is, that in the former the time which supervenes between exposure to the contagion and the appearance of the general eruption is about a third longer than in the latter. I cannot give a satisfactory explanation of this remarkable circumstance; but even a guess, however imaginary, is better than no attempt at all being made at interpretation. It must be remembered that I am not at present discussing principles, but combining and converging principles to the explanation of concrete phenomena; and in such a case a much greater latitude in the use of the imagination is permissible than if the principles themselves were under consideration. The whole of this application of Pangenesis, and of the principles already advanced, might be proved to be erroneous; and yet the principles themselves might still remain unaffected. According, then, to the theory advanced here, the contagium particles in inoculated small-pox first impregnate the cells at the seat of operation; these about the fifth day cast off gemmules which impregnate the cells of the nearest lymphatic glands, and the latter about the tenth day cast off gemmules which fertilize the cells of the epidermis or of the adenoid tissue of the skin. Let us suppose, for instance, that in the natural small-pox the contagium particles gain entrance into the stomach—which they may easily do with the saliva even when no food is taken—and impregnate the cells of part of the mucous membrane of the digestive tract, that on the fifth day the fertilized cells cast off gemmules which impregnate the cells of the mesenteric glands, and that these in their turn, after other five days, cast off gemmules which impregnate the cells of the liver, the cells of which, after other five days, cast off gemmules which fertilize the cells of the adenoid tissue of the epidermis; this will give us fifteen days, which is the length of the stage of incubation in natural small-pox. In inoculated small-pox the contagium particles were supposed to have impregnated two sets of tissues before producing the general eruption; but in the natural small-pox three sets of tissues are supposed to be impregnated by successive crops of gemmules before the fertilization of the cells of the adenoid tissue of the epidermis; and this not only gives the requisite time, but the greater multiplication of gemmules consequent upon the series consisting of three instead of two terms may account for the much greater severity of the natural than of the inoculated variety. To show that the severity of the disease may be expected to vary in intensity according to the number of the gemmules, we may again have recourse to the analogies afforded by the concourse of the reproductive particles. Newport's experiments with the ova of Batrachians have already been mentioned, and we may here quote, from Mr. Darwin's work, Naudin's experiments with the pollen-grains of mirabilis, as very apposite instances of the principle to be illustrated. The pollen-grains of this plant are very large, and the ovarium contains only a single ovule; "and these circumstances," says Mr. Darwin, "led Naudin to make the following interesting experiments: a flower was fertilized by three grains and succeeded perfectly; twelve flowers were fertilized by two grains, and seventeen flowers by a single grain, and of these one flower alone in each lot perfected its seed; and it deserves especial notice that the plants produced by these two seeds never attained their proper dimensions, and bore flowers of remarkably small size."* These facts, then, render it probable that the severity of the general eruption in natural small-pox stands related to a greater multiplication of gemmules in the system prior to their union with the epidermic cells. But, as already stated, the whole of this attempted explanation is purely imaginary, and if the contagium particles gain entrance into the system through the lungs (and there can be little doubt that occasionally they do), I am unable

* "Animals and Plants under Domestication," vol. ii. p. 364.

even to guess what tissues become affected prior to the general eruption.

Although the differences between the discrete and confluent small-pox are of very great importance practically, yet theoretically the difference appears to be only one of degree. It may, however, be noticed that the confluent variety runs rather a shorter course than the discrete. This may depend upon the quality or the quantity of the original contagium, or probably upon a certain predisposition upon the part of the recipient. But upon whatever the differences between the discrete and confluent varieties depend, it would appear that the impetus given by the contagion sends the confluent through its stages more quickly than the discrete disease. One of the facts discovered by Newport in the experiments already mentioned is very interesting with regard to this point. I allude to the fact that the rate of the segmentation of the yelk of the ova of Batrachians is determined by the number of spermatozoa applied. This fact is particularly noticed, because the severity of the natural small-pox in comparison with the inoculated variety has hitherto been explained on the supposition that when a person was breathing an atmosphere charged with contagium particles, more entered the system than in inoculation. But if this were the only cause of the differences of the two varieties, we might expect that, according to the analogies just mentioned, the stage of incubation in the natural would be shorter than in the inoculated pox; but the reverse of this is the case. Hence the differences between inoculated and natural small-pox are clearly not entirely dependent upon the quantity of the contagium particles which are absorbed into the system. With regard to the secondary fever, I believe it to be in great measure dependent upon the local affection; other things being equal, it varies in intensity directly as the confluence of the eruption; and in time it corresponds to the "maturation" of the disease, and therefore comes within the scope of the theory of suppurative fever.

After the secondary fever subsides the specific action of small-pox is supposed to be at an end, and the further disturbances which arise are due to what are termed sequelæ. The only definite meaning which I can attach to this language is, that the subsequent perturbations might be produced by any injury to the system, provided it were as extensive as that caused by small-pox, and that it affected the same tissues. This may be true, but the sequelæ themselves observe some degree of regularity in their order of appearance; but I shall reserve the few remarks I intend to make upon this point until after the consideration of the other contagious diseases.

It is unnecessary to speak at any great length of the modified small-pox which occurs after successful vaccination, since the explanation of its mildness, which is its principal characteristic, was given by implication when treating of cow-pox. It appears, from what has already been said, that in the contagious diseases the contagium particles have an affinity for a certain tract of tissue, either directly or through the intervention of changes caused in other tissues; and when that tract of tissue has once undergone the motion which constitutes the disease, it is no longer capable of acquiring that motion by subsequent contact with the contagium particles of the same disease, or at any rate only capable of acquiring that motion to a very modified extent. Reasons have already been adduced for believing that cow-pox and small-pox are essentially the same disease, and that after vaccination the whole tract of tissue which has an affinity for variolous matter is modified, although the disturbance produced is not so great as to cause inflammatory action; and we must also believe that the subsequent nutrition of this tract of tissue is carried on according to the altered type. When, therefore, the contagium particles of small-pox are grafted upon a body so modified, they produce a less degree of disturbance than would be produced by them prior to the modification. But facts have already been adduced which tend to show that the degree of modification of the system produced in all instances is not the same, even when the vaccine vesicles are perfect. We have seen that after vaccination there is occasionally a general eruption, which is in some cases vesicular and in others papular. Here, then, are the outward manifestations of two grades of general effect, and even when no outward lesion is produced we inferentially believe the tissues to be modified to a very considerable extent. A fourth grade can easily be imagined, in which the gemmules are not sufficiently active, or not sufficiently numerous, to cause a general effect at all, or only an effect so slight that what I have called the variolous tract of tissue soon regains, by the contagiousness of healthy tissue, the normal type of its nutrition. To these gradations of general effect produced by vaccination we may expect to find in the reverse order corresponding gradations in the severity of modified small-pox. It is known, as a matter of clinical observation, that these gradations in the severity of modified pox do occur, although I am not aware that clinical observation has shown the inverse relation which the degrees of the modified pox bear to the degrees of general effect produced by vaccination; yet it may be inferred that such a connection exists. And if it does, we may expect to find in practice small-pox after vaccination of every degree of severity, from cases equal in severity to the natural pox, down to those persons who are so protected as to have a complete immunity from the disease. Of those cases where the disease appears to run a natural course it is unnecessary to speak further, but I will briefly mention a few of the modifications of the general eruption met with in practice. The eruption may run its natural course up to the sixth or seventh day, when it stops, instead of going on to the eighth or ninth day. It may be remarked in passing, that this forcibly reminds one again of Newport's experiments upon the ova of Batrachians, which, "when only a few spermatozoa are applied are only partially impregnated, and the embryo is never fully developed." Although the arrest of development in the case of the modified pox probably does not arise so much from a deficiency in the number of the fertilizing elements, as from the molecular motion of the fertilizing and fertilized elements being too similar to generate by their union a sufficient motion to send the pox through the different stages of its development. But to proceed—the eruption is sometimes very scanty, while at other times a few pustules, vesicles, and pimples are found at the same time on the body. Occasionally the eruption is so copious as to be confluent, but its development does not proceed beyond a pimple or a vesicle, which begins to dry up on the fourth or fifth day. In other cases, again, instead of a vesicular or papular eruption, there is a scarlet efflorescence like that of scarlatina or of roseola, succeeded in some cases by a few pimples. Again the fever may be severe while the eruption is so scanty that only one or two pustules make their appearance; and lastly, during epidemics of small-pox, cases occur in which there are three days' fever similar to the fever of small-pox; but it is not followed by any eruption at all. Here, then, observation reveals the gradations in the severity of the modified pox, which deductive reasoning had led us to expect. To sum up, then, it appears probable that in the modified disease which occurs after vaccination a few of the cases have no eruption at all. In other cases the eruption is rose-rash graduating into pimple; and this graduation may go on through vesicle, modified pox, up to cases where the eruption appears to run its natural course. These facts very much strengthen the whole of my argumentation, and more especially the opinion already advanced that the papular and vesicular eruptions which occasionally appear after vaccination are abortive pox, and that in the cases where the disturbance has not been sufficient to cause any eruption; yet the tissue which is the seat of eruption in small-pox has been attacked and more or less permanently modified in its nutrition. Another circumstance which may affect the character of the modified small-pox is the time which has elapsed after vaccination. Sir James Paget has treated this part of the subject with his accustomed ability, and to his remarks I must refer my readers. He thinks that the character impressed by cow-pox upon the tissues may disappear, just as a cicatrix improves in appearance with the lapse of time. Both are instances of "that inner law, that after a part has changed by disease it tends naturally to regain a perfect state."* But this "inner law" is only empirical, and therefore it is desirable that it should

* "Surgical Pathology," edited by Professor Turner, p. 39.

be resolved into other laws which are more fundamental. It has already been noticed that when a tissue acquires a certain variation in the molecular motion of its units, it becomes contagious; but if the units of a locality differ in molecular motion from the units of the correlative parts of the organism, it is also evident that the latter must also differ from the former. It follows, therefore, that while the units of the diseased locality will, by contagion, tend to propagate the disease to the correlated parts of the organism, the correlated parts of the organism will also tend to propagate health to the diseased locality. During the active stage of the morbid process the contagiousness of the disease is the most marked feature of the case; but when the diseased process becomes more or less passive the contagiousness of health begins to manifest itself upon the diseased locality, and the tissues of the latter will ever tend to health. The qualifications which this statement require will become more apparent hereafter; in the mean time what concerns us most to notice is, that the tendency which is manifested by morbid tissues to regain a condition of health is only a branch of that tendency which healthy tissues exhibit to acquire a condition of disease, and that both can be explained by the fundamental laws of contagion. The small-pox group have now been considered at considerable length, and an endeavour has been made to apply the theory of Pangenesis to the explanation of the principal phenomena presented by them. There are many other points in connection with small-pox as yet undetermined, such as the immunity afforded by one attack against a subsequent attack; but since this and the other points alluded to are general questions, it will be better to postpone their consideration till the remaining zymotic diseases have been discussed.

CHAPTER V.

THE ZYMOTIC DISEASES—*Continued.*

In the last chapter, when discussing the small-pox group, the method adopted was to apply in the first place the theory of Pangenesis very minutely to the explanation of the phenomena of cow-pox; but, after evidence was adduced to show that cow-pox and the different varieties of small-pox were essentially the same disease, it was not found necessary to apply the theory of Pangenesis to the explanation of each variety of the disease separately. Cow-pox was compared to the less severe forms of small-pox, and the less severe with the more severe forms: the differences between the various grades in the ascending series were noted, and those differences were explained in conformity with the hypothesis of Pangenesis; but the explanation of cow-pox by that hypothesis was found to require only a slight modification to make it applicable to each grade in the series. A somewhat similar method will be adopted when dealing with the remaining zymotic diseases. An attempt will be made to explain some of the points in which they differ from the small-pox group; but the hypothesis of Pangenesis will not be applied to the explanation of the phenomena of each. My great object is to notice certain points which will strengthen the argument in the previous chapter, and assist us in explaining the peculiar characteristics by which the zymotic are distinguished from other diseases. We shall, in the first place, make a few remarks upon scarlet fever. In small-pox the severity of the fever, other things being equal, varies directly as the copiousness of the eruption, but in scarlet fever this is only true to a very limited extent. During epidemics of scarlet fever cases of sore throat occur without being accompanied by any eruption. The fever in such cases is very mild, but their prevalence at such times appears to indicate that they are produced by the contagium of scarlet fever. In other cases the eruption is so slight that unless carefully looked for it may not be noticed at all; and indeed there can be little doubt that both the fever, sore throat, and eruption are frequently overlooked, and the only indication from which it is inferred that the patient has had scarlet fever is the subsequent attack of anasarca. And as we ascend through the different degrees of scarlatina mitis, and the less severe forms of scarlatina anginosa, the severity of the fever bears a direct proportion to the copiousness of the eruption. But in the severer forms of scarlatina anginosa, and in the dreadful cases of scarlatina maligna, the severity of the disease appears to bear an inverse ratio to the degree of eruption. This is so well known to be the case that in the teleological conceptions of a past age the eruption was regarded as a beneficent arrangement to eliminate the poison from the system. What other interpretation can be put upon these facts? The nutrition of a locality is a moving equilibrium, and an increase of molecular change will produce increase of vital changes; but an excess of that molecular disturbance will overthrow the equilibrium, and arrest all vital changes. A slight increase of the temperature of a part will increase its vital activity, while a higher degree of temperature will destroy all vital activity. In the severer forms of scarlet fever the character of the slight eruption which appears bears out the idea that its arrest is due to excess of action. The eruption is of a dark or livid colour, disappears in a few hours, reappearing again in a few hours if life is so far prolonged. Aphthous elevations appear in the throat, surrounded by a livid base, and on bursting they expose an excoriated, dark, gangrenous surface. Without entering minutely into a consideration of such cases, their whole history points to paralysis and death of tissue as the result of a primary excess of action. Hence the treatment which would add to this excess by applying heat to the surface is not so likely to be successful as that which would partially arrest the action by cold, especially if the cold is applied in the early stage of the disease, and before paralysis or death of tissue has resulted. By the application of cold the action of the gemmules on the tissues would be less violent and more gradual, and the disease would be assisted through its evolution.

Scarlet fever has been propagated by inoculation, but it has not been found that the inoculated variety was less severe than the natural disease. Rostan inoculated a case and the disease appeared on the seventh day, and this is about the average period of the stage of incubation of the natural variety. This fact, so far as it goes, confirms the opinion already given that the prolonged stage of incubation in natural small-pox, and the severity of the disease in comparison with the inoculated variety, are connected together by some fact of causation. But if the inoculated variety of scarlet fever is not milder than the natural disease, we may at some future time succeed in obtaining a modified disease by the process of double grafting. The disease may be grafted upon some of the lower animals, and by transmission through the same species for a few generations a modified form may be obtained, which, when grafted upon man, will produce a disease so mild as to be entirely innocuous, and yet afford a sufficient protection against the natural variety.

Measles does not present any feature which differs remarkably from scarlet fever, except in the character of the eruption, and in the general mildness of the disease, facts which are as inexplicable to us as the different forms of galls produced by closely allied species of cynips. It is, therefore, unnecessary to discuss this disease at present.

Erysipelas is a disease which is generally classified along with the eruptive fevers, but it differs from them in many respects. There is no doubt that this disease can be communicated by inoculation. Dr. Willan says that if a person be inoculated with the fluid contained in the phlyctenæ or vesicles of a genuine erysipelas, a red, painful, diffused swelling and inflammation analogous to erysipelas results.* Some go so far as to maintain that erysipelas is always propagated by inoculation, and that there must be a slight external injury where the contagium finds access to the body; and certainly in the majority of cases which have come under my observation I have succeeded in detecting a slight abrasion of the cuticle. Accepting the innoculability of erysipelas as a central fact, two questions present themselves. Is the disease also propagated by contagion, like small-pox and scarlet fever? and does it in some cases arise as a general disease from disturbances in the ordinary conditions of life? It is probably impossible to give a full and satisfactory answer to the first of these questions in the mean time. There can be no doubt that erysipelas rages at some times in the wards of hospitals; and there

* See Dr. Aitken's "Practice of Medicine," 4th edit., vol. i. p. 361.

can be little doubt that in these cases the disease is propagated from one individual to another; but, as already said, a careful scrutiny reveals the fact that there has been an abrasion on the surface of the body in the majority of those who have received the disease, and a high probability that this may be the case in all. It is also certain that several of the characteristics of the more distinctly contagious diseases are wanting in erysipelas. It does not appear, for instance, that one attack affords any protection against a subsequent attack of the disease. It would be a very interesting point to determine how far the innoculable product of erysipelas is similar in morphological characteristics to the contagium of the eruptive fevers. The morphological characteristics which highly contagious tissues must possess will be mentioned hereafter, and it must be left to subsequent experiment to determine whether the morbid products of erysipelas possess those characters in a high degree. With regard to the second question which the consideration of erysipelas suggested, I think that there can be little doubt that the disease, or at any rate some forms of it, do arise from changes, or a combination of changes in the conditions of life. The contagiousness of inflammatory products have already been noticed, and it seems probable, when from a combination of causes a high degree of inflammation results, that this will lead to a high degree of contagiousness in the inflammatory products which would cause the disease to be propagated from individual to individual with great facility. This consideration introduces us to very interesting questions with regard to the affinities of erysipelas and the classification of disease, and brings prominently before us the more general question whether all the contagious diseases may not arise *de novo*, but it would lead me too far from the subject under consideration to discuss these questions at present.

Before leaving the specially contagious diseases it might be expected that I should make a few remarks upon typhoid fever and cholera; but as it is only intended to discuss general principles here, it is sufficient that I express it as my opinion that these diseases can be brought under the general theory already advanced. Mr. Simon summarises Dr. Budd's opinions with regard to the causation and pathology of typhoid fever, as follows: "That the fever is essentially contagious; that the living human body is the soil in which the specific poison breeds and multiplies; that all the emanations from the sick are infectious; that by far the most virulent part of the specific poison by which the contagion takes effect is cast off by the diseased intestine of the fever-patient; that the characteristic affection of the bowel in the disease is, in reality, the specific eruption of a contagious fever; that the sewers and other places into which this virus passes are the principal channels through which this fever is propagated; that they propagate it solely in consequence of being the channels for the diffusion of the poison; that it is no more the offspring of common sewage than mildew is the actual offspring of damp and decay; and that 'by placing two ounces of caustic solution of chloride of zinc in the night-stool on each occasion before it is used by the fever-patient, the intestinal discharges may be entirely deprived of their contagious powers.' To anticipate some arguments which might be urged against parts of this doctrine, Dr. Budd observes that typhoid fever scarcely ever reattacks a person who has once suffered from it; and that, like malignant cholera, dysentery, yellow fever, and others that might be named, this is one of the great group of diseases which infect the ground."* In these views I entirely concur; but instead of adopting a pathological *fluidism* for their interpretation, I should, of course, as in the foregoing pages, have recourse to *solidism.*

I shall now make a few remarks upon syphilis, a disease which differs widely from the contagious fevers; but it is interesting to observe that there are several points of community between them. The chief characteristic of the primary infecting sore is that, after the pustule bursts, an ulcer is left which, instead of healing, becomes indurated. In about four weeks the lymphatics in the vicinity become swollen and indurated, and in about other

* "Third Report of the Medical Officer of the Privy Council" (1860), p. 2.

four weeks the eruption appears upon the skin, and this is soon followed by the affection of the fauces. Here, then, we see a certain degree of resemblance between syphilis and inoculated small-pox; more especially in the succession of tissues which become affected. In both the local lesion of the epidermis is followed after a definite period by swelling of the neighbouring lymphatics, which is succeeded after the lapse of a similar period by a general eruption; which in both instances is followed or accompanied by the affection of the fauces; and if the primary lesion is excluded from the comparison the succession of tissues affected in natural small-pox, scarlet fever, and measles, is also very similar. But the most interesting point is that the further progress of syphilis, and what are called the sequelæ of the contagious fevers just mentioned, exhibit a considerable degree of similarity in the tissues affected. In the further progress of syphilis inflammation of the dense structures of the body predominates; the bones, tendons, periosteum, serous membranes, and the cellular tissue; in short, the white tissues of the body are liable to become affected. The conditions which exist during the eruptive stage of small-pox are so involved that it is very difficult to disentangle the sequences of events. The occurrence of phlebitis, ichorhæmia, and swelling of the glands of the groin and axillæ as sequels to small-pox, are probably caused by the extensive suppuration; hence these ought to be eliminated before comparing this disease with others where there has been either no suppuration, or only to a very limited extent. One of the worst complications of small-pox, however, is pleurisy, an inflammation of one of the white tissues which are so frequently affected in syphilis. But the tendency of the white tissues of the body to become affected is much more apparent after scarlet fever. The patient is liable to rheumatism, a disease which mainly falls upon the dense structures of the body; and even the kidney disease, so frequent after scarlet fever, may have some relation of cause or of effect to the rheumatism, or both may be effects of a deeper cause. The tendency of the white tissues to become affected is not so marked in measles; but in tubercle and cancer, diseases which have in some particulars an affinity with the contagious fevers, it is well ascertained that after the lymphatic glands, the lungs and the liver have become diseased, the dense structures of the body frequently become affected. This is a subject which has not attracted so much attention as it deserves; since by a careful scrutiny of the successions of tissues which are liable to become affected during the progress of disease, much light may be thrown upon the correlations of growth, and our powers of prevision in anticipating disease might be greatly increased.

