AN ADDRESS ON THE GERM-THEORY OF DISEASE.*

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IN these days of rapid progress, when so many busy workers in all parts of the world are continually adding to our stores of knowledge, there is some danger, I think, lest the country medical practitioner, whose time is so fully occupied in the active duties of his profession, should lag behind in the race, and find himself unable to keep up with those who have more time and opportunity to devote to medical science. Perhaps, therefore, on occasions like the present, it may not be unprofitable to glance, not only at the principles which seem to animate the active workers of the present day, but also at the measure of success which has attended their labours.

"The germ-theory", briefly stated, is, that many diseases are caused by the presence in the system of minute germs or spores, which form no natural part of the body, but which, by their growth and multiplication in the body, give rise to the phenomena of the disease. The origin of these germs, though a matter of much interest and importance, is a separate question from, and is not involved in, the theory of causation of diseases by germs. This germ-theory has been used to explain the origin of a great number of diseases; but it will only be possible for me to glance at a few of these on the present occasion; and I propose to select those in which the theory in question has proved useful in suggesting means, either for prophylaxis or treatment.

The class of diseases whose phenomena are, perhaps, best explained by the germ-theory are the eruptive fevers—such as small-pox, scarlet fever, measles, typhus and typhoid fevers. The features which these complaints possess in common—such as their period of incubation, the fever which attends them, the local manifestation, such as the rash of small-pox, scarlet fever, and measles, and the intestinal ulcerations of typhoid fever; the fact that they have a definite period of duration, and attack patients, as a rule, but once in a life-time—are all better accounted for by the germ-theory than by any other. It has been clearly proved in the case of vaccine lymph, and more recently in the case of measles, that the contagium is particulate, *i.e.*, that it consists of minute particles. These, according to the germ-theory, are the germs of parasitic fungi. On finding an entrance into the system, either by being introduced directly into the blood, as in vaccination, or through the mucous membranes of the air-passages or alimentary canal, they commence growth, provided they can find in the body invaded a suitable soil; or, in other words, that material which they require for growth. The period of incubation corresponds with this period of growth, and also of reproduction. The few germs at first introduced increase a thousandfold. When this increase has reached a certain point, fever sets in, and a further change in the history of the parasite ensues, *viz.*, a process of fecundation, or something analogous to that of flowering in the higher plants, the result of which is the production of a vast number of fresh germs or spores. The fecundating process takes place in those parts of the body which are the seats of the local manifestations of the disease, *viz.*, in the skin, in the case of small-pox; in the skin and mucous membrane of the respiratory organs, in measles; and in the intestinal glands, in typhoid fever. When the material in the system on which these parasitic fungi live has all been exhausted, the disease comes to a natural termination. The definite period of duration of an eruptive fever is thus accounted for; and the fact that a person is, as a rule, attacked but once in his life by a disease of this class, is explained by the supposition, that the material on which the parasite feeds having been once destroyed, is either never, or only after a lapse of some years, and then only imperfectly, reproduced in the system. This is a very brief sketch of the application of the germ-theory to the explanation of the phenomena attending eruptive fevers. For a full account of the subject, I must refer my hearers to an *Essay on the Germ-Theory*, by Dr. MacLagan, which is especially interesting to us as general practitioners—inasmuch as he regards the question from a clinical point of view. Now, probably few of my hearers have the

time and opportunities for making elaborate physiological experiments, or minute microscopical investigations; but there are none of us who are not having the clinical features of the eruptive fevers constantly brought under our notice at the bedside of our patients; and we are all, therefore, well able to form an opinion as to how far the germ-theory is capable of explaining the symptoms which diseases of this class manifest.