Another fundamental agreement between syphilis and the contagious fevers, and even other diseases, is that, as already noticed, the primary affection impresses its own characteristics upon all the subsequent changes which take place. This is especially apparent in the case of the inoculable diseases, but it is not the less true of the rest. We have already noticed that the cycle of changes which constitute small-pox, not only observe a definite regularity in the tissues successively affected, but also a definite regularity in the time during which these tissue changes take place. In small-pox, what may be called the chronological unit of the disease was found to be a period of five days. In the inoculated variety the time which intervenes between the insertion of the virus and the appearance of the general eruption is a period of twice five days; while in the natural small-pox, the time which intervenes between exposure to the contagion and the appearance of the general eruption is three times five days. What I wish particularly to bring into prominence at present is, that there is a certain more or less constant relation in time between the stage of incubation and the subsequent evolution of the disease. In scarlet fever and measles this relation of the stage of incubation, and what I have called the chronological unit of the disease, may be traced; while, with regard to typhoid fever, Dr. Aitken says, that "in the majority of cases, severe as well as mild, a peculiar periodicity of weeks and half weeks cannot be mistaken. Each week shows a distinct character, which cannot be overlooked in a graphic representation. On the first and last days of each week, changes generally take place, which are either temporary changes, or continue till the fever subsides."* Taking the half week as the chronological unit, we shall notice the relation which exists between it and the stage of incubation, the duration of which, Dr. Budd believes to be seven, ten, or fourteen days, each number being a multiple of the half week. These facts give a further significance to the critical days so carefully observed and noted by Hippocrates. But, although this relation between the initial and subsequent stages of the disease is more manifest in the case of contagious fevers, it is also apparent, though greatly obscured, in other diseases. In syphilis, for instance, the chronological unit is about a month, at the end of which the neighbouring glands become affected, and about the end of two months the first affection of the skin and fauces appears. After a protracted course these affections may heal, and it may be that no other lesion appears for a long time subsequently. But when discussing the small-pox group, we have been obliged to believe that changes in the tissues may take place even when there is no outward lesion, and that this is the case in syphilis is obvious from the fact that the secretions of an individual may communicate the disease, even when there is no active eruption; and when the disease after a long time reappears it is in a different form, and generally in a different tissue, showing that there were

* "The Science and Practice of Medicine," vol. i. p. 390.

changes silently at work connecting the latter with the primary manifestations of the disease. It is evident, however, from the protracted course of this disease, and from its being sometimes apparently, though not really, dormant during long periods, that the relation which subsists between the initial and subsequent stages of the disease cannot be clearly traced. This relation is also difficult to trace in tubercle and cancer, but even in those diseases glimpses of the law may be obtained.

Having now noticed one or two agreements between syphilis and the contagious fevers, we must proceed to dwell upon a few of the more remarkable points in which they differ from each other. It has already been noticed that one of the distinguishing peculiarities of the contagious fevers is, that one attack affords either a partial or a total immunity from a subsequent attack; and it has been seen that, in the case of small-pox, a modified disease has been produced by the process of double grafting which, when implanted upon the body, produces a very mild variety, but one which affords a practically good protection against the severer form of the disease. The reason given for the immunity afforded by one against a subsequent attack was that, in the body, there is a definite tract of tissue which has, either immediately or mediately, an affinity for the contagious matter, and when this tract has once undergone the motion which constitutes the disease, it is no longer capable of being acted upon by the same cause. The tracts of tissue which have an affinity for the gemmules of the specific fevers undergo a revolution within a definite period of a few weeks, at the end of which they return to a condition nearly, but not entirely, similar to that in which they were before the commencement of the revolution. But the time occupied by the cycle of changes which constitutes syphilis, is very different. It has more than once been pointed out that chronic induration, with little tendency to heal, is the distinguishing characteristic of the primary syphilitic sore; that it impresses this characteristic of chronic induration upon the nearest lymphatic glands, and that this slow and sluggish movement is traceable throughout the entire progress of the disease. From this it arises, that instead of the disease running its course in a definite number of days, like small-pox, it may, and without interference generally does, linger on for years, or even last during the life of the individual. But if it were possible to get what I may call the syphilitic tract of tissue through a revolution, as occurs in the contagious diseases, a complete immunity would be given to the individual against a subsequent attack of the disease. And this has actually been accomplished. "Galileo set a heavy pendulum in motion by the well-timed puffs of his breath," and it is by a more or less similar "accumulation of small impulses" that the syphilitic tract of tissue has been made to undergo its revolution. The method adopted is to inoculate repeatedly with matter from a primary sore, and it has been found that after a time the system becomes insusceptible of further inoculation; and stranger still, that the secondary and tertiary lesions become cured. These results are brought about by successive crops of syphilitic gemmules uniting with the cells of the affected tissues, till finally the whole tract of tissue which has an affinity for these gemmules passes through a revolution, and is no longer capable of acquiring the motion which constitutes the disease.

Another point in which syphilis and the contagious fevers differ from one another is that the former may be transmitted to posterity; but in this respect the difference is more apparent than real, and in all probability depends upon the evolution of syphilis proceeding during the whole subsequent lifetime of the individual, or during a large portion of it, while that of the contagious fevers is completed within a certain number of days. Hence there is a much greater probability that the genesis and development of the new individual will proceed during the course of syphilis in either parent than during the progress of one of the contagious diseases; and this probability is rendered still greater by various other circumstances which need not be dwelt upon here. But the transmission of syphilis to posterity does not appear to depend upon inheritance, since the most fundamental law of hereditary transmission is, that "at whatever period of life a peculiarity first appears it tends to appear in the offspring at a corresponding age, though sometimes earlier." * A great many diseases exemplify this law, but in the case of syphilis the fœtus becomes affected, although it was in adult age that the disease was contracted by the parent. This, therefore, is only a case of the direct transmission of disease from one individual to another by contact. And there are well-ascertained facts which prove that the fœtus may become affected by a contagious fever during the course of the disease in the parent. Sir Thomas Watson adduces several instances to show that small-pox can, through the mother, be communicated to the fœtus in utero.* There are, however, no instances to show that the disease could be propagated to the embryo through the fertilizing element of the male; but this could hardly be expected, since the action of small-pox is so rapid that if any effect were produced upon the germ at that early period it would either die, or if it lived, no traces of the disease would be apparent at birth. Instances might be adduced of cases where the mother was vaccinated during pregnancy, and when the child was afterwards vaccinated the vesicles which resulted were imperfect. The latter cases are equivocal; but enough has been said to show that the transmission of syphilis to posterity is more analogous to the phenomena of direct contagion than to those of hereditary transmission.

* On the "Origin of Species," by C. Darwin (1859), p. 13.

† Watson on the "Principles and Practice of Physic," 4th edition, vol. ii. p. 863.

It has been assumed that the particles which are found in infective liquids, and which are the active elements in causing the disease, are anatomical units modified and individualized by the morbid process, and not independent organisms. It has also been shown that when a morbid product varies considerably from the tissues of the body it acquires contagious properties; and since the infective liquids have varied to a great degree from healthy tissues, it may be concluded that they are highly contagious. The more remarkable phenomena of the zymotic diseases have now been deduced from this fundamental assumption without our being obliged to have recourse to any data other than those necessarily employed in interpreting the phenomena of the general diseases. So far, then, our primary assumption is verified by the deductive interpretation of the main phenomena of the diseases under discussion. It has been assumed hitherto that pathological products having certain morphological characteristics are highly contagious; but let us now change our point of view, and endeavour to ascertain what must be the morphological characteristics of the pathological products of the contagious diseases. If the results obtained under the second aspect are in accordance with those obtained under the first aspect of the question, it will be a striking confirmation of the truth of our fundamental assumption, and of the whole of our argumentation.

If the contagious diseases have originated by successive modifications of the pathological products of the general diseases, the means by which the former are propagated must have been acquired by natural causes; and it may be possible to trace certain laws within the limits of which those causes must have operated. Let us endeavour to trace what must be the consequences when a pathological product like pus varies further and further from the normal structure of the body as the result of a greater and greater complication of causes. A slight irritation of a mucous membrane will cause inflammation, and in a few days pus results. This product is very similar to some of the structures of the healthy body. Its corpuscles cannot be distinguished from the white blood corpuscles, nor from the cells of the mucous membrane, from which in this instance it is supposed to originate, except that the growth of the pus-corpuscles is more rapid and more discontinuous; and in these respects it is, as Virchow remarks, more like embryonic tissue. But suppose that, owing to an unhealthy condition of body, such as starvation combined with local irritation, a further deviation from healthy structures is produced in the pus, what would happen? In the first instance the pus is produced in a healthy well-fed body by common irritation, in the second by the same means in a starved body; and since in a low state of human society a starved body would be probably as common as a healthy body, the second grade of pus would be produced as readily and as frequently as the first. But let us suppose a further complication of causes, such as a starved body, common irritation, and unusual atmospheric influences, a third grade of pus would result, and this grade could only be produced when these or other equally complicated conditions came together. Therefore this grade of pus had a less chance of production than either of the former; and for every additional complication of causes and consequent deviation of the pathological product, the appearance of the latter would become less and less frequent, until after a few terms its chances of production would practically vanish. But we have not to do with diseases which have appeared once or twice and then disappeared, but with diseases which have been continued through generations: hence for every increment of deviation of a morbid product from healthy structure some compensating change must take place, which will enable the disease to propagate itself, otherwise it must die out. The power by which the disease is produced is rapidly diminishing in one direction, and there must be a corresponding gain in some other direction; and since from the supposition this compensating gain cannot be from changes or combination of changes in the organism, or in the environment, it can only be attained by the pathological products of the disease acquiring properties which will enable them to propagate the disease to other individuals. The transmission of the disease to other individuals may be insured either by inheritance or by contagion: in the one case such a modification of the individual is produced as will tend to produce the disease in the progeny at the same age, while in the other the disease is directly propagated from one individual to another. It will, however, be shown hereafter that the modification of the individual which ensures the propagation of disease by inheritance is produced by the acquisition of contagious properties by the morbid product, which causes more or less permanent change of all the correlated parts of the organism. And since the properties of the morbid product, which will insure the transmission of the disease by inheritance, are reducible to the fundamental laws of contagion, it will be better to defer the consideration of this mode of propagation at present lest the argument should become too much involved. With this qualification no error will arise if we say that the compensating changes which take place in pathological tissues, which deviate greatly from normal structures, are obtained by the pathological products acquiring contagious properties. It has already been pointed out that before a structure can acquire contagious properties, the fertilizing element, along with a general likeness, must vary to a certain degree from the element which it fertilizes; and now we have come upon the complement of this proposition, namely, that a structure which deviates greatly from normal tissues must, if it is to be reproduced, except at rare intervals, acquire contagious properties. Having now shown this double relation which exists between great variation of pathological tissues from healthy structure, and the contagious properties of the former, we are in a position to proceed to inquire what must be the morphological characteristics of the pathological products in all highly contagious diseases. As has already been said, the experiments of Mr. Darwin upon dimorphic and trimorphic plants render it apparent that there is an element in fertilization which causes great differences in the effect, and yet which cannot be connected with appreciable differences in the form of either the sperm or the germ cell. This difference of effect produced was referred to differences of molecular motion in the fertilizing agent. Judging from analogy, therefore, the diseases produced by infective tissue may differ very widely both in degree and in kind without our being able to detect any morphological characteristic by which one tissue can be distinguished from another, and as in the case of the fertilization of plants, these differences must be referred to slight variations in the molecular motion of the units. These remarks have been made to show that there is an unresolvable element in contagion which our means of observation do not enable us to connect with definite changes in the form of the units of the contagious tissues, so that morphological distinctions cannot be made to correspond exactly to degrees of contagiousness, and are only valuable when we come to compare tissues which are highly contagious, with those which are only slightly so. Whatever be taken as the test of contagiousness, whether the rapidity and extent of the changes produced in the primary individual upon which the disease is grafted, or the facility with which it may be communicated from that individual to a great many subsequent individuals, it may be accepted as a central fact that there must be a high rate of multiplication of the units of the pathological product. A reference to a concrete instance like small-pox will render it apparent that such a rapid multiplication really does take place; but since we are only dealing here with *à priori* considerations, this evidence is not available at present. It has already been seen that the rate and degree of the development of the embryo depend, to some extent, upon the number of sperm-cells which come in contact with the germ-cell. If, then, the rapidity and extent of the changes be taken as the test of contagiousness, it is evident that, according to the theory of Pangenesis, an innumerable number of gemmules must be rapidly produced so as thoroughly to fertilize a large tract of tissue in a short period of time; and if the facility with which the disease is propagated to other individuals be taken as the test, it is evident that the larger the number of units cast off by the diseased body the more likely is it that the disease will be propagated to other individuals. But a high rate in the multiplication of units involves many concomitant changes. In the first place it involves great rapidity of growth, and this, as we have already seen, necessitates gradual thinning and final disappearance of the cell wall. But when the multiplication of units is very rapid, the production of new units must occur at short intervals; hence the size attained by each must be small. Therefore rapid growth, disappearance of the cell wall, along with diminished bulk, must proceed along with rapid multiplication of the units. But this is not enough; if the units of the pathological product aggregate together so as to form a continuous growth there may be no propagation of the disease; hence the growth of the pathological products in contagious diseases must be discontinuous, and indeed it will follow from the disappearance of the cell wall that the growth must be discontinuous. The morphological characteristics of the pathological tissues of the highly contagious diseases may, therefore, be briefly summed up as rapid and discontinuous growth, small size of units, along with disappearance of the cell wall.

These conclusions are amply verified by observation and experiment. In small-pox, for instance, a short time only elapses between the insertion of the graft and the appearance of the primary vesicle; in a few days more the effects begin to show themselves in other parts of the body, especially in the lymphatics, and after another short interval of time this is followed by the general eruption, while in a few days more myriads of particles are cast from the body, each one of which is probably sufficient to initiate a similar disease in a healthy individual. This shows great rapidity of growth with a high rate of multiplication. The vesicle itself is soft, its tissues have much less cohesion than the surrounding tissues, and it does not form into a permanent growth, but soon disperses; in other words the growth is discontinuous. As already mentioned, the experiments of Professor Chauveau and of Dr. Sanderson have shown that the active part of the pathological products are minute particles less than the $\frac{1}{200000}$th of an inch in diameter, and it is almost unnecessary to add that these particles have no recognisable cell wall. The pathological product of small-pox, then, fulfils the conditions which deductive reasoning has shown it must possess to render it a highly contagious disease. When these tests are applied in the case of syphilis, which is not a highly contagious disease, and differs in other respects from small-pox, the results obtained are rather curious. The whole course of syphilis indicates a slow rate of growth; chronic induration appears to be the great characteristic of all the lesions which appear during its progress. This induration is caused by the growth of a tissue which is more or less similar to the surrounding structures; and this product has greater instead of less cohesion than the normal tissues. The aggregation of tissue at each diseased point is greater than in the contagious diseases, at any rate it does not disperse so soon; in other words the growth is more continuous, although by no means continuous in a remarkable degree. It has not yet been determined experimentally, so far as I know, whether the inoculable units of syphilis are without cell wall and as small as the units of the contagious diseases, and until experiment shall decide, it is hazardous to offer an opinion; but judging from the principles laid down here, the syphilitic inoculable particles will be found to be larger than contagium particles, and probably to have a recognisable cell wall. But the molecular motion—the unresolved element of contagious matter—vitiates any conclusions which may be formed from *à priori* considerations, unless these are verified by reference to observed facts. But however this point may be determined, the similarity of the pathological product of syphilis to the tissue in which it is formed, the slow rate of growth, and the degree of continuousness it possesses, are sufficient to account for the non-contagiousness of the disease. As already mentioned, syphilis can be sent through the different stages of its progress more quickly by the method of repeated inoculations, and the individual has a complete immunity from a subsequent attack of the disease. This fact, along with the considerations just advanced, seems to indicate that, *cæteris paribus*, rapid growth of the morbid product, high contagiousness, and immunity from subsequent attacks of the disease, are bound together by a causal connection, so that the one varies directly as the other. It may also be added that the dependence of contagiousness upon rapid growth will tend to confine the highly contagious diseases to young persons when growth is most active. But there is another factor which will tend to confine the highly contagious diseases to early age. The immunity afforded by one attack of a contagious disease to a subsequent attack shows that a modification is produced which is more or less permanent during the lifetime of the individual, and it becomes a question whether such a modification may not be, to a certain extent, inherited. This subject will be more fully treated hereafter; but in the mean time we may notice that if this modification is inherited to any extent at the same period of life in which it was acquired by the parent, this will also tend to confine the action of the contagious diseases to an early period of life. The inheritance of the modification of the tissues of an individual which gives an immunity from an attack of a contagious disease, may account for the well-known fact that when the inhabitants of the country emigrate into large towns they are more liable to contagious diseases, and generally suffer from a severer form than the resident inhabitants, even when the latter have not had a previous attack of the same disease. This, however, may be a case of increased fertility from slight changes in the conditions of life which is so familiar to the agriculturist.* There are several questions in connection with the relations which the contagious and other diseases bear to each other which must be deferred until after the consideration of the constitutional diseases.

* This passage, relative to the inheritance of the immunity afforded by one attack of a contagious disease from a subsequent attack, was the last I wrote on the night of the 13th October, 1871. On the following morning I was pleased to find the same idea beautifully expressed by Dr. T. Clifford Allbutt, in his able introductory address at the opening of the Winter Session (1871-72) of the Leeds Royal School of Medicine. "Expose one," says he, "who has had small-pox to the same infection, and no, or but little, reaction is seen. Why? Probably because his tissues are converted into some isomeric form so like the infector that no further reaction can take place between the two; and this new molecular constitution, though it tends, as probably being less stable, to revert to its former position in long periods of time, yet is for the while so profoundly established that it is unquestionably propagated from parent to child as a transmitted protection. Take small-pox into a virgin population, and its ravages are like the plague; so, in like manner, we observe at home that the longer the interval between the epidemics of infectious fevers, the more terrible the reaction to these infectors when introduced, the reason being that there are fewer persons who have obtained or have inherited protection." The perusal of this passage has suggested to me that, owing to my isolated position as a country practitioner, I may be enunciating propositions in these pages as original which may in reality have been the common property of the profession for years, and that I may frequently appear to borrow ideas from others without due acknowment. If such should be the case, I hope it will be assigned to its true cause, ignorance—excusable, probably inevitable, in my position—and not to any intentional neglect of the rights of others.

CHAPTER VI.

THE CONSTITUTIONAL DISEASES.

THE INHERITED LOCAL DISEASES.

THE constitutional diseases are those which require a special predisposition on the part of the organism for their production. It will be seen hereafter that considerable latitude must be permitted in the application of this definition, but it may be adhered to in the mean time without leading to any important error. This constitutional predisposition may declare itself in several ways. Some cases of disease may arise as the result of "spontaneous" variation, or some other obscure causes in the parent, and be transmitted to the offspring. Such cases may be called *inherited congenital diseases.* Other diseases may be acquired by structural alterations produced by the action of the environment during the lifetime of the individual being bequeathed to descendants. Since structure is the correlate of function, such diseases might be called inherited functional diseases; but this name is objectionable, since in the ultimate analysis all the constitutional diseases are caused indirectly, if not directly, by adaptive changes of function. But in diseases which are produced in an individual by the direct action of the environment, the local alteration of structure is the most important point to attend to, and must be the starting point of our inquiry. Hence when such diseases are transmitted to posterity they may be called *inherited local diseases*. But there is another set of cases, in which the disease is only developed as a concomitant of a certain modification of the organism which is not a disease in itself. In such cases it is of importance to attend to the modification of the organism which gives the predisposition to the disease; and as such constitutional modifications have been called *diatheses*, the diseases themselves may be called *inherited diathetic diseases.*

Before proceeding further, I must remind the reader that my main object is to show that the principles which have been developed while treating of the general, and applied to the solution of the zymotic, are equally applicable to the interpretation of the constitutional diseases. The wider and the more seemingly unlike the number of facts which can be interpreted by an hypothesis, the more trustworthy does the hypothesis itself become; and my desire is to secure this trustworthiness to the Graft Theory of Disease in its application to the zymotic diseases. This being the case, it is not necessary for me to treat of the constitutional diseases exhaustively. It will be sufficient for my purpose to select a good specimen of the local and of the diathetic inherited diseases, and show that the principles advanced here are applicable to the interpretation of the main phenomena presented by them. The inherited congenital diseases may be passed over here, since they present no features which would throw additional light on the principles advanced. With these remarks I shall proceed to the consideration of the inherited local diseases, and for various reasons shall select cancer and its allies as a good specimen of this order of disease. The method which will be adopted is that already employed when treating of scarlet and typhoid fevers. The agreements and differences between cancer and the zymotic diseases will be noted, and the differences interpreted, according to the theory of Pangenesis. An endeavour will afterwards be made to trace the laws which must govern the genesis of the disease, and to deduce from the principles already laid down the morphological characteristics of the various forms or rather degrees of cancer.

It need scarcely be remarked that cancer is not a contagious disease; but it is an interesting subject to inquire how far it may be capable of being propagated by inoculation. From the dangerous nature of the malady there is no reliable evidence to show that cancer can be propagated from one man to another by inoculation. The disease has, however, been communicated to dogs. "There are cases," says

Paget, "in which, by the inoculation of cancerous material into the bodies, or by the injection of such material into the blood of dogs, cancer has seemed to be produced. I think that, in a large number of experiments, that result has been three times obtained; but it is quite possible that the dogs used for these three experiments were cancerous before the human cancerous matter was injected into them; for cancer is indeed a frequent disease among dogs. The instances are certainly too few for proof of inoculation."* It may be presumed that this disease is not readily communicable by inoculation, otherwise it would be more frequently met with in the hands of nurses accustomed to dress cancerous ulcers; and if it is not propagated by inoculation it is still less so by contagion. But if cancer is not propagated by inoculation nor by contagion, it must be caused by changes in the environment or by some transmitted peculiarity in the organism. The further consideration of its origin, however, will be deferred at present. But if there is no evidence that the disease is propagated from one individual to another by inoculation, there is abundant evidence to show that it is anto-inoculable. Cancer in a pendulous breast has been known after ulceration to communicate the disease to the neighbouring skin in such a manner that a healthy piece of skin has intervened between the primary and secondary seat of

* Paget's "Surgical Pathology," edited by Turner (1863), p. 776.

the disease. This shows that the disease was not propagated by continuity of tissue. The fact that cancer is not propagated from one healthy individual to another by contagion and inoculation, while at the same time it is probable that it may be propagated by inoculation in those who have a predisposition to the disease, and that it may be propagated in this manner from one part of the body to another of one suffering from the disease, will be hereafter explained deductively; but this must be delayed till we are in a position to compare the morphological characteristics of the cancerous with that of the pathological products of the highly contagious diseases.

But if cancer is not communicated from one individual to another by contagion, the phenomena of its propagation within the body by contagion are well marked, so much so, that the infectiousness of cancer within the body is made the basis for practically defining the disease for clinical purposes. "We may say that carcinomata are very infectious tumours," says Billroth, "and that this infection, which first attacks the lymphatic glands, afterwards more distant organs, is probably due to the passage of elements from the tumour (whether of cells or juice is not yet known) through the lymphatic vessels and veins into the blood." * The first appearance of the disease is a single tumour in some part of the

* "Surgical Pathology and Therapeutics," by Dr. Theodore Billroth, p. 626.

body; but it has always this peculiarity, that the primary tumour forms in some tissue which has descended from either the external or internal epithelial layer, and never from the middle germinal layer of the embryo. So far, then, it is similar to the primary vesicle of inoculated small-pox. The primary tumour begins now to grow, and to encroach upon the surrounding tissues, while occasionally several secondary tumours develope around it, exhibiting in a marked degree the phenomena of contagion by direct contact, similar to the areola of the primary vesicle of small-pox. Soon afterwards the lymphatic glands become affected, and at no distant date cancerous deposits will be found in those internal organs which are in the direct line of the venous circulation, such as the lungs and liver. We are already perfectly familiar with such phenomena as these—they merely require mentioning; and after what has been said, they do not call for special interpretation at our hands. But there is one remarkable circumstance in connection with the spread and progress of cancer to which special attention must be drawn. It is the fact that a large part of the osseous system is apt to be invaded by secondary, or rather by tertiary inoculation, since we have seen that the secondary affections generally occur in the lymphatic glands, lungs, and liver. This is a fact parallel to those already mentioned, when it was noticed that the sequelæ of the eruptive fevers and the tertiary affections of syphilis fell mainly upon the white tissues of the body. We see, then, that the tissues which become successively affected in cancer are not very different from those implicated during the progress of the zymotic diseases. This fundamental agreement between the tissues successively affected in the zymotic and cancerous diseases throws light upon the phenomena of recurrence of the latter after the primary tumour is extirpated. A series of very able papers on "The Origin of Cancer" appeared in the *Lancet* in July, 1871, by Mr. Campbell de Morgan, in which he mentions the fact that "cancer rarely returns in an organ corresponding to its original seat, or indeed in any organ which is the usual seat of the primary disease."* What would be the use of resorting to excision of the primary vesicle of small-pox after the eighth day? It is probable that the lymphatic glands would be affected even if the vesicle were excised as early as the fifth day. But in syphilis, whose progress is much slower than that of small-pox, eminent surgeons are of opinion that if the primary sore is burnt by the free use of the nitrate of silver before the pustule bursts, secondary symptoms are not developed. Judging, therefore, from analogy, it may be inferred that if the cancerous primary tumour were extirpated at a very early date, and more especially if the tumour were not growing very freely, the disease might not recur. But since cancer is, as will be here-

* *Lancet* (July 8th, 1871), p. 41.

after shown more fully, an inherited disease, no great reliance could be placed upon this conclusion from analogy—if not that it is partly verified by experience; for there are a small proportion of cases in which the disease does not return, or not till a long period after the excision of the primary tumour.

When treating of the inoculable diseases it was found that the primary lesion impressed its own characteristics upon all the subsequent changes which took place during the progress of the disease. This finds expression both in the correspondence of kind between the primary and secondary lesion, and in the correspondence between the time of their evolution. With regard to the correspondence in kind between the primary and secondary cancerous affections, it is so obvious that a cancerous primary tumour always gives rise to cancerous lesions of the lymphatic glands and other organs, that the fact need scarcely be mentioned; but the correspondence between the progress of the primary and secondary affections in time may be dwelt upon a little more at length. It may be laid down as a general rule that the more rapid the growth of the primary tumour the sooner do the lymphatics and other organs become affected, and the more rapid will be the whole course of the disease. This rule is not without many and important exceptions; but since my object at present is to trace the operation of this rule inductively, the consideration of the exceptions may be deferred till we are in a better position to interpret them deductively. In some free-growing cancers there is an almost simultaneous development of the disease throughout almost all the tissues of the body, not unlike the outburst of the eruptive stage of small-pox. On the other hand, in epithelioma, which is not very unlike the tissue in which it grows and does not increase so rapidly, there is a corresponding slowness in the appearance and subsequent growth of the secondary affections. And if we examine the non-cancerous growths which occasionally infect other parts of the body, it will be found that a similar relation exists between the primary and secondary affections manifested by a slower rate of growth of both.

When examining the contagious diseases we noticed that a very rapid growth of the pathological product was associated with a high degree of contagiousness, and that this showed itself not only in the facility with which the disease was propagated to other individuals, but in the rapidity with which a large tract of tissue became affected within the body. A comparison of the very free with the less free-growing tumours will show that this rule is exemplified in the case of cancerous diseases. It has just been mentioned that in some free-growing cancers there is an almost simultaneous outburst of cancer in almost all the tissues of the body. But as Mr. De Morgan says, "in epithelioma the neighbouring lymphatics and the parts surrounding them are the chief homes of secondary deposit and growth."* The same rule is also exemplified in the case of the recurring non-cancerous tumours. "In the fibrous or cartilaginous, or other non-cancerous growths," says Mr. De Morgan, "the secondary tumours appear in the line of the absorbents leading to the more central parts, as in the neighbouring lymphatic glands, or those still more removed, or else in the direct line of the venous circulation, as in the lungs or liver. But we rarely, if ever, find recurrent non-cancerous growths in organs out of the direct line—if we may so speak—of infection."†

The cancerous and zymotic diseases have now been compared, and several fundamental agreements have been found to exist between them, more especially with regard to the phenomena of contagion within the body; and even the differences already noticed are only such as were found to exist in a minor degree between the inoculable and the highly contagious diseases. So far, therefore, as we have gone, no exceptional phenomena have been found which require a special application of the hypothesis of Pangenesis for their explanation. It has already been noticed that cancer is not propagated by contagion nor by inoculation; hence it may be expected that the consideration of the origin of this disease will disclose the most fundamental differences between it and the zymotic diseases, and that a true solution of this question will afford to us the best aid in interpreting those differences. To the determina-

* *Lancet* (July 8th, 1871), p. 42.

† *Ibid.*

tion of the origin of cancer let us now address ourselves.

If cancer is not propagated by contagion nor inoculation, can it be the result of ordinary irritation? The callosities which appear on the hands of workmen may be caused by prolonged intermittent pressure upon the hands of any individual; but although irritation of this kind appears to be one of the conditions which determine the appearance of cancer at a particular time and in a particular locality, yet no amount of irritation will develope a cancerous tumour upon the majority of individuals. This of itself is sufficient to show that there must be some other cause concurring with common irritation to originate a cancerous disease. And if the disease does not arise solely from prolonged irritation, it is highly probable that it does not arise from the introduction into the body of any special material from the environment. It is difficult to prove a negative of this kind, but until some positive evidence is adduced that cancer is caused by a deleterious gas or miasm, or chemical substance in solution, or parasitic germs gaining entrance into the body—such agents as a probable cause of the disease may be dismissed from consideration at present. If, then, neither common irritation nor special agents in the environment acting upon an individual can of themselves originate cancer, there must be some inherent tendency in the organism, however acquired, towards the production of the disease. This inherent tendency may be imagined to produce the disease in one of two ways,—either by the transmission from parent to offspring of a modified organization, which under certain conditions of the environment develops cancer as a concomitant, or by the direct transmission of the disease from parents to offspring by heredity, or reversion to the characters of a remote ancestor. Let us now endeavour to determine by which of these two modes the tendency to the disease is acquired by the offspring.

The diseases to which cancer is most closely allied are the great group of tumours or outgrowths. Professor Huxley remarks, that "from such innocent productions as corns and warts, there are all gradations to the serious tumours which, by their mere size and the mechanical obstruction they cause, destroy the organism out of which they are developed; while, finally, in those terrible structures known as cancers the abnormal growth has acquired powers of reproduction and multiplication, and is only morphologically distinguishable from the parasitic worm, the life of which is neither more nor less closely bound up with that of the infected organism."* The various degrees in the ascending series of tumours, from the simple outgrowths to the most rapidly growing cancer, will be specially noticed hereafter; but in the mean time I would remark that since cancer has the greatest affinities

* "Address to the British Association" (Liverpool, 1870), p. 18.

with the group of diseases called tumours, that the key which will unlock the mystery of its origin must be sought by carefully attending to the mode of production of the simplest outgrowths. It is well known that the callosities on the hands of workmen are caused by intermittent pressure, and that corns may be developed by long-continued irritation on the feet of probably all healthy persons. But it has already been seen that no amount of irritation will develop cancer upon some individuals; but it is quite possible that prolonged local irritation acting upon a succession of organisms through several generations, aided by inheritance, is quite competent to cause the disease. If this could be shown to be the case, the origin of the disease would be as local as that of a corn; only that in the case of cancer the disease is produced by irritation acting upon a succession of organisms, while the corn can be produced by irritation acting upon a single individual. It gives me pleasure to be able to quote Mr. De Morgan as sanctioning this opinion. "The general conclusion," says he, "at which I arrive is, that in some persons, and in some parts, there is a tendency, local in its origin, to the formation of tumours. That this tendency may in some have been implanted in the tissue, even in its embryonic condition, though the actual development may not take place till years after birth; in others, although there may be a disposition to morbid growth, the actual tumour will not be developed unless under some irritation."* I cannot, however, agree with the further development of Mr. De Morgan's views when he says: "In all cases I should regard the morbid product as the result of undirected or ill-directed growth force, the elements of the structure thus formed possessing, as the natural elements do, the tendency to reproduce their like."† Undirected the force cannot be if the doctrine of the persistence of force is to be believed, and it is only ill-directed in the sense that the force produces a disease, that is, produces a structure which is undesirable; hence the "ill" is a characteristic of the effect as viewed from the stand-point of art, and to place it as a characteristic of the cause involves a *petitio principii.* If, then, cancer has its origin in a local irritation acting upon a succession of organisms through long generations, and propagated by inheritance, before proceeding further the laws of inheritance must be mentioned.

One of the most distinguishing characteristics of living beings is that they are derived by a process more or less direct from parents of a like kind, and the fundamental law of hereditary transmission is that "at whatever period of life a peculiarity first appears, it tends to appear in the offspring at a corresponding age, though sometimes earlier." ‡ This law is no doubt derivative, but it has not yet been resolved into more general laws. The progress

* *Lancet* (July 29, 1871), p. 155. † *Ibid.*

‡ "On the Origin of Species," by C. Darwin (1869), p. 13.

of investigation, however, may enable us hereafter to deduce it as a necessary consequence from first principles. In the mean time the main evidence for the law must rest upon its wide verification throughout the whole biological series. But it must be remembered that this fundamental law of inheritance is one which is only expressive of a tendency, and it does not follow that all the individual peculiarities should reappear in the offspring. There is no truth more manifest than that every organism is not exactly similar to its parents, but that it deviates from them in several minor peculiarities. This tendency of the offspring to differ from the parents in several smaller details is called variation; and there is abundant evidence to prove that such variations, however acquired, may be transmitted to posterity by inheritance. "Numerous illustrations of heredity," says Mr. Herbert Spencer, "are yielded by experiment, and by direct observation of successive generations. They are divisible into two classes. In the one class come cases where congenital peculiarities, not traceable to any obvious causes, are bequeathed to descendants. In the other class come cases where the peculiarities thus bequeathed are not congenital, but have resulted from changes of functions during the lives of the individuals bequeathing them."* This dichotomous division of all inherited peculiarities into two classes, the

* "Principles of Biology," by Mr. Herbert Spencer (1864), vol. i. p. 241.

congenital and functional, is comprehensive of all cases, whether healthy or diseased; but as I wish to restrict myself to the consideration of the diseased variations, and as I have a practical as well as a theoretical object, the trichotomous division already made into congenital, local, and diathetic, is more suited to my purpose. It is evident, from what has been said, that it is with the functionally acquired peculiarities—that is, with peculiarities which have been acquired by the action of the environment upon one, or upon a succession of organisms—that we have to do at present. Let us restrict our further remarks, therefore, to the inherited local diseases. The most superficial observation will render it evident that some structural changes called forth by functional changes tend to be inherited, while other structural changes exhibit no tendency to reappear in the offspring. For instance, the callosities which are formed on the hands of gentlemen by rowing and other manual exercises have no tendency to be inherited; while it is well known that the large hands of labourers are bequeathed to their children. Is it possible to draw a distinction between those functionally acquired structural changes which are frequently and those which are rarely transmitted? If the instances just mentioned of callosities on the hands of gentlemen and the large hands of manual labourers be contrasted, it will be found that the cause which has produced the deviation in the former instance is transient; while it is continued in the latter through, not only the whole active life of the individual, but through a succession of individuals. It may therefore be laid down as an empirical generalization that structural deviations which result from a transient cause are not inherited, while, on the other hand, the structural changes which are produced by causes acting for long periods of time upon a succession of individuals, are transmitted to posterity. It will be as well before proceeding further to give such a deductive interpretation of these inductive laws as the case admits of. It is implied in what has already been said, that not only is the structure which distinguishes species and varieties transmitted, but that even the structural peculiarities of families tend to be inherited. A great deal of evidence may be brought forward to show that this law of inheritance is in accordance with first principles, although it has not hitherto been found susceptible of deductive explanation; hence the necessity already noticed of basing it upon a wide inductive evidence. If now we look at all the structural peculiarities which are inherited, whether those which characterise the species, the variety, or the family, they have this in common, that they must bear a definite and constant relation to the whole structure of the individual, and more especially to the structure of the more immediately correlated parts of the organism. It is evident that an organ or part of the body cannot increase in size without coincident structural changes taking place in the blood-vessels and nerves leading to it, and that these changes cannot take place without secondary changes also taking place in more remote parts of the organism; and if this enlargement of the organ is to continue permanent in the individual, or in a succession of individuals, so must also the secondary deviations which result from it. The same idea may be expressed in terms of function by saying that the functional activity of a peculiarity which tends to be inherited must be in equilibrium with the functional activities of all the parts of the body, and more especially with that of those parts which are more immediately correlated with it. It is noticeable that a congenital peculiarity always exhibits a strong tendency to be inherited, and the reason is obvious. The congenital deviation is already in equilibrium with the aggregate functions of the body, and its appearance is a result, and not a cause of this equilibrium. On the other hand, functionally acquired peculiarities have to react on the organism at large, so as to bring about the correlative changes necessary to produce a new equilibrium; hence such modifications of structure must act upon the organism for a long time before they are inherited. There are one or two apparent exceptions to this rule, which must now be noticed. Dr. Brown-Séquard has found that epilepsy can be induced in a guinea-pig by section of one-half of the spinal cord, and that the subsequent progeny of the injured animals are subject to epileptic attacks. Here, then, is an instance where the functionally acquired peculiarity is produced by a stroke of the knife, and yet it is found to be inherited. But it must be noticed that the epilepsy is only a secondary result of the primary injury, and that neither the injury itself, nor the paralysis which directly results from it, is inherited. The question at issue, then, will be solved if it can be determined how the injury to the spinal cord can produce such a modification of the organization as continues permanent during the remaining life of the individual. It is not possible to trace the successive steps by which the effect is produced, but general considerations can easily be adduced to show why the effect when once produced should become permanent. The injury has affected an important structure which has a wide and intimate connection with almost every other part of the body, and also a tissue so highly specialized as to be able to affect every part of the body almost simultaneously. The consequence is that the functions and also the structure of such a large portion of the body is so disturbed that the parts which remain more or less healthy are never afterwards able to regain the equilibrium which existed prior to the injury. It may therefore be concluded that the larger the tract of tissue which becomes affected by the primary deviation, the more likely is a permanent effect to be produced which becomes inherited.

This appears a fitting opportunity for explaining the permanence of the modification produced by one attack of a contagious disease, and its appearance in the offspring as an inherited protection. The products of the contagious diseases have acquired properties which enable them to be rapidly diffused throughout the entire organism, and to produce structural changes in a short time in almost all the tissues of the body. If these structural deviations are so large, and involve such a wide area, that they outbalance the tissues which remain more or less healthy, the equilibrium is upset, and death results. If, on the other hand, the tissues which remain more or less healthy preponderate (an exact balance between the two is for various reasons an inadmissible supposition), there is a partial return to health; but unless the healthy tissues very largely preponderate the equilibrium which existed prior to the disease can never be attained, and the new equilibrium which is formed being permanent in the individual may be transmitted to posterity. From what has been said, then, it may be concluded that when a structural deviation is small, but, owing to the continuance of the incident forces which gave rise to it, more or less permanent during the life of the individual, the greater the chance that the entire organism should adapt itself to such a modification, and that it should become inherited. While, on the other hand, the greater the change of structure, and the more transient the cause which has produced it, the greater the probability (unless the equilibrium of the organism is overthrown) that such a change should become readapted to the rest of the organism, and that it should fail to become inherited. This statement must, however, be qualified to a certain extent, since, as has just been noticed, a large deviation of structure may produce minor deviations over a wide tract of tissue; and although the primary major structural deviation is not transmitted, the secondary minor deviations are frequently inherited.

But it must be noticed that even when a structural change is permanent during the life of the individual it is lost in a few generations, unless the original cause continues to act upon a succession of individuals. A species, for instance, which has by domestication for a long series of years acquired many structural peculiarities, loses a great many of these when it becomes feral. The large hands of agricultural labourers are lost in a few generations when they become operatives in cotton factories. The fact that a functionally acquired peculiarity disappears in a few generations, unless the cause which has originally produced it continues to act on the organism, shows that the local diseases which exhibit a strong tendency to inheritance must possess the characteristic of being permanent during the life of the individual. There are many diseases which were originally undoubtedly functionally acquired, and which are inherited, but do not appear to conform to this rule. The functionally acquired epilepsy of guinea-pigs, induced by section of the spinal cord, has already been noticed, but the epileptic attack only appears at occasional intervals. It is not the epileptic attack, however, which is inherited, but a certain modification of the organism which develops an attack under certain conditions, and it is this modification which is inherited, and it is permanent during the life of the individual. A similar remark may be made with regard to those tumours which, when once they appear, continue during the subsequent life of the organism. Such tumours are liable to attacks of disease, as inflammation for instance. The inflammation of the tumour may subside of itself, or be amenable to treatment as in any other tissue of the body; but this secondary disease is, in the language of the old logicians, only an accident, and not an essential part of the primary disease, and it is the latter alone which is inherited.

Having now made these few remarks on the phenomena of inheritance, it might be expected that I should now proceed to interpret them by the hypothesis of Pangenesis. Mr. Darwin has, however, applied his own hypothesis with such wonderful skill to the explanation of the phenomena of genesis, inheritance, reversion, and the other great operations which take place in living beings, that it would be presumptuous on my part to deal with this part of the subject. All that need be said here is, that when a part becomes structurally changed, the gemmules cast off from its cells act upon all the correlated parts of the organism, each gemmule *tending* to produce its like, and acting upon the correlated parts of the body in such a manner as to adapt them to the new deviation in structure. On the other hand gemmules from the correlated parts of the organism are constantly acting upon the structural deviation, and they tend to readapt it to its former equilibrium. If the deviation only affects a small portion of tissue, the gemmules cast off (unless they acquire new powers, and it will hereafter be seen what those powers are) can only maintain the equilibrium of the new deviation against the gemmules of the correlated parts of the organism so long as the cause which first produced the structural change continues to act. When, again, the structural change affects a large tract of tissue, say half the tissues of the body, it is evident the gemmules cast off such a large portion of tissue will have a more powerful action upon the correlated portions of the organism, while the gemmules from the correlated portions of the organism will have a much less powerful effect upon it than in the former instance; and a new balance may be almost immediately struck which continues permanent during the remaining life of the individual, and probably for a succession of generations, even when the cause of the primary structural change has only acted for a short time. An endeavour will now be made to show a method by which cancer might have originated from minor structural changes by small increments of change deviating further and further from the healthy tissues, and acting, not upon one, but upon a succession of organisms.

It is the opinion of Virchow that the physiological type of cancer is epithelium, and in accordance with this view Billroth says, "I only call those tumours true carcinomata which have a formation similar to that of true epithelial glands (not the lymphatic glands), and whose cells are mostly actual derivatives from true epithelium."* This is the view which will be adopted here; and it is obvious, therefore, that in tracing the origin of cancer our starting point must be from the minor structural changes which occur in a tissue which has descended from one of the epithelial layers of the embryo. It may, however, be remarked in passing, that if future researches should show that Virchow's opinion is incorrect, the general principles advocated here would remain unaffected, although it would necessitate a considerable alteration of expression in the working out of details.

There are three minor structural changes of the skin which are closely allied, and yet present slight divergences which it is important to notice—these are callosities, corns, and warts. The first and second of these may be produced on the healthy

* "Surgical Pathology and Therapeutics," by Dr. Theodore Billroth (1871), p. 627.

skin by prolonged irritation applied in a particular manner in each, while it is probable that the third might be cultivated upon a healthy skin; but this is more doubtful, at any rate the production of the latter would require a greater complexity of causes, and the complexity of general causes necessary to the production of every further deviation from the normal structures must rapidly increase, while at the same time the chances of the deviation being produced by those means must rapidly diminish. If, therefore, a structure which has deviated greatly from the normal tissues of the body is frequently produced, it must have acquired properties by which its own propagation is insured. From what has already been said it is obvious that the structural changes now under consideration are not propagated by contagion nor by inoculation; and in short that the properties acquired must be such as to bring about such an adaptation of all the correlated parts of the organism as will insure its propagation by inheritance. And if the phenomena presented by callosities, corns, and warts are closely noted, it will be found that the last, whose deviation from normal structures is slightly greater than that of the others, is more intimately related than they are to some modification of the organism. As frequently noticed a callosity soon disappears after the irritation which has caused it is withdrawn, and the same is true of a corn, although probably it does not disappear quite so fast; but the continuance of the wart is much more independent of the original source of irritation. Warts sometimes disappear suddenly without any very obvious cause, but it is undoubtedly true that they appear to be much more independent of an external irritation in their modes of appearance and disappearance than either callosities or corns. This clearly shows that the former is more intimately related to a certain adapted modification of the organism than the latter, however that modification may have been produced; and this is rendered still more manifest when it is considered that neither callosities nor corns appear to be inherited, while there is strong evidence to show that warts are more frequently found in some families than in others. Here, then, we find the first faint beginnings of that condition of the organism which insures the reappearance of a disease by inheritance; while for the structural changes, which deviate still further from healthy tissues, the inductive evidence for transmission by inheritance is still stronger.

But every further deviation from healthy tissues must be accompanied by morphological changes by which the growths which can be cultivated upon a healthy individual with difficulty can be distinguished from those which are readily producible by changes in the incident forces. The first morphological deviation which may be expected to take place is that the units of the new growth will begin to present differences in their mode of arrangement from those of the surrounding tissues. It is manifest that slight differences in the mode of arrangement of the units present a much less deviation from what occurs in health than if the units of the new structural change were wholly unlike those of the tissue in which it grows; and therefore the minor deviations will be accompanied by minor changes in the disposition of the units. We are endeavouring to trace at present how functionally acquired peculiarities may acquire properties which will enable them to leave their impress upon the organization in such a manner as to become inherited; and as structure is the correlative of function, it follows that the minor deviations of the latter will be accompanied by minor deviations of the former. This is undoubtedly true with regard to the minor epithelial growths already noticed; namely, callosities, corns, and warts. Even when the units of the part of the skin affected by a callosity increase in number—numerical hypertrophy—the new units maintain a similar definite arrangement to those of the skin prior to the formation of the growth. In a corn, again, the new units, while on the whole observing a similar mode of arrangement to that of the healthy skin, yet tend in some measure to separate from the healthy tissues, and to form a continuous growth within itself; and in a wart this separation of the units from those of the surrounding tissues is complete—the growth becomes continuous with itself, and discontinuous with the surrounding tissues except at its point of attachment. And this law is not only true of these minor epithelial growths, but also of all the morbid growths which are under consideration at present. "The class of diseases which include tumours," says Sir J. Pagét, "may be reckoned as a part of the great division named hypertrophies or overgrowths;" and he adds, "if we compare any tumour with one of the hypertrophies that are least morbid, with one of those, for instance, in which the excessive growth is adapted to some emergency of disease, as an hypertrophy of the heart is adapted to some emergency of the circulation, we shall, I believe, always see between them this difference—that, to whatever extent the adapted hypertrophy may proceed, the overgrown part maintains itself in the normal type of shape and structure; while a tumour is essentially a deviation from the normal type of the body in which it grows, and in general the longer it exists the wider is the deviation."* We see, therefore, that as a growth deviates from normal structures it must become more *continuous;* but before using this word any longer it is necessary to enter into a short explanation. Sir James Paget, looking at the subject from a different standpoint, calls that *discontinuous* which I have called *continuous*.† When a growth becomes individualized, and its tissues sepa-

* Paget's "Lectures on Surgical Pathology," edited by Turner (1863), p. 372.

† *Ibid.* p. 387.

rated from those surrounding it, looked at from the standpoint of the organism as a whole, the growth has become *discontinuous;* but viewed from the standpoint of the growth, its tissues have become more *continuous*, and it is in the latter sense that the term is applied here. The cardinal distinction is, that in my view the principal share in the production of the new growth is taken by the locality affected, and not by some mysterious change in the blood; and this is a cardinal distinction affecting all the doctrines of the two schools of *Solidists* and *Fluidists*. This appears a fitting opportunity for making another explanation, which would be very unnecessary except for special circumstances. When "advantageous" and "disadvantageous," or their equivalents, are employed here, they are, unless anything is said to the contrary, applied to the disease, and not to the organism. It is, for instance, advantageous to small-pox that, as its particles lose the power of being generated from the tissues of the body by the concourse of ordinary causes, they should acquire independent powers of propagation. The special circumstance which renders this explanation necessary is, that investigations into morbid structures have been so bound up with the practice of the art of medicine, that it requires a little consideration to see that what is disadvantageous to the organism as a whole is advantageous to the morbid product. The influence of the art upon the science is specially manifest in the case of tumours, which have been divided into innocent and malignant for no other reason than that the growth of the latter is specially antagonistic to the life of the organism. Such a distinction may be very useful so far as the art of medicine is concerned, but it should not intrude itself into the science of pathology. Instead of looking at cancer as a malignant disease, and disadvantageous to the individual, an endeavour will be made here to show that, as these growths deviate more and more in certain directions from the healthy tissues of the body, they must acquire properties in other directions which will insure their propagation—properties which will be advantageous to the growth of the cancer, and, unless acquired, it would speedily die out. It has already been noticed that a growth which is not readily cultivated upon a healthy individual by ordinary changes in the environment must have deviated to some extent from healthy tissues, and that the first deviation is that the growth becomes more continuous; and now the obverse truth may be noted, that, other things being equal, the more the deviation becomes aggregated into a continuous growth, the greater the expenditure its production will entail upon the organization, and the less likely is it to be caused by changes in the incident forces. The more the units of the new growth become separated from the units of the surrounding tissues and aggregated into an independent growth, the less adapted will the correlated parts of the organism be to the new growth.

So that every unit derived from the correlated parts of the organism has to be adapted to the position it must occupy in the latter, and this adaptation entails cost. If, then, the units of the new growth acquire properties in another direction which will enable them to counterbalance this disadvantage, it must be by an economy of force. This economy is brought about by the new units losing all power of structural development, and retaining only powers of reproduction. Organization entails cost; the absence of organization is an economy of force in one direction which may be expended in another. That tumours on the whole do as a matter of fact lose all structure, is easily verified by observation; and the only question is, whether the higher power of reproduction which we have already seen this loss of development gives to the units of the growth enables them to act upon the organization in such a manner as to insure their propagation by inheritance.

This arrest of development and increased power of reproduction of the units involve many other concomitant changes. A rapid multiplication of units causes the cell-wall to become thinner, and it is apparent that this thinning of the cell-wall, along with the greater powers of multiplication, will enable the units of the new growth to cast off a greater number of gemmules than the units of the tissue in which the growth is produced. In this manner we see that as the growths become more continuous outwardly, it must, if it is to reappear except at long intervals, be accompanied by an inner discontinuity, by which a greater number of gemmules are cast off; and this enables the growth to act more powerfully upon the correlated parts of the organism. We saw that, as the units of a growth become separated from the healthy tissues and more continuous with themselves, it becomes more difficult to cultivate it upon a healthy individual. This implies that the correlated parts of the organism have less power for producing such a growth than if its units maintained the healthy type of development; and it follows that when once a growth of this kind is formed, the correlated parts of the organism will have less power over it, and that it must exhibit less tendency to return to health. We now see that as a growth becomes more outwardly continuous, this is accompanied by an inner discontinuity which increases the contagiousness of the new formation, while the contagiousness of health is acting upon the growth at a disadvantage. Hence, as a growth becomes more continuous outwardly, it becomes more independent of the original cause which has produced it, and is generally enabled to maintain its existence during the remaining life of the individual; and thus it is that "a tumour as a general rule increases constantly." * We have just observed that, as a growth becomes more outwardly

* Paget's "Lectures on Surgical Pathology," edited by Turner (1863), p. 373.

continuous, by giving up development and acquiring greater powers of reproduction, its units are enabled to cast off a greater number of gemmules in the same time than they could do if the power of development were retained; and now we may add that by the same means the growth acquires a more independent existence, which enables it to cast off gemmules for a long period independently of the continuance of the cause which originally determined its production. In this manner the power of a continuous growth over the organism is augmented, not only by an increase of the number of gemmules cast off in a certain time, but also by an extension of the time during which gemmules are produced.

But there is a third mode by which a growth receives a great accession of power over the rest of the organism. When discussing the contagious diseases, it was proved that a tissue becomes contagious, other things being equal, in proportion as its units exhibit high powers of reproduction. Therefore, as the units of the new formations under consideration at present acquire higher and higher powers of multiplication, their gemmules will possess higher powers of infecting the rest of the organism, independently of the increase in their number and of the time during which they act. In this manner, as a tumour deviates further and further from the healthy structures, its units must acquire higher and higher powers of multiplication, all trace of development must disappear, the cell-wall must become thinner at each increment of deviation, the tissues of the new formation must become more juicy, and the inner discontinuity which was inferred to be present in the minor deviations becomes more and more apparent, by producing secondary affections in the surrounding tissues, and in those organs which are in the line of the lymphatics and veins of the part. Such being the course of events, it might be supposed that the morphological characteristics of the rapidly infecting cancers are similar to those of the pathological products of the contagious diseases. But the multiplication of units in cancer must proceed within certain very narrow limits. Suppose, for instance, that the units of cancer multiplied as fast as those of contagious matter; the disease would be so rapidly fatal that no progeny could be left to inherit the peculiarity. If the true origin of cancer has been assigned here, the growth must have a certain degree of continuity; and if it is propagated by inheritance it must be permanent during the life of the individual; and we now see that it must not be too rapidly fatal. To secure these requirements involves certain peculiarities in the units of which the cancerous tissue is composed. If the cancerous units multiplied like pus in such a manner that the cell-wall were to disappear, they would either disperse through the body, and the disease would be rapidly fatal, or make their way to the surface, and become detached, so that the growth would not have the necessary permanence and continuity; and in either case the disease would cease to be propagated by inheritance. It is evident, therefore, that the mode of production of the cancerous units cannot be like that of pus, and still less, therefore, is it like that of the contagium particles. To secure the requirements already laid down, the units of cancer must possess a cell-wall, to give them the necessary cohesion and permanence; and also a high reproductive power when compared with that of the units of healthy tissue, although the rate of multiplication may be small when compared with the units of the contagious tissues. It would be impossible to meet those requirements if the cancerous units were produced according to the mode of multiplication which obtains in the production of pus-corpuscles and contagium particles; since, as we have already seen, in the latter the cell-wall must become gradually thinner, and the units smaller, as the power of multiplication increases. But another mode of multiplication is known, which, if present, would meet all the requirements of the case. I allude to the mode of endogenous growth, in which the individuality of the parent cell is lost in a swarm of progeny which has grown within it. According to this method of multiplication, the individual unit may attain a considerable size, have a comparatively thick cell-wall, possess some degree of permanence, and yet after all have a moderately high reproductive power. In this manner the tumour will have the necessary cohesion to make it a permanent growth, and the necessary rate of multiplication to give it the infecting power. It is almost unnecessary to add, that although a cancerous tumour must have a certain degree of continuity, it is not necessary for it to be composed entirely of true cancerous or infecting tissue. A muscle, besides the proper muscular cells, contains also connective tissue, not to mention blood-vessels and nerves; and similarly, a cancerous tumour, besides the proper infecting cells, contains also a more or less considerable growth of connective tissue or its allies, and this helps to give the growth greater cohesion and permanence; but on the other hand, when there is a large proportion of these tissues in the tumour, it interferes with its infecting power, and the progress of the disease is very slow.

We see, then, that the morphological characteristics of cancerous tissue and contagium must differ greatly; and that these deductive necessities correspond to the facts, may be verified, by referring to a good description of the microscopical appearances of cancerous tissue and virulent fluids. But there is a remarkable passage in Sir James Paget's Lectures bearing upon part of the subject just discussed; and since it is a description of facts, and fulfils the conditions of an inductive verification, I shall quote it at length. "Although," says he, "the various instances of recurrent tumours recorded present many diversities of structure, yet they may be said generally to have possessed the character of incomplete development, and to have approximated to the embryonic or rudimental, rather than the perfect state of the natural tissue; and this rule of persistent or arrested embryonic structure in the recurrent tumours is so general, that in practice it is advisable to speak with hesitation of the ultimate result of any case in which a tumour is found to be composed of rudimental tissues. This similarity in structure to embryonic texture becomes strongly marked after each removal and recurrence, so that a tumour which at first might be not unlike the normal fibrous or glandular texture in which it grew, after repeated removal and recurrence becomes softer, more succulent, and in its later growths may seem to the naked eye little more than like masses of yellow or ruddy soft gelatine, with blood-vessels. The latter are usually much more rapid in their progress than the earlier growths; they are generally less well defined, penetrating further and more vaguely among the interstices of adjacent parts, and more quickly protruding through the skin or scars over them;" "and in these characters," he adds, "the later formed tumours assume more of the character of malignancy than the earlier."*

This description is so apposite and so confirmatory of our deductive conclusions that it requires no

* Paget's "Lectures on Surgical Pathology," by Turner (1863), p. 576.

comment; and the gradations from the slighter to the greater deviations of recurrent tumours, with their corresponding degrees of infectiousness, are so manifest that little more need be said with regard to the cancerous diseases. The cancerous diseases differ from the recurrent tumours in the higher power the former manifest of infecting the organism at large, and we shall also find that they exhibit corresponding morphological differences. The units of the recurring tumours do not deviate very much from those of the tissue in which they grow; but in cancer the units differ from any found in the body at any stage of its development. But although this will account for much of the difference in the degree of infectiousness between the recurring tumours and carcinomata, it is highly probable that it will not account for all. But there is another peculiarity by which the cancerous are distinguished from the recurrent tumours, which may help us to solve the difficulty. As already noticed, the primary cancerous tumour becomes developed from a tissue which has descended from one of the epithelial layers of the embryo, while the recurrent tumours are developed from the tissues which have descended from the germinal layer. We have seen that epithelial structures are the least differentiated, and are characterised by high powers of growth; so that even the embryo itself was produced by the concourse of two modified epithelium cells. It is therefore highly probable that the original tissue from which the tumour has descended has a great deal to do with the degree of infectiousness which it can acquire.

But if we attend to the different forms of cancer, it will be found that they conform to the principles already advanced in their mode of growth and degree of infectiousness. In epithelioma the units of the morbid structure do not deviate much from those of the surrounding skin, neither in their rate of multiplication nor consequently in the other morphological characteristics which rapid reproduction involves; and hence the growth of the primary tumour is slow and its degree of infectiousness is small, whether tested by the time in which lymphatic invasion begins or by the extent of tissue invaded. The primary tumour may exist for years before the neighbouring glands become affected, and, as a general rule, the secondary affections are confined to the tissues surrounding the tumour and to the nearest lymphatics. On the other hand, in a free-growing cancer like encephaloid, the primary tumour grows rapidly, the units differ considerably from those in which the tumour is formed; they have a high rate of multiplication, and this produces the thinning of the cell-wall and diminished bulk of units, which must to a certain extent be its concomitants even in the endogenous mode of cell multiplication; and consequently the lymphatic invasion begins early, and the degree of infectiousness is sometimes so great that there is an almost simultaneous growth of cancer in every tissue of the body, reminding one of the general eruption of small-pox. It is evident, therefore, that there is a close agreement between the rapidity of the growth of the primary tumour and its degree of infectiousness, but, as already noticed, this relation is not uniform. The cause of this want of uniformity must now be found. As we have seen, a tumour influences the organism at large by the units of which the tumour is composed casting off a great number of gemmules in a short time, by the continued production of these gemmules for a long period, and by the gemmules differing from the tissues of the body in molecular motion. It is evident that the gemmules may be produced very rapidly, and that the other conditions of infectiousness may be present in a high degree; but if, owing to local causes, these gemmules are prevented from circulating through the body, it is manifest that the phenomena of infection will not appear. The tumour called encephaloid is the most rapidly growing of all the cancerous diseases, and its units vary the most from those of healthy structures, especially in the morphological characteristics which indicate rapid multiplication; but, as Mr. De Morgan says, "of all the true cancerous diseases, encephaloid is the one which may go on to spontaneous cure or to long quiescence." And Mr. De Morgan also supplies the explanation. "Encephaloid," he says, "is often the least infiltrating of cancers: it pushes the tissues aside, it forms connective tissue capsules around it; and hence, in a large number of cases, it is not brought into connection with the tissues as scirrhus is, so as to be disseminated amongst them."* It is apparent, therefore, that the degree of infectiousness of a morbid growth must depend not only upon the rate of multiplication of its units, and upon their differing in molecular motion from the units of healthy tissues, but that also a great deal depends upon that inner discontinuity of growth which deductive reasoning led us to believe is present in a minor degree in the non-infectious tumours.

Having now traced the origin of cancer from such minor structural changes as corns and warts, and enunciated some of the laws which must govern the formation of these growths if they are to become inherited, and having also verified the results obtained by reference to observed facts, we are in a position to discuss the chief points in which cancer differs from the contagious diseases, with a view to interpret them deductively. It has previously been mentioned that cancer is not contagious, nor readily propagated by inoculation to a healthy individual. These conclusions were then founded upon the inductive evidence, but we shall now endeavour to give them a deductive interpretation. The morphological characteristics which cancerous tissue must possess, on the supposition that it is a disease which is mainly propagated by inherit-

* *Lancet* (15th July, 1871), p. 81.

ance, has already been traced, and our theoretical conclusions were then found to be verified by reference to observed facts. On the other hand, it was shown that, if cancerous tissue did possess the morphological characteristics of highly contagious matter, the disease could not become inherited, so that all the evidence points to the conclusion that cancer is not propagated from individual to individual by contagion.

But if cancer is not propagated by contagion, it may be supposed that it possesses all the characteristics of a highly inoculable disease. The only characteristic which we found it necessary for a readily inoculable morbid product to possess was, that its units should vary to a considerable extent from the units of the normal tissues of the body. But this statement will now be found to require considerable modification. The units of a cancerous growth have varied considerably from those of healthy tissue, and at a certain stage in the progress of the disease they begin to infect other parts of the organism; and it is also known that a portion detached from the tumour can be readily grafted upon another part of the same organism; it may, therefore, be supposed that it can be grafted with equal facility upon the tissues of a distinct individual. But this conclusion will be found, upon further scrutiny, to be very doubtful. It must be remembered that cancer is an inherited disease, and that, therefore, owing to a morbid impulse bequeathed by the parents, the tissues of the organism as a whole must vary to some extent from healthy tissues in order to accommodate themselves to the growth of the tumour. And when the units of a cancerous growth are grafted upon another part of the same organism, they meet with units which have varied in the same general direction as themselves, although not so far, and consequently they become readily incorporated with those units. But because morbid units become readily incorporated with units which, however healthy they may appear, yet must have varied to some extent in the same general direction as themselves, this gives no warrant for concluding that the same morbid units would unite with other units which have not become morbid at all. The morbid products of the distinctly inoculable diseases have descended directly from the normal tissues of the body, or rather from the morbid products which may be cultivated upon a healthy person by change in the incident forces; and every increment of variation must have been accompanied by the acquisition of properties which will enable the disease to propagate itself by inoculation from one individual to another. Hence the power of propagating the disease to a distinct individual is acquired as the power of genesis from the normal tissues by ordinary causes is lost. But in the inherited diseases it is quite different. When a tissue varies considerably from healthy tissue, it must acquire properties which enable it to stamp its impress upon the whole of the organization, and the more it varies the more it must drag the entire organism after it in the same general direction; therefore, when the tissue has varied greatly from health, it must produce a profound modification of the individual, and may be readily grafted upon another part of the same organism; but it will probably be powerless to affect the tissues of an individual which has not come within the scope of its influence before it has varied so greatly from healthy tissue. From these considerations, it is manifest that cancer must be readily inoculable upon the same individual, and probably upon one who has a hereditary tendency to the disease, yet it need not be expected to be readily inoculable upon a healthy individual. But if cancer is not propagated by contagion nor by inoculation, the disease is undoubtedly more frequent amongst individuals who follow certain occupations, and amongst others who adopt a particular mode of living. It becomes, therefore, a question whether changes in the environment may not produce the disease independently of a hereditary predisposition. From what has already been said, it is evident that no changes in the incident forces could generate the disease in an individual who had no inherited tendency to the formation of cancer, or of its allies; but at the same time we have just seen that the hereditary predisposition was generated by changes in the incident forces acting upon a succession of individuals, and every increment of deviation in the growth from healthy tissues was assumed to be caused by a certain outward irritation. Such being the case, a certain amount of external irritation may be necessary in almost all instances to call forth the latent predisposition to the disease into activity at a particular time, and in a particular locality of the body. But not only may external irritation be necessary to render the disease active, but it is also probable that an epithelial growth which, under favourable circumstances, might remain innocent during the lifetime of the individual, may by continued irritation assume the aspects of malignancy. That there is a gradation between the innocent and cancerous growths, and that the transition between them may be made during the lifetime of a single individual, was amply proved inductively by reference to the history of the recurring tumours, which were found to become more and more like cancerous growths at each period of extirpation and recurrence. And that a close relation exists between cancerous and other epithelial growths is shown by an interesting observation made by Mr. De Morgan. "I have noticed," says he, "and it has been verified by the observation of many others, that concurrently with, or following on, the development of cancer, small outgrowths of warty or vascular or dermoid structure are frequent." These observations upon the transition which may take place from a non-infecting to an infecting growth in the lifetime of a single individual, and upon the relation which exists between innocent and malignant growths, give a very high probability to the conclusion that an innocent epithelial growth may, by long-continued irritation, even during the lifetime of a single individual become malignant.

Having now concluded my remarks on the genesis of cancer, several points would require consideration if the object were to write a treatise on that disease. Amongst these may be mentioned the favourite seats of primary cancer, and the diseases to which the growth is subject; but these will be passed over here, since, so far as I can see, their consideration would not throw any additional light upon the principles advanced. Before concluding this chapter, however, a few remarks will be made upon the age at which cancer generally appears. It was remarked with regard to the contagious diseases that, since one attack affords an immunity to a subsequent attack, and since the propagation of the disease depends upon the rapidity of the growth of the units of the contagious matter, those diseases would tend to become the diseases of childhood and early youth; and now it will be found that cancer must tend to be confined to adult life and old age. Since a cancer is an inherited disease, it will tend to appear in the progeny at the same age in which it appeared in the ancestors, and it must also be permanent during the life of the individual; and owing to the infecting properties acquired by its units, it is always fatal; hence, if the disease appears before puberty, no progeny would be left to inherit the peculiarity, and the disease, so far as this individual is concerned, would die out. In so far, therefore, as cancer is propagated by inheritance, except in very exceptional cases it will not appear until a few years after puberty. But there is another circumstance which will tend to prolong still further the age before cancer appears. It was already noticed that if cancer were too rapidly fatal, it would fail to be inherited; and it is probable that if cancer became active during the active growth of the individual, its course would be so rapid that it would fail to be transmitted. That the disease is more rapid in its progress in youthful subjects, the following quotation from Billroth will show:—"The earlier these carcinomata appear," says he, "the more proliferant the local tumour, the earlier the lymphatic glands are implicated, and the more rapid the whole course."* When cancer therefore appears in early life, even a few years after the age of puberty, the course of the disease would be so rapid that, in a large proportion of cases, the disease would fail to become inherited, so that this will restrict still further the time during which cancer can become active. From this it may be concluded that cancer, as an inherited disease, does not become active, except in very rare cases, until

* "Surgical Pathology and Therapeutics," by Dr. Theodore Billroth (1871), p. 638.

several years after puberty. I have said that cancer, as an inherited disease, does not become active before that time; but when syphilis was under consideration, it was found that the disease was propagated from parent to child by direct transmission, and not by inheritance; and it is manifest that cancer may be propagated in this manner also if the disease is more or less active in either of the parents. If this is the case, a few cases of cancer may be found in the child at birth and in early childhood; but such cases cannot, of course, propagate the disease. The limitations which the appearance of the disease must observe at the declining period of life is much more entangled; therefore I shall not enter upon its discussion at present. It is sufficient for my purpose to have indicated the general principle by which the question may be determined, so far as this can be done deductively.

CHAPTER VII.

THE CONSTITUTIONAL DISEASES.

THE INHERITED DIATHETIC DISEASES.

I HAVE already intimated that my object in discussing the constitutional diseases was to secure greater trustworthiness for the Graft theory in its application to the zymotic diseases, by showing that the main phenomena of other diseases can be explained by the same general principles. In dealing with the *Inherited Diathetic Diseases*, a similar method will be adopted to that employed when the *Inherited Local Diseases* were under consideration; and as cancer was then singled out as a good example of the latter, so will tubercle be now selected as an equally good example of the former. Tubercle, or rather the succession of tubercular diseases called tuberculosis, will be compared with the zymotic diseases; and in so far as there is an agreement between these diseases, there will be no necessity for making a special application of the hypothesis of Pangenesis to the former; and this will enable me to proceed immediately to search for the origin of the disease amongst the unexplained residuum.

But before proceeding further, it is necessary to give, if not a true definition, at least a few characteristics by which tubercle may be practically distinguished. It has been pointed out that the morbid units of the zymotic and cancerous diseases were modified epithelial cells; but the lymph gland may be taken as the type upon which tubercle is constructed. Dr. Burdon Sanderson has shown that all the favourite seats of tubercle are naturally characterised by the presence of a tissue, which is very similar to that of the follicles of Peyer, and of the ampullæ of the lymphatic glands. This tissue, which Dr. Sanderson terms "adenoid," "is to be found in the lymphatic glands, in the spleen, in the neighbourhood of the ducts of the liver and of the bronchi, in the serous and mucous membranes, and in the medulla of bone;"* and it has also been proved that wherever this tissue is found, it is in close relation with the lymphatic system. And since this tissue is present wherever true tubercle is found, it may be inferred that this morbid structure is never a heteroplastic formation, but is always the result of continuous development. With regard to the structure of tubercle, Dr. Sanderson says, that "the characteristic product of tuberculosis is not an aggregate of shrivelled particles of irregular form, but a tissue

* "Eleventh Report of the Medical Officer of the Privy Council," p. 94.

composed of lymph corpuscles, held together by a network of hyaline connective substance." * But although this is the most characteristic product of tuberculosis, it passes through several transformations, which it is necessary to notice. The tissue, composed of lymph corpuscles, and the network of hyaline connective substance, become, by-and-by, hardened by a process of fibrous degeneration; while, at a further stage, the central portion undergoes fatty degêneration and necrosis, constituting the cheesy metamorphosis. Now, this cheesy metamorphosis is one of the most important points in connection with the evolution of tubercle; and since this transformation occurs in other morbid products—such as pus, cancer, and sarcoma—a practical difficulty arises, which must be particularly noticed before proceeding further. The cheesy masses found in cancer and sarcoma can be readily distinguished from the tubercular cheesy products; but it is by no means so easy to distinguish between the latter and inflammatory cheesy products. "If a lung be laid before you with cheesy masses scattered through it," says Virchow, "and you are asked if that be tubercle or no, you will frequently be unable to say with certainty what the individual masses originally were. There are periods in the course of development when that which is inflammatory and that which is tuberculous can with precision be

* "Eleventh Report of the Medical Officer of the Privy Council," p. 116.

distinguished from one another; but, at last, there comes a time, if one does not know how the whole arose, when no opinion can any longer be formed as to what its nature is."* In dealing with this part of the subject, it will be as well to recur to the theory of inflammation previously advanced, since the determination of the conditions under which the inflammatory cheesy metamorphosis occurs will assist us in ascertaining the conditions which lead to this transformation in tubercle, and will enable us at least to point out the cause of the difficulty which exists in distinguishing between the tubercular and inflammatory cheesy masses. It may be remarked, in passing, that it is only when the cheesy mass is found in "adenoid" tissue that any practical difficulty can arise; since, if it be included in the definition of tubercle that the morbid process must occur in "adenoid" tissue, there can be no difficulty in deciding that, when a cheesy mass is found in any other tissue, such as in the alveoli of the lungs and in the terminal bronchi, it cannot be of a tubercular nature. But this distinction between these two products is one derived from the physiological tissues, and not from any essential difference in the morbid processes which lead to them. It now remains to be seen whether any pathological distinction can be drawn between these morbid products and the diseased processes which precede and cause their formation.

* Virchow's "Cellular Pathology," translated by Dr. Chance, p. 479.

The first stage of inflammation was shown to be an excess of the normal action of the part, an excess which produces such rapid cell proliferation as to lead to a secondary oscillation in the opposite direction. During this secondary oscillation the struggle for nourishment amongst the rapidly formed units is so great that they undergo a fatty metamorphosis, the cell-wall ruptures, and the whole product becomes dissolved and ultimately re-absorbed; or it makes its way to the surface and becomes detached. Sometimes, however, the cells during the secondary oscillation remain entire—they become partly filled with fatty particles; the fluid is absorbed, but the morphological elements remain, constituting a cheesy nodule embedded in the tissues.

In tubercle, on the other hand, the process which leads to the formation of the cheesy mass is very much slower than this. The primary oscillation, by way of excess of action, is very slight; the cells do not proliferate rapidly, so that the new growth does not depart greatly from the healthy condition. The consequence is that the tissue does not break up into independent units, and the struggle for nourishment is not so great as to lead directly to the fatty metamorphosis; the cells retain for a time their albuminous contents, but as the capillaries become pressed upon and the circulation enfeebled, the fluid portion of the new growth becomes absorbed, and this constitutes the fibroid degeneration; and as the capillaries become obliterated, the supply of nourishment is in a great measure cut off, and this leads to the fatty degeneration and necrosis of the cells which constitute the cheesy metamorphosis. We see, therefore, that the only real pathological distinction which can be made between the inflammatory and the tubercular cheesy masses, even when the history of the morbid process which causes the formation is known, is that the evolution of the former is cell-proliferation, formation of pus, caseation, and of the latter cell-proliferation, fibroid degeneration, and caseation. But every case of inflammatory cell-proliferation is not followed by the formation of pus; while there is no reason to doubt that, even in the cases where pus is not produced as an intermediate formation, the cheesy metamorphosis may result. Therefore the distinction which has just been made between the morbid process which leads to the tubercular, and that which leads to the inflammatory cheesy product vanishes; and we may expect to find that the one morbid process graduates into the other; and that, however distinct the two products may be in certain cases, intermediate forms will be met with in which, even when the whole history of the disease is known, it is impossible to decide whether the product is tubercular or inflammatory. That there is no essential difference between the tubercular and inflammatory product is still further confirmed by the fact that the inflammatory cheesy nodule frequently gives rise to true tubercle. At a certain stage the cheesy nodule, whatever may have been its origin, begins to soften in the centre, owing to necrosis of its units. Now it is known that dead tissue always excites cell-proliferation in the surrounding living tissues with which it is in contact; and when this cell-proliferation extends to the adenoid tissue diffused through the surrounding tissues, or to that of the nearest lymphatic glands, it may be followed by fibroid degeneration and subsequent caseation constituting true tubercle. "That chronic catarrhal pneumonia," says Niemeyer—"the so-called gelatinous infiltration —leads more frequently than the other forms of pneumonia to tuberculosis, has its cause simply in this, that its products, more frequently than those of a common acute, or of an acute catarrhal pneumonia, undergo a cheesy metamorphosis."* But the primary cheesy products in catarrhal pneumonia cannot be reckoned as true tubercle, not only because they are the result of inflammatory action, but also because they are formed from epithelial tissue. That the production of true tubercle is a secondary result of inflammatory caseation is well shown in one of Dr. B. Sanderson's experiments, in which a seton of unbleached cotton was inserted by means of a needle, which had not been used before, in the shoulder of a guinea pig. The animal died in about two and a-half months, and in the locality where

* "Clinical Lectures on Pulmonary Consumption," by Felix Von Niemeyer; the New Sydenham Society's Translation, p. 14.

the seton was inserted were found all the signs of inflammatory action, "and there were numerous purulent deposits in the neighbouring muscles and areolar tissue;" but the internal organs contained growths presenting the characteristics of true tubercle. "In this case, therefore," says Dr. Sanderson, "a pathological process which had originated in traumatic subcutaneous suppuration, resulted in lesions which were characteristically tuberculous." * If, then, tubercle is defined as a morbid process occurring in adenoid tissue, and consisting primarily of cell-proliferation, which subsequently undergoes fibroid degeneration and cheesy metamorphosis, it will be evident that when a morbid product is found in other than adenoid tissue, it cannot be tubercle. But the remarks just made show that when adenoid tissue becomes the seat of the disease there is no distinct line of demarcation between the inflammatory and the tubercular products; and that they must graduate into each other by intermediate forms. These observations anticipate a subject which will be more fully discussed hereafter, but it is necessary to give some definition of tubercle before comparing the disease known as tuberculosis with the zymotic diseases. We are now in a position to undertake this comparison.

The relation which exists between tuberculosis and the zymotic diseases has been so fully worked

* "Tenth Report to Privy Council," p. 150.

out in recent years that a very brief notice of the subject will suffice here. From a strong conviction that tubercle was a zymotic disease, M. Villemin was led to conduct a series of experiments which has amply proved that tubercle can be communicated to some of the lower animals by inoculation; and therefore it is highly probable that it can be propagated from one man to another in a similar manner. These experiments, however, do not bear out M. Villemin's inference that tubercle is a contagious disease, a conclusion which is discredited by the subsequent experiments of Drs. Fox and Sanderson, not to speak of other workers, both in this country and in Germany. These experiments are so well known that it is only necessary for me to notice the results obtained. It is now amply proved that although tubercle can be propagated by inoculation with tubercular matter on some of the lower animals, it may also be produced in them by inoculation with various other matters. Amongst many agents which have been employed may be mentioned various inflammatory products—a thread saturated with vaccine lymph, putrid muscle, and, as already mentioned, Dr. Sanderson has shown that a local irritant, such as a seton, is capable of causing the disease. This conclusively proves that even if tubercle is occasionally propagated by inoculation, it also arises from other causes, and therefore it is not entitled to be regarded as a zymotic disease. The fact that tubercle can be propagated by inoculation no more proves it to be a zymotic disease than the occasional production of pus by inoculation with pus proves the latter to be a contagious disease. And irrespective of any experimental evidence the fact just noticed that there is no distinct line of demarcation between the tubercular and certain inflammatory products, renders it probable that the former has not varied sufficiently from the healthy tissues to render it a readily inoculable and certainly not a highly contagious disease.

But if tubercle has not properties which enable it to be propagated from individual to individual by contagion, the phenomena of contagion in an organism once affected are well marked. This subject has also been so thoroughly worked out by recent investigators that I need only allude to the conclusions arrived at, without stating the grounds upon which these are founded. These results are so succinctly given by Dr. Sanderson that I shall quote his words at length: "The primary local lesion in artificial tuberculosis," says he, "whether the cause be simple wound or specific inoculation, consists in the development at the seat of injury of granulations or nodules which have similar structural characters with those of adenoid tissue elsewhere, but cannot as yet be shown to be in relation with the absorbent vessels. The first step in the dissemination of tubercle consists in its being absorbed primarily by the lymphatics (which convey it to the lymphatic glands of which they are tributaries), and secondarily by the veins. Having thus entered the systemic circulation, it is distributed universally by the arteries. The serous membranes, however, appear by preference to appropriate it, and from them it extends by contiguity to the superficial parts of the organs which they cover. The final stage of the process consists in the tertiary infection of the glands of each diseased organ, which glands consequently undergo enlargement and induration, and eventually become partially caseous."* Here, then, are phenomena of infection similar to those with which we were so familiar when discussing the zymotic and cancerous diseases; and it is worthy of remark that not only is the infectious matter distributed in the line of the circulation, but that its appropriation by the serous membranes shows an elective affinity similar to that which was assumed to exist in the case of small-pox. Amongst many hypotheses propounded to account for the infectious phenomena of tubercle, I am glad to find that the opinion of one distinguished pathologist is in accord with the view advanced here. M. Chauffard compared "the effect of inoculation to the fecundation of one tissue by the contact of elements emanating from another, the fecundated

* "Eleventh Report of the Medical Officer of the Privy Council," p. 116.

tissue being thereby caused to produce histological elements not resembling its own, but those of the fecundating tissue."*

When discussing the zymotic diseases it was found that the primary lesion impressed its characteristics upon the secondary lesions, both with regard to the kind of morbid structure produced and to the time which the latter take in passing through their evolution; and this rule was found to be in a great measure true with regard to the cancerous growths. But this law requires a certain qualification before it can be said to be applicable to tuberculosis. When once tubercle forms in a locality it tends to become disseminated in the adenoid tissue surrounding the primary locality affected, in the neighbouring lymphatic glands, and in various other parts of the body; and the secondary and tertiary lesions are, like the primary, of a tubercular nature. So far, then, the law with regard to the kind of lesion produced is observed. But it has been noticed that an inflammatory lesion may cause tubercle, and this introduces an element into the comparison which has not met us hitherto. In making a comparison between a case of tuberculosis which arises from a local irritation and that of a zymotic disease, the inflammatory centre which has preceded and caused the disease in the former instance must be reckoned

* See "Tenth Report of the Medical Officer's Report to the Privy Council," p. 148.

as the primary lesion; and if there is a distinction in kind between tubercular and inflammatory products, our law fails to be applicable to tuberculosis; but if, as I have endeavoured to show, there is only a distinction of degree between them, the law is quite applicable to this disease.

But the correspondence in time between the evolution of the primary and the secondary lesions cannot be so clearly traced as in the zymotic, nor even as in the cancerous diseases. But a comparison of the acute and chronic varieties of tuberculosis shows that in all probability such a correspondence exists, however much it may be obscured by attending circumstances. In the acute variety the disease is rapidly fatal, and almost all the organs of the body become invaded with tubercle; while in the chronic variety not only is the progress of the disease slower, but the number of organs invaded is more limited. This shows that the morbid product in the former has higher powers of infecting the organism than in the latter. It may also be noticed, that when the first stage of the disease is rapid all the subsequent stages will be equally rapid, and the disease will soon be fatal; while, on the other hand, the slower the course of the first stage of the disease, the more likely is it to be protracted.

But there is another circumstance in which tuberculosis differs from the zymotic and cancerous diseases; but further consideration will show that in this respect also the difference is more apparent than real. We have seen that the primary lesion in the zymotic and cancerous diseases occurs in the tissues derived from one or other of the epithelial layers of the embryo; but tubercle is developed in adenoid tissue which is derived from the germinal layer. It has just been noticed, that in instituting a comparison between a case of traumatic tuberculosis and a case of a zymotic disease, it is necessary to regard the inflammatory lesion as the starting point of the former disease, and this lesion may occur in epithelial structures. It may, therefore, be found that an inflammatory lesion of epithelial tissue, if not an integral part of tuberculosis according to the restricted definition employed here, frequently plays an important part in the production of the disease. I think it will be admitted that, if the lungs be excepted, tubercle of the internal organs generally results as a secondary formation to caseation, whether inflammatory or tubercular, of some of the lymphatic glands. But the enlargement of the lymphatic glands, which leads to the cheesy metamorphosis, results, in the generality of cases, from irritation of the structures from which the afferent vessels of the gland are derived; and the structures, which are the seat of the primary irritation, are generally epithelial. The most frequent causes of enlargement of the cervical lymphatic glands are moist eruptions about the head and face, and teething in children; and swelling of the abdominal glands generally results from inflammation of the mucous membrane of the alimentary canal; while the formation of real tubercle in the lungs, when not a secondary result of caseous degenerations in some other parts of the body, is always—according to Niemeyer—produced by caseous degeneration of epithelial accumulations in the alveoli and terminal bronchi, principally after chronic catarrhal pneumonia. This shows that the zymotic, cancerous, and tubercular diseases exhibit a considerable degree of similarity in the succession of structures which become affected. The disease always in some, and very frequently in others, starts in epithelial tissue; the surrounding tissues and neighbouring lymphatic glands become next affected; the internal organs are subsequently invaded; and there is a tendency manifested, which is strongly marked in some and faintly marked in other cases, for the dense structures of the body to become finally affected. But although a general agreement in the succession of tissues affected in those diseases is manifested, this is quite compatible with the exhibition of special affinities by each diseased product for certain tracts of tissue. The zymotic diseases and tuberculosis have now been compared, and several fundamental agreements have been detected, and, in so far as their phenomena harmonize, the application of the hypothesis of Pangenesis already made to the former is equally suited to the latter. We shall now proceed to dwell a little more upon the differences between the zymotic diseases and tuberculosis, with a view to interpret them and to trace a probable mode of origin for the latter disease.

The experiments which have been conducted of late years are adverse to the idea that tuberculosis is a contagious disease; and although it can, under certain circumstances, be readily communicated by inoculation, it is only like ordinary pus in this respect; and further experiments have shown that it may also be caused by other means, so that it is neither a contagious nor an inoculable disease as these terms have been defined here. But tuberculosis has been produced by local irritation, independently of pre-existing tubercle, and it becomes a question whether it should not be classed along with inflammation amongst the general diseases. It is well known, however, that no amount of local irritation, even when combined with other favouring circumstances, will generate tubercles in some individuals; while others are so susceptible to the disease that they acquire it under circumstances the most adverse to its genesis, and apparently without any external determining cause. It is evident, therefore, that inheritance has something to do with the propagation of the disease, and we must now endeavour to trace the mode in which it acts. This disease could not be locally acquired in a succession of individuals, like cancer, since it has not the morphological characteristics which the pathological products of locally acquired diseases, especially when infectious and fatal, must possess; nor is it confined, as such diseases generally are, to childhood and adult age. Some other mode of origin must, therefore, be sought for tuberculosis.

It has already been noticed that a disease may be propagated by inheritance, either directly or indirectly. In the one case a direct tendency is developed in the organism to produce the disease at a certain age, while in the other case an indirect disposition to certain diseases is generated as a concomitant of some other modification of the organism. All the evidence tends to show that tuberculosis belongs to the latter class of inheritable disease. The close relation which exists between the production of tubercle and a certain modification of the organism termed a Diathesis has long been noticed, and the particular modification which exhibits the tendency to tubercle has been called scrofulous, so that the first question which presents itself for solution is, how has the scrofulous diathesis been acquired? But before proceeding to give a probable answer to this question, I shall make one or two quotations from Niemeyer, to show that the view adopted here is sanctioned by his high authority. "I do not hesitate to say," he remarks, "in spite of all assertions to the contrary, that it is by no means sufficiently proved that tuberculosis, in the strictest sense, is an inheritable disease."* And again he says, "Quite as decidedly as we have opposed the evidence that tuberculosis is inheritable, must we pronounce in favour of a frequent occurrence of an inherited disposition to pulmonary phthisis. But even here what is transmitted is not the disease itself, but a weakness and vulnerability of constitution which in the parents has already either been the cause of pulmonary phthisis, or has been developed in them by the disease."† We shall now proceed to delineate a method or methods, by which it is possible that the scrofulous diathesis *may* have been originally acquired, even if it be impracticable to *prove* that this disposition *must* have been acquired by such means.

It is necessary in the first place to make a few general remarks upon the relation which exists between certain modifications of the organism, and a disposition to particular diseases, in order to find out some general law or laws to which these diatheses must conform. The inherited disposition to epilepsy, induced in guinea-pigs by section of one-half of the spinal cord, has already been mentioned; but it is highly probable that such a diseased tendency as this would be lost in a few generations, either directly by the contagiousness of the healthy tissues enabling them to encroach upon

* "Clinical Lectures on Pulmonary Consumption," by Felix Von Niemeyer, M.D., p. 19.

† *Ibid.* (Syd. Soc.), p. 20.

the diseased structures, or indirectly by the extinction of the individuals having this tendency under circumstances of competition, unless indeed the disposition to the disease is being constantly generated from the healthy. The diatheses under consideration at present have, however, maintained themselves for ages; and it may therefore be inferred that the tendency to extinction, either by direct or indirect means, is counteracted by other tendencies which enable them to maintain themselves. Hence, in tracing how an organism having a certain disposition to disease may have been originally modified, it must be shown that the disadvantage which this tendency to disease entails upon the individual is counterbalanced by corresponding advantages, which enable a succession of such individuals to maintain themselves in the struggle for existence, or it must be shown that if a diathesis manifests a strong tendency to become extinct, either by direct or indirect means, it also exhibits a corresponding facility in being generated from healthy individuals. It is not probable that the counterbalancing advantages can be clearly distinguished in all instances, especially when the disadvantage of the disposition to the disease is not very great; but in the case of the scrofulous diathesis, where the tendency to the production of tubercle must be a great disadvantage to a succession of individuals, the corresponding advantages ought to be clearly traceable. In the gouty diathesis, for instance, the tendency to active disease is not strongly marked until a late period of life, so that the disadvantage of the modification of the organism in the struggle for existence is small; but even in this case it is not difficult to point to certain advantages which those inheriting a gouty tendency generally possess. That a certain mode of living has a tendency to develop this disease in an individual who inherits a tendency to it is well known, and the gouty tendency itself has probably been acquired by a succession of individuals adopting certain habits, and transmitting to their posterity, both by example and by inheritance, a preference for a similar mode of living. In this case, therefore, an inherited tendency to the disease, and an inherited tendency to the adoption of a certain mode of living, combine to produce the morbid process. And not only is the disposition to disease in gout a disadvantage to the individual, but the inherited tendency to the adoption of the mode of living which tends to develop the disease is a further disadvantage, since it is well known to entail great expenditure. On the other hand, it is only certain individuals of a society who can afford to adopt this mode of living, and this implies that they have acquired corresponding advantages over their competitors in the struggle for existence; and it is not difficult to trace how these advantages may have been acquired. Even amongst savages a few men gain an ascendency over the rest of the tribe, which give to them and to their progeny certain advantages in the struggle for existence; but during the transition from the savage state to modern civilization the ascendency gained by some individuals over the rest was much greater, and the advantages and privileges acquired by these individuals were more firmly secured by laws to their successors. In this manner it is quite possible for a succession of individuals to adopt a mode of living which is wasteful in itself, and which subjects them to disease without sacrificing the advantages which an acquired ascendency in property, and what is known as "position in society," secures to them and to their descendants. That gout has been developed in this manner is strengthened by the fact that on the whole this disease is confined to the wealthier classes of society, and by the additional fact that it is principally confined to the male sex; since present observation, and especially the history of the habits of our forefathers, proves that the mode of living which tends to develop gout is mainly adopted by the male sex, and as it is a disease which does not appear until a late period of life, it tends to be limited to that sex.

But if gout is a disease of the rich, it is not the less true that tubercle is on the whole the disease of the ill-fed and the starved; and therefore it might be inferred, since the former is generated by the adoption of what is called "high living" by a succession of individuals, that the latter is generated by poor living in another succession. There can be no doubt that this is an important element in the genesis of the disease, and that, when misery and poverty are bequeathed to the progeny, the tendency to tubercular disease is very much intensified. But on the supposition that the diseased tendency is generated by inherited misery and poverty, the diathesis could not be associated by any very marked corresponding advantages. Consequently, these individuals, from the poverty of their bequeathed possessions, and from their inherited tendency to a terrible disease, if left to the rigid operation of natural selection, as they would be in a savage state of society, would soon become extinct, and in modern societies, even when it is an acknowledged principle that every member of the community must be kept above the level of starvation, these heavily weighted individuals would be found amongst the lower stratum of society, in our workhouses, penitentiaries, prisons, and lunatic asylums. There can be no doubt that the most terrible cases of the disease, whether in its active state or as a latent tendency, are found in the latter circumstances. And if the disease were dependent upon these individuals for its propagation by inheritance it would die out in a few generations; but it has already been noticed that if on the one hand there is rapid extinction of the disease, there must be on the other hand, if the disease maintains itself, a corresponding rapidity of genesis from healthy individuals. It is admitted that tubercle may be developed by a combination of circumstances favouring its production in a healthy individual, and this may readily happen in a succession of individuals, so that the deductive necessities of this supposition correspond to a considerable extent with observed facts. It is scarcely necessary to add that although this supposition of the mode of origin of the tubercular tendency affords a satisfactory explanation to a great many cases, yet there is a large residuum which is still unaccounted for. The scrofulous diathesis appears in individuals whose ancestors have for generations been above the condition of poverty, and who also exhibit qualities which enable them to compete successfully in the battle of life with individuals who have no diseased tendency. These high powers of self-maintenance are in some cases so remotely connected with the diseased tendency, that their concomitance may be called accidental; but even the cases where advantageous properties are accidentally associated with a diseased tendency would tend to become preserved by natural selection. But compensating properties may, however, be acquired in a more direct way; and we shall now proceed to sketch a probable method by which certain advantageous properties and a tendency to tubercular disease may have become coincidently developed in a succession of individuals.

In a case of inherited disposition to disease, the compensating change may be supposed to be sometimes functionally acquired. Excessive exercise of one of the active tissues of the body may leave a weakness in another direction which generates a tendency to disease. Excessive muscular action, for instance, is known to produce a tendency to disease in individuals, such as trained gymnasts; and if the operation of this cause is more difficult to trace in a succession of individuals, it may be inferred that such a relation between excess in one direction and deficiency in another is sure to result. But that the tendency to tubercular disease has not been developed as a concomitant of excessive muscular exercise, may be concluded from the fact that in the scrofulous diathesis the muscular system is on the whole badly developed. It is more probable that this constitutional weakness is related to an excess of nervous action, because in the first place excess of nervous action, such as trouble and anxiety, is apt to generate the disease in an individual, and more surely would this occur in a succession of individuals; and in the second place some who have a strong tendency to consumption exhibit mental qualities of a high order. That the tendency to tubercular disease may occasionally be generated by excessive action of the nervous system, along with neglect of other functions, acting upon a succession of individuals, is very probable; but on the other hand it must be remembered that if the scrofulous diathesis and high mental endowments are sometimes associated, this is by no means frequently the case. On an average those who have the scrofulous diathesis are deficient in the intellectual and active mental characteristics which give to their possessor a calm judgment, indomitable courage, and perseverance—the qualities which will ensure success in an open struggle for existence against all competitors. On the whole, therefore, it is not probable that the scrofulous diathesis, with its diseased tendency on the one hand, and the compensating advantages which we assume to exist on the other, has been developed by the direct operation of natural selection; but there are many indirect ways in which the principle of "survival of the fittest" may act.

It is now admitted by almost all competent authorities that when human history is contemplated on a wide scale, the present condition of civilized nations is to be accounted for by a theory of *progression* from savage life, even if it require to be supplemented to a certain extent by a theory of concomitant *degradation.* One of the first steps which a savage race takes towards refinement is the adoption of some sort of covering for the body; but simple as this change may appear, it must lead to many modifications of the body, all of which may not be clear gain to the individual. During the advance of primitive man towards civilization only small portions of the body would be covered at first, so that the adoption of clothing for the entire body would be gradual, and hence no very marked effect would be produced. If, however, a savage in the present day, who lived a large portion of his life almost naked, and whose ancestors were naked for ages, were suddenly to adopt the habits of civilized man, it is almost self-evident that changes would take place in his organization which would render him more subject to certain diseases, even if he were rendered less liable to others. The adoption of a covering for the body would ward off many forces which might otherwise be injurious to life; but, at the same time, the skin would become more tender and less able to resist those incident forces. Any one can convince himself that the skin would become more delicate and tender under such circumstances by noticing the difference between the skin of hands habitually protected by gloves, and those exposed to frequent atmospheric changes. When a savage, therefore, suddenly adopts clothing, the skin over the entire body becomes more tender, and it is manifest that this modification of the epithelial covering would entail certain modifications of all the correlated parts of the organism. We have already seen that the superficial lymphatic glands are very sensitive to the morbid processes of the skin, and it is therefore probable that they would participate in the superinduced tenderness of that tissue; and this tenderness of skin and sensitiveness of lymphatic glands are closely connected, if they do not form an essential part of the scrofulous diathesis.

But there is another mode by which the adoption of clothing by a savage might modify the organism. It is well known what a remarkable effect is produced upon the growth of a plant by excluding it from the rays of light, the most obvious being that produced upon its colouring matter. Now, mysterious as is the function of the colouring matter of vegetables, and that of the blood, they are in all probability closely related; and it is possible that the sudden exclusion of the major part of the body of a savage from the direct action of the sun's rays, might cause a deficiency in the colouring matter of his blood, and this deficiency is a concomitant of the tubercular tendency. It is notorious that savages are very free from tubercular diseases until they come in contact with civilized man, and then they are frequently decimated by this disease; and this is one of the most powerful arguments used by Dr. Budd, and by others, to show that tuberculosis is a zymotic disease; and my object in making these remarks is to show a probability that these facts may be explained on another assumption.

It is maintained by Carlyle that clothes were not originally adopted by man for comfort, but for decoration. "Warmth he found in the toils of the chase," Teufelsdröckh is made to say, "or amid dried leaves, in his hollow tree, in his bark shed, or natural grotto; but for decoration he must have clothes. Nay, among wild people we find tattooing and painting even prior to clothes. The first spiritual want of a barbarous man is decoration, as indeed we still see among the barbarous classes in civilized countries." *

But not only were clothes originally adopted and afterwards variously modified for decoration, but the body itself has been variously modified for a similar purpose; and Mr. Darwin has shown that these modifications are produced in the sexes principally in relation to reproduction. When an individual acquires a certain modification which is advantageous, not from enabling him to survive in the struggle for existence, but by rendering him superior to other individuals of the same sex and species in exclusive relation to reproduction, this constitutes sexual selection.† This is a very wide subject, and I must limit my remarks as much as possible to that part which has a direct bearing upon the subject under discussion, which is to show that in developing certain qualities by sexual selection, man has also produced a tendency to disease, and more especially to tubercular disease. There can be no doubt that beauty gives an advantage to an individual over another of the same sex and species, especially amongst those animals which exercise a choice in the selection of a partner; and Mr. Darwin has shown that beauty has been acquired through long generations by a process of

* "Sartor Resartus," by Thomas Carlyle, People's edition, p. 26.

† See "The Descent of Man, and Selection in Relation to Sex," by C. Darwin, vol. i. p. 256.

sexual selection. "In the same manner," says he, "as man can give beauty, according to his standard of taste, to his male poultry—can give to the Sebright bantam a new and elegant plumage, an erect and peculiar carriage—so it appears that in a state of nature female birds, by having long selected the more attractive males, have added to their beauty." * In the human species, however, the advantages of female beauty in relation to reproduction are much more conspicuous than those of the male; and since disease is more likely to be developed as a concomitant of the former, our remarks will be chiefly confined to it.

We are now met by the question—what constitutes female beauty? "What different ideas," asks Adam Smith, "are formed in different nations concerning the beauty of the human shape and countenance! A fair complexion is a shocking deformity upon the coast of Guinea. Thick lips and a flat nose are a beauty. In some nations long ears that hang down upon the shoulders are the objects of universal admiration. In China, if a lady's foot is so large as to be fit to walk upon she is regarded as a monster of ugliness. Some of the savage nations in North America tie four boards round the heads of their children, and thus squeeze them, while the bones are tender and gristly, into a form that is almost perfectly square."† In some of these instances it is

* "The Descent of Man," by C. Darwin, vol. i. p. 259.

† "The Essays of Adam Smith, LL.D., F.R.S." (1869), p. 175.

impossible to describe the points which are most admired, without at the same time giving a good description of a disease; but in most instances a true disease does not constitute a part of the standard of beauty; but in many cases a certain susceptibility of constitution is generated which gives a tendency to disease.

But not only does the standard of beauty differ in different races, but it is probable that almost every individual has a standard of his own. At the same time if the descriptions of female beauty given by our poets and novelists did not appeal to the sentiments of their readers they would fail to enlist their sympathies; and it may be concluded that the descriptions of those poets who have exercised a wide influence over any one nation are, on the whole, true to the highest ideals of beauty formed by the individuals constituting the nation. Listen then to the description of the ideal of beauty by a recognised master. "Reader," says Carlyle, "the heaven-inspired melodious Singer; loftiest Serene Highness; nay, thy own amber-locked, snow-and-rose-bloom maiden, worthy to glide sylphlike almost on air, whom thou lovest, worshippest as a divine presence, which indeed, symbolicably taken, she is, —has descended, like thyself, from that same hair-mantled, flint-hurling aboriginal Anthropophagus."* With this may be compared one kind of scrofulous

* "Sartor Resartus," by Thomas Carlyle (People's edition), p. 26.

diathesis, as delineated by a scientific poet. "The complexion is fair," says Professor Miller, "and frequently beautiful, as well as the features. The form, though delicate, is often graceful. The skin is thin, of fine texture; and subcutaneous blue veins are numerous, shining very distinctly through the otherwise pearly-white integument. The pupils are usually spacious; and the eye-balls are not only large but prominent, the sclerotic showing a lustrous whiteness." * Who does not recognise the close similarity between these two pictures drawn by very different men for very different purposes? The first in a few phrases strikes off a vivid picture of female beauty; and therefore the existence of the beauty is silently assumed; while with the second the object is to describe the prominent characteristics of a certain vulnerable organisation; but in every line there is a distinct recognition of the existence of beauty. In the one delineation amber-locked, snow-bloom are descriptive phrases corresponding to fair complexion, skin thin, and of fine texture, and pearly white in the other; in the former the rose-bloom is dwelt upon, which is fittest to arouse the sentiments of the lover; while in the latter the blue veins shining through the integument is noticed, which, although it arises from the same physical cause as the rose bloom, is more suggestive to the physician. In the first picture all that is meant by gliding sylph-like almost on air, is suggested by

* See Aitken's "Science and Practice of Medicine," vol. ii. p. 213.

delicate and graceful in the second. Who can doubt that so long as delicacy and gracefulness of form, along with a light transparency suggestive of gliding on air, constitute some of the most fundamental elements of the highest female beauty as conceived by a large portion of civilised man, a tendency to certain diseases will be generated as a concomitant by sexual selection? The sylph-like beauty portrayed by Carlyle entails a large amount of expenditure of force in its genesis and maintenance, without adequate return in simple mechanical action; and this want of economy is, in other words, a tendency to disease. It is evident that beauty of this stamp could not be maintained even in a single individual without a great deal of protection and expenditure which robuster organisations could dispense with.

The similarity between the poetic ideal of a certain standard of beauty, and the scientific description of a certain class of individuals who have a tendency to tubercular disease, is of itself an argument that the one is developed as a concomitant of the other. But if there is a real bond of causation between the two these outward resemblances ought to be accompanied by profounder internal agreements; and it will hereafter be found that such agreements exist.

That both beauty and the scrofulous diathesis are inherited are facts so familiar that it is scarcely necessary to mention them. Medical men especially are perfectly conversant with the inheritance of scrofula; and poets are equally familiar with the inheritance of beauty.

(*Benvolio.*) "Then she hath sworn that she will still live chaste?"
(*Romeo.*) "She hath; and in that sparing makes huge waste:
For beauty, starved with her severity,
Cuts beauty off from all posterity."

But before proceeding further to trace resemblances between beauty and the scrofulous diathesis, it will be as well to remove one or two objections which might be urged against the hypothesis that there is a causal connection between them. It may be said that, as a matter of fact, beauty is not always associated with disease, and much less is it always associated with scrofulous disease. But it is not maintained here that every style of beauty develops a tendency to disease. Into the general question of the relation which must exist between the development of beauty and diseased tendencies, we do not wish to enter at present; we wish to confine our remarks to a particular style of beauty, and observation will show that this particular style is very frequently associated with a tendency to tubercular disease. An objection of greater weight is, that active tubercular disease is not often found associated with great beauty. But the perishable nature of beauty is well known, so that when disease becomes active the beauty vanishes. But even although the inroads of disease will cause beauty to fade, there is no other disease, probably, in which we find beauty preserved so long as in tubercle, and this is notably the case in tubercular phthisis.

But the most serious objection is, that although those who manifest a strong tubercular tendency exhibit certain fragments of beauty, yet in very few instances do they come up to any critical standard. This, however, is easily explicable on the supposition that the diseased tendency has been developed by sexual selection. The great advantage which a beautiful woman possesses in the struggle for existence is, not that she can select a partner of any kind, but that she can select the one who is best able to afford her the necessary protection for the preservation of herself and her progeny. It has been noticed already that beauty entails expenditure, and so long as the circumstances of the family are such that this expenditure can be met, that beauty which is compatible with each age may be preserved and active disease warded off. The perishable nature of beauty in the individual has just been mentioned, and we must now notice that it is equally perishable in a succession of individuals. That beauty perishes in a succession of individuals is recognized by poets as a silent influence guiding their plots, rather than distinctly expressed. How frequently does the novelist give to the fictitious beauty he has discovered in the cottage a fictitious aristocratic descent! We have supposed a beautiful woman to be married in those circumstances in which a great degree of protection is afforded her; but now suppose that the circumstances of the family change, and continue to change, for the worse for a succession of generations. The females in each generation will be called upon to contend with incident forces from which the parent was protected, and it is evident that the beauty will perish more and more each generation. During this downward progress two things must happen, either robuster qualities must be generated in these females as the beauty decays—in which case the tendency to disease may also vanish—or the tendency to disease will become intensified, and unless circumstances place the necessary protection within their reach, those females and their progeny will soon become extinct. It is during this downward progress towards extinction that we meet with the intenser forms of the scrofulous diathesis, associated with mere traces of beauty. What occurs when domesticated animals run wild is very instructive. It is the opinion of Mr. Darwin that many of our domesticated animals could not live in the wild state; and those animals which have become feral show a great tendency to revert to the characters of the aboriginal parent species, and if they were not to acquire some of those characteristics they would soon become extinct. Imagine our beautiful race-horses let loose upon a prairie to contend with their wild kindred. The probability is that they would die off in a few years; but there can be no doubt that if they lived for a few generations their beauty would soon vanish. That such altered circumstances would produce wonderful changes in the organization of a race-horse is so evident, that we almost feel shocked at the cruelty of the thought of abandoning one of these beautiful creatures unprotected; and yet this is what is so common around us with regard to man that it excites no surprise. Daily do we see the children of luxury and protection having to descend into the arena and struggle in open conflict with their robuster brethren.

Having endeavoured to remove objections, we now proceed to notice more particularly the agreements between the style of beauty already described and the scrofulous diathesis, and to connect these, as far as possible, with the phenomena of tuberculosis. One of the elements of this beauty is a white skin, and colour is in some mysterious manner associated with disease by what Mr. Darwin calls "correlated variability." In Addison's disease a wonderful change takes place in the colour of the skin; and although this is a marked instance, it is not the less true that the skin changes in colour in almost all diseases. These are instances in which changes of colour are produced as a concomitant of disease; but there are many cases in which a tendency to certain diseases is developed as an accompaniment of changes of colour. Mr. Darwin says, that "white terriers suffer most from the distemper, white chickens from a parasitic worm in their tracheæ, white pigs from scorching by the sun, and white cattle from flies; but the caterpillars of the silk-moth which yield white cocoons suffered in France less from the deadly parasitic fungus than those producing yellow silk. All the hogs, excepting those of a black colour, suffered severely in Virginia from eating the root of the *Lachnanthes tinctoria*. By two accounts, the *Hypericum crispum* in Sicily is poisonous to white sheep alone; their heads swell, their wool falls off, and they often die."* The relation which exists between the colouring matter and the powers of growth of both plants and animals is one of the most mysterious departments of physiology; but it is well known, however, that deficiency of colouring matter is associated generally with want of vigour, and this is one of the elements which tend to produce tubercle.

But not only must the skin be white to constitute beauty, but it must be thin and transparent so as to allow the red blood to be seen through the capillary walls in certain parts of the body. But in order that the red blood may be seen through the skin the capillary walls must also be thin, and when this tendency is developed in excess it leads to their bursting, and thus hæmorrhagic spots will result. But the mucous membranes being part of the epithelial structures they will participate in the delicacy of the skin, so that internal hæmorrhage

* "Plants and Animals under Domestication," by C. Darwin, vol. ii. p. 336.

will be apt to take place; and when this occurs in the lungs it is well known that it frequently leads to the formation of tubercle.

The delicacy of the skin consists partly in the layers of the cells of the epidermis being fewer; but the main cause of this delicacy is that the proportion of the protoplasm of the cells to the intercellular substance is increased, and this is equally true of the mucous membranes. But the skin is a passive expander, and the morphological characteristics of such tissues were found to be dense intercellular substance with a small amount of protoplasm, so that the skin in this instance is approaching in character to those tissues which are characterised by high reproductive activity. The consequence is that an irritation which would produce no appreciable effect upon denser epithileal structures, will cause the cells of such delicate structures as are under consideration at present to proliferate and to assume a high rate of reproductive activity. It is evident, therefore, that when a skin is rendered very thin and transparent it will exhibit a tendency to the production of local lesions of nutrition upon very slight causes, and by correlated variability the mucous membranes may be expected to participate in this delicacy, and the individual to exhibit a tendency to mucous irritation and discharges without any obvious cause. The close relation which exists between epithelial structures and the lymphatic glands has been already impressed upon us by manifold considerations; we need not, therefore, be surprised to find that when there is great tenderness of the former the latter are also rendered more susceptible. "It very frequently happens," says Niemeyer, "especially during childhood, that the lymphatic glands participate in this morbid tenderness, which, as a rule, is accompanied by augmentation of irritability, and a strong tendency to profuse cell-formation; while, in persons exempt from this tendency, the lymphatic glands neither enlarge, inflame, nor suppurate, excepting in cases of intense and malignant inflammation of the parts from which they derive their lymph, very trifling irritants and mild and innocent inflammation of the region whence the lymphatic vessels originate, suffice to excite the glands of individuals who are thus affected into an active production of new cells."* After what has already been said, it need scarcely be added that this cellular hyperplasia of the lymphatic glands from slight causes is the very condition which will lead more surely than any other to caseous degeneration of the gland, and to the whole of the co-existing and successive morbid conditions which constitute tuberculosis.

It is evident, therefore, that a delicate skin has a direct tendency to produce tuberculosis; but if this delicacy has been developed by sexual selection,

* Niemeyer's "Text-Book of Practical Medicine," vol. i. (1871), Transl., p. 211.

several other concomitant changes must have been produced. Perhaps there is no part of female beauty more fundamental than smoothness and delicacy of skin, since these are closely connected with the pleasures of touch, pleasures so essential to lovers. Burke* lays so much stress upon the beauty of smoothness, a quality so essential, he thinks, to beauty, that he cannot recollect anything beautiful without it; and although Price and Dugald Stewart disapprove of Burke's principles, both admit that they are strictly applicable to female beauty. It has already been remarked that this smoothness and delicacy of skin can only be maintained when it is protected from various incident forces, and now it must be noticed that the maintenance of this condition of skin will entail upon its possessor the adoption of a particular diet. It is well known that the adoption of a certain diet, comprised under the phrase "high living," is apt to make the bloom slip from the cheeks, and fasten upon the nose. The inexorable Teufelsdröckh asks whether Queen Elizabeth at a certain period "was red painted on the nose, and white painted on the cheek, as her tire-women, when from spleen and wrinkles she would no longer look in any glass, were wont to serve her?"† Now, if this particular visage were an ideal of beauty, and not of ugliness, it is possible that instead of being the result of high living and bad

* See "Mental and Moral Science," by Alex. Bain, M.A., p. 307.
† "Sartor Resartus," by Thomas Carlyle (People's edition), p. 33.

temper, exaggerated by paint, the tendency to the adoption of the high living would be generated as a necessary accompaniment of the physiognomy. The relation between the two might be brought about in either of two ways. A gathered experience might show that the adoption of a certain mode of living tended to develop and maintain the desirable qualities, or the adoption of the necessary diet might depend primarily upon the likes and dislikes of the individual, and those who had a preference for the proper diet to produce the desirable qualities might transmit both to their offspring. But in either case the result would be the same. Let us now endeavour to connect a particular mode of diet with the scrofulous diathesis. It is evident that if smoothness and delicacy of skin, along with gracefulness of form, is to be preserved, there must not be a large accumulation of fat, and hence prior to the age of puberty fat must not be taken too freely as an article of diet, otherwise the beauty becomes coarse. It is doubtful how the mode of excluding fatty substances from the food has been acquired, whether by the direct method of experience, or the indirect method of selection just mentioned. There can be no doubt that a few years ago young ladies were kept at boarding-schools upon very insufficient diet, a method which reconciled the superior's ideas of economy, with the desirable object of making the ladies refined. This points to the conclusion that fatty substances are frequently excluded from the diet of ladies by a gathered experience of the effects which it would produce. On the other hand it is well known that those who have a tendency to tubercle have a great dislike to fatty substances, and that a peculiar dyspepsia is frequently present which makes these articles of diet difficult of digestion, and this points to the conclusion that the exclusion of fat from the diet is frequently owing to the likes and dislikes of the individual. But in whatever way this exclusion is brought about the result is the same; the standard ideal of beauty may be to a certain extent secured, but the tendency to disease is intensified.

It has been seen that beauty is advantageous to the female chiefly in relation to reproduction, but that it entails upon her certain concomitant changes which are disadvantageous; let us now balance these against each other, in order to see in what direction each of them may be expected to act. Beauty is of advantage to the female principally in enabling her to select a partner who is in a position to extend to her the comfort and protection similar to that which must have been expended in developing the beauty, and which must now be expended in maintaining it. It also follows that the beauty of the mother will have a tendency to secure a similar degree of comfort and protection to her progeny. In other words, beauty and delicacy of constitution are developed together; and while the latter renders the individual less capable of resisting incident forces, the former tends to secure to its possessor a position in which the reaction to incident forces is rendered less necessary. So far, then, there is a pretty even balance maintained between the advantages and disadvantages of beauty.

But a beautiful woman is frequently asked, and generally consents, to be married at a young age, and this circumstance brings along with it a redistribution of profit and loss. In the first place, there is good statistical proof to show that there is greater mortality amongst the children after early marriages than when the parent arrives at full maturity, say from twenty-two to twenty-five years of age. But if a larger proportion of the progeny die in childhood a larger proportion must be born, otherwise the progeny of those who marry young cannot compete upon equal terms with the progeny of those who defer that important step a few years longer. It is evident that the rate of multiplication is largely dependent upon the age at which reproduction commences; so that those who marry young will have a great advantage so far as the number of progeny is concerned; but we have seen that this advantage is largely counterbalanced by a higher death rate. But those who survive to propagate the variety will probably be more delicate than the children of later marriages; and it is well known that when reproduction begins growth is checked, and when it begins at an early age this of itself tends to gender delicacy. But beauty tends both to produce delicacy and to foster early marriages, so that these act and react on each other; and the consequence is that, other things being equal, as the beauty increases the delicacy of constitution becomes greater, and as reproduction begins at an early age the death rate amongst the progeny must increase, and as the death rate increases larger and larger families must be produced, or the progeny of such must soon become extinct. Females, therefore, who have the scrofulous diathesis may be expected to have, as a rule, large families. But the production of new individuals entails cost upon the parent, and the delicate parents of which we are speaking at present can ill afford to expend much upon others. "The whole expense of establishing each new individual includes," says Mr. Herbert Spencer, "first the forces latent in the substance composing it when born or hatched; second, the forces latent in the prepared nutriment afterwards supplied; and third, the forces expended in feeding and protecting it."* The possession of beauty, however, frequently enables its possessors to marry into such circumstances as will render it possible for them to dispense to a great extent with the second and third mode of expenditure in

* "Principles of Biology," by Mr. Herbert Spencer, vol. ii. p. 409.

the rearing of offspring; and when these functions are devolved upon others for a series of generations, the mammæ become atrophied, and the mother may be quite unable to resume the function of suckling her children.

It has already been mentioned that on an average those who are subject to the scrofulous diathesis are not on a level with the generality of healthy individuals in intellectual capacity; but even as regards mental characteristics we may expect to find that they have certain compensating advantages. All the advantages which we have found them to possess hitherto have been closely related to reproduction; and we may now expect that those mental characteristics which cluster around the production of offspring will be highly developed in those individuals. This subject is so wide that I can only briefly allude to it. If the scrofulous diathesis has been developed as a concomitant of a certain style of beauty by sexual selection, it may be expected that many of those who exhibit the tubercular tendency will be characterised by great warmth of affections, and by the possession of a vivid imagination, qualities which in civilized societies are of signal advantage to their possessors in the struggle for existence.

If my object were to write a complete article on scrofula and tubercle, several other questions would require to be discussed; but at present I shall only allude to one other point. According to the hypothesis advanced here the scrofulous diathesis was developed in the female, but since it appears at an early age it is equally transmitted to both sexes. In this respect it is unlike gout, which is, on the whole, confined to the male sex; but the latter disease appears at a late period of life, and it is confined to the sex in which it is first developed.

CHAPTER VIII.

CLASSIFICATION AND CONCLUSION.

In the following remarks I do not intend to enter upon a systematic discussion of classification, but shall take up a few points upon which the principles advanced here appear to cast a considerable degree of light. My primary object is, not to advance classification, but to give additional validity to the principles I have advocated, by showing that they may be usefully applied in systematizing our knowledge of disease. The principal subject of which I shall treat at present is the specificity of disease, and this will lead to the discussion of collateral problems. The experiments of M. Villemin upon the inoculability of tubercle brought into prominence the question whether tuberculosis was or was not a specific disease, and to this various answers have been given by different authors. But there is a prior question which should be determined before addressing ourselves to the specificity or otherwise of a particular disease, and that is, what constitutes a specific disease in any case?

Sir James Paget understands by specific, something distinct from a common or simple disease. "The specific characters of any disease," he says, "whether syphilis or hydrophobia, gout or rheumatism, typhus, small-pox, or any other, are those in which it constantly deviates from the characters of a common or simple disease of the same general kind."* The inflammation which arises in a healthy individual, as the result of ordinary changes in the environment, is regarded as a simple disease, and when an inflammation deviates from this form it is regarded according to this definition as a specific disease. If this, however, were the meaning generally attached to "specific," the discussion which arose from M. Villemin's experiments would soon be settled. But it is very evident that something more than this was meant by "specific" in that controversy, and that the term was employed in a much more restricted signification. Let us inquire if specific in relation to disease means the same thing as it does in biology. "We may conveniently speak of these two senses, or aspects of 'species,'" says Professor Huxley, "the one as morphological, the other as physiological. Regarded from the former point of view, a species is nothing more than a kind of animal or plant, which is distinctly definable from all others by certain constant, and not merely sexual, morphological peculiarities."† If,

* Paget's "Lectures on Surgical Pathology," edited by Professor Turner, p. 353.

† "Lay Sermons, Addresses, and Reviews," by T. H. Huxley, LL.D., F.R.S., p. 284.

therefore, a specific disease corresponds to Professor Huxley's definition of morphological species, the only question which can arise is whether tuberculosis is capable of being distinctly definable from other diseases. But if this were the meaning of specific in pathology, it is evident that common inflammation may be as specific as any other disease. If gouty, tubercular, and rheumatic inflammation are capable of being defined so as to exclude common inflammation, it follows that the latter can be defined from the former, and is as much entitled to be regarded as "specific" in this sense of the term. This shows that "specific" in pathology does not correspond to the definition which Professor Huxley gives of morphological species.

Let us now turn to his definition of physiological species. "Most physiologists," he says, "consider species to be definable as 'the offspring of a single primitive stock.'"* According to this definition, community of descent is the most important element in determining physiological species, and there can be no doubt that this is a very important factor in the meaning of "specific" as employed in pathology. If M. Villemin—when he had shown that tubercle was inoculable, and had brought forward arguments to show that all the phenomena of tuberculosis could be explained on the supposition that it was a zymotic disease—could only have proved

* "Lay Sermons, Addresses, and Reviews," by T. H. Huxley, LL.D., F.R.S., p. 289.

that no other assumption was possible, every one would have admitted that this disease was specific. When, on the other hand, Drs. Sanderson and Fox proved that tubercle could be traumatically produced, it was at once regarded as decisive against this supposition. But the definition of physiological species only corresponds to what naturalists in the present day regard as good species, when considerable latitude is allowed in interpreting the meaning of a primitive stock; for, as Professor Huxley says, "the primitiveness of the supposed stock is not only an hypothesis, but one which has not a shadow of foundation if by 'primitive' be meant 'independent of any other living being.'"* All that a naturalist in the present day means by species is, that a group of animals or plants is distinctly definable by characters which have remained constant for a long period. "A species," says Mr. Wallace, "is an organic form which, for periods of great and indefinite length, as compared with the duration of human life, fluctuates only within narrow limits." †

Let us now inquire how far the zymotic diseases are entitled to be regarded as specific, according to this modified definition of physiological species; and in order to facilitate the inquiry, we must dwell for a moment upon the principles at work in the formation of biological species. After defining species as just quoted, Mr. Wallace continues: "But

* "Lay Sermons, Addresses, and Reviews," p. 289.

† See *Nature*, Nov. 17, 1870, p. 49.

the spontaneous tendency to variation is altogether antagonistic to such comparative stability, and would, if unchecked, entirely destroy all species. Abolish, if possible, selection and survival of the fittest, so that every spontaneous variation should survive in equal proportion with all others, and the result must inevitably be an endless variety of *unstable forms*, no one of which would answer to what we mean by the word species. No other cause but selection has yet been discovered capable of perpetuating and giving stability to some forms, and causing the disappearance of hosts of others." * We shall now endeavour to show that, on the supposition that the zymotic diseases have resulted from successive variations of healthy tissues, the principle of "survival of the fittest" must have acted in such a manner as to give stability to some forms, and to lead to the extinction of others; and that the theory of the formation of organic species by natural selection is applicable, in its main principles, to the formation of the different groups of the zymotic diseases.

According to the theory advanced here, syphilis has been developed from an ordinary pustule by successive slight variations ; and, as already noticed, in proportion as the morbid product deviates further and further from normal structures, it acquires compensating properties which ensure self-propagation. Suppose, now, that the discharge from the central

* *Nature*, Nov. 17, 1870.

portion of the ulcer left by the bursting of an ordinary pustule were to deviate further from the healthy tissues than that from the margins, the former would acquire higher powers of contagion, and would be more readily grafted upon a second individual. And if the discharge from the centre and that from the margins were grafted at the same time upon the same individual the former would, in all probability, be prepotent over the second; so that its characteristics would be impressed upon the morbid process initiated, and the grafts subsequently produced upon the second would all be similar to the grafts from the centre of the ulcer on the first individual; and the marginal grafts of the first would have no representatives on the second ulcer. That this process of selection does really occur is rendered very probable from what is known to take place in the fertilization of plants. "It is well known," says Mr. Darwin, "that if pollen of a distinct species be placed on the stigma of a flower, and its own pollen be afterwards, even after a considerable interval of time, placed on the same stigma, its action is so strongly prepotent that it generally annihilates the effect of the foreign pollen."* And a similar process of selection may be presumed to exist in the case of the zymotic diseases. Suppose, for instance, that an individual is inoculated by a mixture of vaccine lymph and

* "Plants and Animals under Domestication," by C. Darwin, vol. ii. p. 181.

small-pox, it is almost certain that the particles of the latter would alone leave successors, and that so far as this individual is concerned, the former would die out. Here, then, we find a process of selection and of extinction going on hand in hand; and if syphilis has been formed by successive slight variations from an ordinary pustule, we may expect to find that there is a distinct line of demarcation formed between the former and the latter, by the extermination of intermediate forms.

It may be supposed, however, since the inoculable and the contagious morbid products differ from each other only in degree, that intermediate forms will be found between syphilis and the contagious diseases by which the one graduates into the other; but natural selection places a barrier between them which cannot be passed. If syphilis acted with as much rapidity as small-pox, either the disturbance produced would be so great as to render sexual congress impossible, or if possible the danger would be so manifest that it could be easily avoided; so that the disease would fail to be propagated by inoculation. If, on the other hand, along with rapidity of action the morbid product of syphilis were to acquire high contagious properties, as in all probability it would do, the disease would merge into a contagious disease, but could not maintain an intermediate form. Here, also, selection and extinction go hand in hand, and a definable margin must be produced between syphilis and the contagious diseases. Syphilis must, therefore, be a disease which is distinctly definable from the general diseases on the one hand, and from the highly contagious diseases on the other; and this deductive conclusion is amply verified by reference to observed facts.

It must now be shown that there is a principle at work which will differentiate the various contagious diseases from each other. The contagium particles will differ in properties according to the kind of epithelial structure from which they have originally descended; so that the most fundamental differences between the properties of the contagium particles may be expected to correspond to the three great tracts into which the epithelial tissues of the body may be divided; namely, the skin, the respiratory, and the digestive mucous membranes. Each contagium particle will show its descent by producing its primary action upon the tract of tissue from which it has descended; so that, corresponding to the three great divisions of the epithelial structures of the body, there will be three groups of the zymotic diseases—the epidermic, the pulmonary, and the intestinal. These groups must be definable from each other, but they derive their distinctness from that of the healthy parent tissues from which they have descended, and not from the action of a special principle producing extermination of intermediate forms during their descent. It is not necessary for me to trace the manner in which the physiological tissues may have become differentiated; but I will now endeavour to sketch a probable method by which the various contagious diseases within each zymotic group may have become differentiated into distinctly definable diseases.

It is not difficult to show that small-pox and scarlet fever, for instance, may have descended by successive slight variations from a disease which was different from both, but which presented characters intermediate between them. Suppose that a contagium particle from this parent disease had become slightly modified, so as to enable it to infect individuals who had already suffered from an attack of the primary disease. The descendants of this particle would have an advantage in the struggle for existence, since the number of the population in which they could take root would be more numerous than those which could be affected by the unmodified particles. The modified disease would, therefore, gain a temporary advantage over the primary disease; but after a time, when a large part of the community had become affected by the former, their chances of propagation would be equalized. But the more these two sets of particles would deviate from each other in their properties, the greater the chance each would have of being propagated, since the immunity which an attack of the one would afford against an attack of the other would become less and less. But suppose that this process of divergence had begun before the primary disease had varied so far from healthy tissues as to have become a highly contagious disease, the acquisition of higher and higher powers of contagion might have gone on hand in hand with the process of divergence just sketched; so that the parent disease would have to contend with two more vigorous descendants, while from the intermediate position of the former an attack of either of the latter would afford a certain degree of immunity to the individual against it. The individuals who could be affected by the parent disease would become less and less numerous unless it diverged in another direction, which would enable it to compete with its descendants upon more equal terms; but in the latter case it would cease to hold an intermediate position between them, so that practically there would be extinction of the intermediate, that is the primary form. Here, then, we find at work in the formation of the zymotic diseases principles which would lead to progression, continued divergence, and extinction of intermediate forms—the forces which are operative in the production of biological species.* It will afterwards be shown that a similar principle is operative in exterminating the intermediate forms between the contagious and inherited diseases; so that it is highly probable that the characters of the zymotic diseases, such as small-pox, scarlet fever, measles,

* See "Contributions to the Theory of Natural Selection," by A. R. Wallace (1870), p. 26, *et seq.*

&c., are fixed by natural selection, and that each will "breed as true" as the species of horse and ass. The hypotheses of the heterogenesis, and of the convertibility of the zymotic diseases are, therefore, as inapplicable to those diseases as they would be to good biological species; at any rate such hypotheses do not deserve much attention so long as their evidential claim consists of an appeal to our ignorance.

But it may be said that if the attainment of high contagious properties is an advantage to a disease graft in struggling with its less endowed competitors, there would be no limit to the acquisition of these properties. A limit is, however, soon reached beyond which it is not an advantage to a disease graft to acquire higher contagious properties. If, for instance, scarlet fever were as rapidly fatal as it is in some cases, no time would be given for the body to cast off from its surface the innumerable grafts which are always detached when the disease runs through the different stages of its evolution. We may say of these diseases, as Friar Laurence says of "delights" on the eve of the marriage of Romeo and Juliet, "These violent 'diseases' have violent ends, and in their triumph die." In this manner contagium particles which produce a less violent action will have a greater chance of propagation; and, as previously shown, rapidity of action and high degree of contagiousness are bound together by a causal connection; so that a medium degree of contagiousness will be most advantageous in propagating the disease. And owing to the precautions taken by instructed persons against the spread of contagion, it frequently happens that a mild case is more dangerous to the community than a more violent one; since all precautions are apt to be neglected in the former case. This is only an instance of what frequently happens with biological species; that one which has high powers of self-maintenance calls into action new forces with which it has to contend. These remarks render it evident that the zymotic diseases can be divided into good species in the strictest sense in which the term is employed by any naturalist in the present day. They are distinctly definable from each other, and exhibit community of descent for long periods; and these are the elements which constitute the strictest definition of biological species. We shall now proceed to examine how far the same principles are operative with regard to other diseases.

The point at which the inherited local and the contagious diseases are most likely to meet, and to graduate into each other, is when the former become highly contagious in the individual, as in the more rapidly growing cancers. It will be seen, however, that barriers exist which render it impossible that these diseases should be connected together by a set of intermediate forms. The differences between the morphological characteristics of the pathological products of cancer and of contagious matter have already been dwelt upon, and arguments have been advanced to show that, if the units of the former multiplied as fast as those of the latter, cancer would be as rapid in its action as the contagious diseases. Let us now suppose that the units of cancer were to multiply as fast as those of the virus of small-pox, and let us also suppose, in the first place, that with this high multiplication of units the disease ceases to be always fatal. The disease would now run a very rapid course, and, after having gone through its evolution, the individual would be so modified as to be insusceptible of a second attack. The consequence would be that the physical modification of the organism which gives the immunity from the second attack would be transmitted to posterity; but the disease would fail to be inherited, so that if it did not along with rapidity of action also acquire highly contagious properties, the disease would die out, and if it did acquire high contagious properties it could not be distinguished from the zymotic diseases; but in either event it could not maintain an intermediate form. Suppose, now, that the disease becomes very rapid in its action, and yet continues to be always fatal. As previously shown, every increment of deviation which takes place in the genesis of cancer from the healthy tissues must be accompanied by changes which will enable the disease to leave its impression upon the organism in such a manner as to become inherited; and if the disease is propagated by inheritance, it must go through its evolution by slow and gradual steps, otherwise no progeny is left to whom the peculiarity is bequeathed. If cancer passed through the different stages of its evolution as quickly as small-pox, it would cease to be inherited ; so that, here again, if, along with rapidity of action the disease did not also acquire contagious properties, it would die out, and if it did acquire these properties it would become a zymotic disease, but could not maintain an intermediate form. It is manifest, therefore, that there is a principle at work which will lead to the extinction of all the intermediate forms between cancer and the zymotic diseases. Let us now see whether this principle is operative in separating the locally inherited from the general diseases, and the former into groups which are distinctly definable from each other.

It may be noticed that tumours and outgrowths will have the degree of distinctness which the healthy tissues from which they are derived, or the parts of the body in which they become developed, give them. In this manner a cartilaginous will be as distinctly definable from an epithelial growth as these tissues, when healthy, are from each other; and a cartilaginous of the parotid will be as distinctly definable from one of the mammary gland as these organs are from each other; but in these instances the capability of being defined has been originally acquired by the physiological tissues. But our object, at present, is to trace the operation of a principle which would separate the pathological tissues which have descended from the same physiological tissue from each other, and from the healthy tissues from which they have descended. A careful survey of the facts will, I think, convince any one that, in so far as the inherited local diseases have been traced out here, no such principle exists. A callosity or a corn can be cultivated upon a healthy individual, and it is probable that a wart might also be cultivated by a definite combination of changes in the environment. The latter, however, occurs in some families more frequently than in others, which shows that its propagation is probably aided by inheritance; but it is difficult to say where the operation of this principle begins, nor is it possible to draw any sharp line of distinction between the growths which may be produced upon the healthy by irritation and those which require a certain aid from inheritance. The one kind of growth graduates into the other. And if we proceed from the simple growths to those termed malignant, we shall find the same graduated formation. It is impossible to draw a sharp line of demarcation to show where innocency ends and malignancy begins. Every increment of deviation from healthy tissue must act upon the organism in such a manner as to become inherited; but there is no principle at work which would give a decided advantage to one deviation over another, and which would enable it to gain in the struggle for existence. There is a certain degree of extermination going on along with every deviation from healthy structures. What are termed innocent tumours may be inherited at any age; but if a malignant disease appears before the age of puberty, it becomes extinct along with the individual; so that, if a tumour does not become active till after the age of puberty (or in childhood by transmission), there is presumptive evidence that it is closely allied, if not actually a malignant disease. But this character is not so constant as to give a good specific distinction, and it may be concluded that the only difference between the simple and malignant tumours is one of degree.

And if it is impossible to draw a distinct line of demarcation between the different local outgrowths which have descended from the same physiological tissue, much less can community of descent be predicated of them. It is evident that community of descent cannot be predicated of the outgrowths which can be cultivated upon a healthy individual, nor can it be predicated for a long succession of generations of the simple growths which require the aid of inheritance for their propagation, since it is possible that, although these cannot be cultivated upon a healthy individual, they may have been produced by irritation continued through a few generations. With regard to the malignant diseases, it is possible that the milder forms might be developed by long-continued irritation upon one who has only an inherited tendency to an innocent outgrowth, while the severer forms might be generated in a similar manner in a few generations. It is also probable that a reverse process is going on, and that some of those who have an inherited tendency to the growth of the severer forms of cancer, may, under favourable circumstances, only generate the milder forms, and that under similar favourable circumstances those who have an inherited tendency to the milder forms of cancer may only generate innocent growths. In the inherited local diseases, therefore, a double process is going on; by the first, individuals who have an inherited tendency to innocent growths are recruited from healthy individuals—those which develop the milder forms of malignant disease are recruited from individuals who have only an inherited tendency to innocent productions, and those which develop the severer forms from individuals who only inherit the tendency to the formation of the milder malignant diseases. By the second process, individuals who have an inherited tendency to the severer forms of malignant disease may, under favourable circumstances, only develop the less severe forms; those who inherit a tendency to the milder forms of malignant disease only innocent growths; and those who inherit a tendency to the formation of innocent growths may not develop any growth at all. We see, therefore, that the cancerous tendency may possibly be developed in a few generations from the healthy, while, under other circumstances, the tendency to it in a particular family may be lost in a few generations, so that it is not possible to predicate community of descent of any two cases of cancer which may present themselves. To sum up, then, the local inherited diseases are marked off from the zymotic diseases by an impassable barrier; but there is no distinct boundary between them and the general diseases; and the only distinctly definable boundaries which they present amongst themselves are derived from the physiological tissues, and the only pathological classification which they admit of is a serial one.

But the most difficult part of our task is to deal with the classification of the inherited diathetic diseases. The subject is difficult in itself, and is rendered still more so because it has only been treated in the foregoing pages in a very imperfect manner. It is a common remark that no two individuals are alike; and constitutional peculiarities, or what are called individual idiosyncrasies, manifest themselves in the diseased as well as in the healthy states of the individual. A great many of these peculiarities and idiosyncrasies are so slight, and depend upon such intricate causes, that they do not present sufficiently marked and constant characteristics for classification. But when a definite combination of causes acts upon a succession of individuals for long generations, a definite modification will be produced, and in proportion to the definiteness of the causes which have produced the modification, will be the definable characteristics of the modification itself. For instance, amongst the upper classes in this country a certain mode of living and diet has been fashionable for generations, and is still adopted to a certain extent, so that we may expect to find amongst the representatives of those classes a modification of constitution corresponding in definiteness to the constancy of the causes which gave rise to it. An endeavour was made to prove that the scrofulous constitution was frequently generated as a concomitant of a certain style of beauty; and if the connection between the development of beauty and of a vulnerable constitution is real, we may expect to find a group of individuals who have the scrofulous tendency which is as distinctly definable as, but no more so than, the style of beauty of which it is a concomitant. But not only are the various features which constitute beauty eminently variable, but the scrofulous tendency is also developed in other ways; and a tubercular tendency may even be developed in a healthy individual by a combination of circumstances, so that it is not probable that the scrofulous diathesis can be defined by well-marked and constant characters. It is possible, however, that constitutions which have been modified by such different processes as sexual selection, and a particular diet, may, when extreme cases are compared, present well-marked differences, which give good definable characters. But it is not often that these causes will operate independently of each other; indeed, both influences frequently combine, not probably in the same individual, but through the parents, so that intermediate forms will be found between the two diatheses; nor can we trace any principle at work which would tend to exterminate these forms. Although, therefore, we may be able to distinguish constitutions which have tendencies to certain diseases, and may probably be able to do this more perfectly as the diseases of families for generations are better recorded, yet these diatheses can never be marked off from each other by characters so marked and constant as we find in biological species. Indeed, this proposition is so obvious that the proof of it is superfluous, for it only requires to be considered that a diathesis is not so definite as a variety, and it must, therefore, be much less definite than a species.

But if the diatheses do not exhibit any clear line of demarcation, it is possible that the diseases which they tend to develop may be readily definable from each other. These diseases will have the amount of definiteness which they derive from the diatheses; they will also have another degree of definiteness, from the circumstances in which they are developed, and a third degree from the tissues, which become simultaneously and successively affected in the course of the disease. We shall now proceed to examine how far tubercle may be expected to present good definable characters; and our remarks upon this part of the subject must necessarily be almost entirely limited to this disease, since it is the only one of the class which has been discussed in the previous pages.

But before proceeding to examine whether there is a distinct line of demarcation between the general diseases and tubercle, it is necessary at first to make a few remarks with regard to the general diseases themselves. When an irritant is applied to a certain part of the body, and either hypertrophy or hyperplasia results, it is evident that the distinction between these conditions is merely arbitrary, and that the one graduates into the other. Again, between the slight degree of hyperplasia which subsides into health in a few days, and the higher form which is followed by degeneration, and between that still higher hyperplasia which leads to the formation of pus, there is no difference but that of degree. The one form passes into the other by insensible gradations, and there is no principle at work to produce extinction of the intermediate forms. It is true that pus and hyperplasia may be distinguished from each other; but they are connected by other morbid conditions, presenting a chain of unbroken affinities; and the same thing is equally true of the various forms of degeneration.

But the degeneration which concerns us most here is the cheesy metamorphosis, a condition which may be produced in several ways; but our remarks will be confined to two of these modes. As previously remarked, the cheesy metamorphosis may be produced through the formation of pus, and subsequent reabsorption of its fluid, or through fibroid degeneration. But when fibroid degeneration takes place, and pus is formed in similar tissues, there is no difference between them but one of degree; and as there is no principle at work to produce extinction, they must be connected with each other by a set of intermediate morbid conditions; so that the cheesy metamorphosis which results from pus and that from fibroid degeneration are not essentially different from each other. One element of Dr. Sanderson's definition of tubercle is, that it must be formed in adenoid tissue; and therefore when a cheesy mass is formed in any other tissue it is not tubercle. But although this is a highly important distinction, it derives its classificatory value from the physiological tissues, and it is not produced by a principle analogous to natural selection acting upon the morbid product itself. But adenoid tissue is liable to inflammatory action, and it is very common for lymphatic glands to undergo the cheesy metamorphosis as the result of this action; and although the extreme cases of cheesy products, which are produced through the intervention of pus, and those through fibroid degeneration, may be distinguished from each other, yet it is evident from what has been said that they must be connected with each other by intermediate forms, and that all these products are closely allied, nor are they likely to differ much from each other in properties. These deductions are amply verified by reference to observed facts. In some of the experiments of Drs. Fox and Sanderson the primary lesion, and the morbid process in the lymphatic glands more immediately related with it, were more similar to pyæmia than to tubercle, while true tubercle was found in the tissues surrounding the primary lesion, and in the organs more remotely connected with it. At any rate, there can be no doubt that the cheesy products found in some cases at the seat of the primary lesion, and also in the nearest lymphatic glands, were the result of inflammatory action and not of fibroid degeneration; so that one and the same cause may give rise to inflammatory and to tubercular cheesy masses, and a graduated series of effects presents itself, in which it is difficult to say where inflammation ends and tubercle begins. And in the morbid products which occur in the lymphatics of man, intermediate forms between inflammatory and tubercular products may be traced in such structures as the leukemic lymphoma of leucocythæmia, the typhoid lymphoma of typhoid fever, and the ordinary scrofulous enlargement of glands. Tubercle has, therefore, close affinities with the general diseases, with which it is connected by a series of formations so gradual that it is difficult to distinguish where the one begins and the other ends. But when a disease becomes entirely dependent upon inoculation or contagion for its propagation, there is a principle in action which exterminates the intermediate forms; hence it may be concluded that even if tubercle may occasionally be propagated by inoculation (and the same may be said of pus), it is not entirely dependent upon this means for its production. And if there is not a distinct line of demarcation between tubercle and the general diseases, much less can community of descent be attributed to all cases of the former. Suppose that all cases of tubercle were found in those individuals in whom the scrofulous diathesis had become developed by sexual selection, it would be impossible to predicate community of descent for a long series of generations of any two cases. It is evident that there are causes at work around us which might strengthen the constitutions of the offspring of those vulnerable individuals, so that the tendency to tubercle might be eliminated in a comparatively short time in a succession of individuals; while, on the other hand, the offspring of individuals who have no tubercular tendency, might have the necessary delicacy of constitution inbred into them in a few generations; so, even on this extreme supposition, it would be impossible to predicate community of descent of cases of tubercle for many generations. But the tendency to tubercle is not always developed by the indirect action of sexual selection, and tuberculosis may occur in a previously healthy individual by the long-continued action of a combination of causes favourable to its production. Tubercle may, therefore, be formed without any aid from the hereditary tendency, the only difference being that when the latter is present the disease may be called into activity by causes so slight that it appears to arise spontaneously; while, when there is no such tendency, it requires a combination of causes very unfavourable to the health of the individual to generate the disease. We see, therefore, that neither with regard to community of descent, nor to its being distinctly definable from other diseases, is tubercle entitled to be regarded as specific in the strict sense in which the term is employed at present.

But although tubercle is not entitled to be considered specific in the strictest sense of the term, it is not meant that no useful purpose is served by giving to it as distinct a definition as the case admits of; and, indeed, a great many good zoological species are not entitled to be ranked as such according to the definition of physiological species. Speaking of the important part which extinction has played in widening the intervals between the several groups in each class, Mr. Darwin says: "There has been less entire extinction of the forms of life which once connected fishes with batrachians. There has been still less in some other classes, as in that of the *Crustacea*, for here the most wonderfully diverse forms are still tied together by a long, but broken, chain of affinities."* Amongst the *Crustacea*, there-

* "The Origin of Species," by C. Darwin, p. 431.

fore, natural selection appears to have acted very imperfectly in procuring extinction of intermediate forms, so that the different groups of this class will not be distinctly definable from each other; but no naturalist will deny that these groups are entitled to rank as species, although the meaning of species must, in this instance, be extended to what Professor Huxley calls morphological; and when a natural classification of disease is more fully adopted, this will be the sense in which the term species and its cognates will be employed in pathology.

It is amongst the inherited diathetic diseases that the most requires to be done relative to the classification of disease; but I can only allude very briefly to this part of the subject. Dr. T. Clifford Allbutt, in his Introductory Address delivered at the Leeds Royal School of Medicine (Session 1871-72), made some capital remarks upon classification. "But I think," he says, "more might be done to make some classification of morbid tendencies, and this must be done chiefly by the accurate records, not only of single cases, and not only by records of the course of individual lives, but by careful summing up of all the maladies as they occur in a series of persons related in blood. We should thus be enabled, perhaps, to establish serial affinities between diseases now unsuspected of having anything in common, and so diseases might, for the first time in the world, be classified on something like a natural system."* In an individual in whom the scrofulous

* See *Lancet* (Oct. 14th, 1871), p. 533.

diathesis is well developed, the first deviation from health may consist of a moist eruption on the scalp: this is followed by enlargement and subsequent caseation of the nearest lymphatic glands; and from these the process may extend to the lungs, liver, serous membranes, and bones; and the children of parents in whom the tendency to tubercle is strongly developed, or in whom there is active tubercular disease, are apt to be affected by epilepsy and insanity. Here, then, there is a group of diseases, apparently widely different from each other, yet all arising from a deviation so trifling as frequently to escape notice. Other groups might be formed in this manner, of what now appear to be independent diseases, but which may be as closely dependent upon each other, and all of them upon one primary deviation, as the tertiary lesions of syphilis are upon the secondary, and both of them upon the primary one. For a long time the affection of the toe in gout was the only feature of the disease which attracted attention; but it is now known that this affection is closely connected with a certain form of bronchitis, a peculiar dyspepsia, scaly skin diseases, arterial degenerations, and various other affections. The successions of these affections may hereafter be unravelled in such a manner as to enable us to foretell what affection will follow a particular lesion as surely as we can now foretell the appearance of the general eruption on a certain day in inoculated small-pox. I cannot think of anything which would be more useful in enabling us to unravel the intricate successions of disease than good family records. If such records were kept, the medical attendant might note down all the cases of disease which occurred in the same family. These records need not be voluminous, but at the same time a strict account should be kept of what are generally considered trifling diseases, such as slight eruptions on the skin, catarrh, and diarrhœa. Such family records would, in a few generations, afford more valuable information than all the records of isolated cases of disease with which our present hospital reports and medical press are teeming. And it might be possible after a time to form all the inherited diathetic diseases into groups, which would be as distinctly definable as many good morphological species in biology. But if the term "species" and its cognates be extended to these groups of diathetic diseases, there can be no valid reason for withholding them from groups of diseases which arise from general causes; and if a good natural classification of disease were attained they would have this extension given to them, and all the lowest groups of disease which are practically definable would be entitled to rank as species. At present, however, there is a disposition to restrict the meaning of the term somewhat further than this, although no one would probably be inclined to give it the narrow denotation of physiological species. In the mean time it is better to define the term in accordance with its generally received acceptation in current medical literature. We have seen that the zymotic diseases would be entitled to rank as specific according to the strictest definition of the term, and it would be almost impossible to exclude the parasitic diseases from the same category; since both deductive reasoning and observation inform us that a specifically definable parasite will give rise to a distinctly definable disease. Parasitic diseases, therefore, can scarcely be excluded from the specific diseases, however strictly the term may be defined. But there are other diseases caused by the absorption of chemical agents into the organism, and these by general consent are allowed to be specific. We have seen that a minute drop of the poison from the bite of a gall insect will produce a local growth on the oak, and that the growth which is produced by each species of the insect possesses good specific characters; and similarly certain chemical substances absorbed into the organism give rise to diseases which are distinctly definable from each other. It is more in accordance with common usage, therefore, to call a disease specific when a distinctly definable agent in the environment gives rise to a distinctly definable disease. M. Villemin says, "Nous venons de voir que des substances bien connues, administrées dans un but d'experimentation ou de traitment, ont chacune une action propre, manifestée par des réactions ayant une évolution déterminée et surtout un siége spécial. Les agents morbifiques que nous connaissons n'agissent pas differemment; en sorte qu'en admettant que chaque agent spécifique détermine une réaction particulière, nous sommes en droit de supposer la proposition inverse, à savoir: qu'à toute réaction spécifique correspond un agent spécial."*

Before bringing this little work to a close, it will be as well to place the leading points of the argument as succinctly as possible before the reader. In the first place, we noticed the influence which the *Germ Theory* of fermentation and of putrefaction has exercised upon the theory of disease; an influence rendered more definite, in the case of the zymotic diseases, by the discovery that the active part of contagious matter consists of particles very similar, not only in physical and chemical characters, but also in their vital movements, to the organisms found in putrefactive fluids. An endeavour was then made to prove that the evidence from which it is inferred that contagium particles are independent organisms is defective; and this conclusion being hypothetical it is equally legitimate to assume that these particles are only modified portions of the individual from which they were detached. This second assumption being equally insusceptible of experimental proof with the first, we were led to resort to the indirect proof from analogy, and to that verification which any assumption derives from leading to true results.

As the first step in this indirect proof, it was shown that there was a close analogy between the mode of formation and detachment of the contagious

* "Études sur la Tuberculose," par J. A. Villemin (1868), p. 12.

particles (according to the supposition), and that of the sperm-cell in the process of reproduction; and a still closer analogy was found between the collision of a contagium particle with a healthy body, and what occurs when a portion of one individual is grafted upon another. A striking confirmation of the reality of this analogy was found in the fact that certain well-known phenomena which occur in the stock after grafting upon it a slightly different scion are so similar to the phenomena presented by the contagious diseases, that the former cannot be expressed in general terms without embracing the latter. Having shown the close analogy between the genesis of contagious diseases, according to the supposition, and the genesis of new individuals, and the almost identity of the former with that modification of existing individuals produced by grafting, the starting point of our theory must necessarily be from the fundamental laws which govern reproduction. In the prosecution of this design, Mr. Darwin's hypothesis of Pangenesis, by means of which he explains the various forms of genesis, inheritance, reversion, and the phenomena of budding and grafting, was adopted; because an hypothesis by which these great operations carried out through the whole biological series can be explained, is much more deserving of credit than one specially constructed for the explanation of a more limited set of phenomena.

After noticing the analogy between the contagium particles and one of the elements concerned in reproduction, it was necessary to consider the individual from which the former were detached; and also the one upon whom they were afterwards to be grafted. This introduced to our notice in a special manner the correlative couple, health and disease; and before proceeding further it was necessary to make a few general remarks upon these conditions, and to discuss specially those morbid processes which arise in the healthy organism as the result of ordinary changes in the environment, in order to arrive at some fundamental conceptions with regard to disease. As the result of this part of the inquiry it was found that contagiousness was a fundamental property of the morbid products of the general diseases, and that it was also a property of healthy tissues in reacting upon these morbid conditions. The laws of contagion, as manifested by the general diseases, were specified, and it was afterwards shown that these laws were deductions from the fundamental assumption of Pangenesis; so that the consilience of the evidence at this point was an additional confirmation of the whole of the previous argumentation.

This part of the inquiry being concluded, we proceeded to apply the laws of contagion, as derived from the consideration of the general diseases; and the hypothesis of Pangenesis to the explanation of the phenomena of the zymotic diseases; and it was found that the principal features presented by them could be deduced from these, without having recourse to any other assumptions except minor ones, in working out the details, which did not affect the general principles. So far, then, the results obtained justified our primary assumption that a contagium particle is only alive in the sense of being recently detached from a living body; and the discussion might have stopped at this stage. But an hypothesis gains in credibility the greater the collateral evidence which can be brought forward in its support; and if it could be shown that the principles already obtained not only explained the phenomena of the zymotic diseases, but were also equally reliable in dealing with other classes of disease, the whole of the argumentation would be much strengthened. With this view the constitutional diseases were discussed at some length, and not only were the main phenomena of those diseases which were selected as good specimens of this class explained, but analogies were also traced between these and the zymotic diseases, in several particulars where a more superficial examination would not lead to the detection of any degree of resemblance. The results reached during this part of the discussion manifest a complete congruity with those previously obtained, and are, therefore, a further ratification of the truth of our primary assumption.

Up to this stage of the discussion, the unity of disease was much more dwelt upon than its diversity; but in the present chapter, the principles advocated here have acquired additional confirmation by its being shown that they are equally applicable when disease is regarded not in union, but in division. It was shown that the principles adopted in explaining the phenomena of the various diseases lead deductively to very important distinctions which may be useful in dealing with disease in division and difference, as well as in union and agreement; and this strengthens still further the general fabric of conclusions.

It is not maintained that this chain of argument—even if admitted to be true—places the truth of the primary assumption that contagium particles are merely units detached from a living body beyond doubt, in the absence of direct experimental evidence; but it is maintained that, in so far as any assumption is confirmed by analogy, by the results obtained corresponding to observed facts, and by collateral evidence, a case of considerable strength has been made out for this supposition; and, until the opposing hypothesis that contagium particles are parasites in the zoological sense is proved by direct experimental evidence, or as strong a case is made out for it by indirect evidence, I shall provisionally adopt what I have termed the *Graft Theory* of Disease.

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