Now, in testing the truth of this theory, it would seem natural that we should act upon the hints thrown out by Sir J. Paget in his very suggestive address, delivered at the Cambridge meeting of the British Medical Association in 1880, in which he directed the attention of his hearers to the study of the diseases of plants, as tending to elucidate obscure facts in human pathology. If the eruptive fevers be really caused by the growth in the body of parasitic fungi, we ought to learn something from the study of these fungi out of the body; and in this, I think, we shall not be disappointed. There is a fungus which appears in its life-history, and, to a certain extent, in its microscopic character, to bear a considerable resemblance to those attacking the human frame—I mean the well known yeast-plant, or torula cerevisiae. This fungus consists of minute spherical or ovoid cells, varying from $\frac{1}{1000}$ to $\frac{1}{2000}$ inch in diameter. These cells, when placed in a suitable soil, such as a saccharine solution, begin, in three hours' time, to multiply by budding, and continue to do so for about three hours, when certain of them, which have become filled with minute granules, burst, and discharge their contents, and these minute granules become the germs of future yeast-plant. But while this physiological process has been going on, a remarkable chemical change has taken place in the saccharine solution, which has undergone the process of fermentation, and has been converted into alcohol and carbonic acid. It would seem probable that all genuine fermentations are due to the growth in the liquids undergoing fermentation of some species of fungus, though each variety of fermentation would seem to require its own special fungus for its production. The analogy existing between the process of fermentation, and the phenomena of eruptive fevers, has long been recognised; and, in consequence of this, the name zymotic diseases has been applied to these fevers; but the close relationship really existing was first shown, I believe, by M. Pasteur, who, in his investigations respecting chicken-cholera, showed—1, that this disease was produced by the presence of an oval bacterium in the blood; 2, that this fungus, when cultivated outside the body in chicken-broth, caused a fermentation, which ended in the production of an alkaloid; and 3, that when grown in the body of the living chicken, this same fungus gave rise to symptoms, many of which were similar to those seen in the eruptive fevers of man; and finally caused the death of the fowl. In the case also of common yeast, if the fungus be grown in blood outside the body, it decomposes the sugar of the blood, producing carbonic acid and alcohol. If introduced into the circulation of a living animal, it destroys life, causing inflammation and ulceration of Peyer's patches. M. Pasteur thinks that, when growing in the body, these fungi act mainly by abstracting oxygen. It is found that, when the yeast-plant is freely supplied with oxygen, it causes but little fermentative change; and M. Pasteur, applying this principle to the fungus which causes chicken-cholera, found that by growing it for some time in oxygen, he obtained a fluid whose active principle was so weakened, that it produced a very mild disease in fowls inoculated with it, but which seemed still to have great protective power against subsequent attacks of the disease; and he further extended this principle to the bacillus anthracis, or organism causing splenic fever in sheep; and, in consequence of his advice, protective inoculation has been extensively employed in France. The analogy, therefore, existing between the phenomena manifested in the growth of parasitic fungi in and out of the animal body, affords strong evidence in favour of the germ-theory as applied to eruptive fevers. On the other hand, it has been urged that if these diseases be caused by germs, these germs ought to be found in the blood. Now, in the case of splenic fever and relapsing fever, germs have been found abundantly in the blood, and have been clearly proved to be the efficient causes of the diseases in question. In most cases, however, the extreme minuteness of the germs would seem to be the reason why they have escaped detection.

If the full grown yeast-cells vary from $\frac{1}{1000}$ to $\frac{1}{2000}$ inch in diameter, how exceedingly minute must be the germs with which these cells are filled, each one of which germs is capable of reproducing, in a suitable soil, a plant similar to that from which it sprung. Moreover, the difficulty of distinguishing between such germs and ordinary granular matter, which may be found everywhere, must be almost insuperable. Professor Tyndall has, I think, clearly shown that, by the passage of a beam of light through the air on a liquid, floating particles may be detected so minute as to elude microscopic research. I think, therefore, that the fact that germs have not yet been detected in the blood

* We omit a few prefatory remarks by the author, of personal and local interest.

in many of the eruptive fevers, must not be considered to be a proof that they do not exist there. But though they have not been detected in the blood, yet in those parts of the body in which the local manifestations of the fever are found, organisms have been discovered in abundance. In the case of measles, Dr. Braidwood and Mr. Vacher state that they found numerous sparkling colourless bodies, like those found in vaccine, but larger, in the breath of the patients. They also found in two fatal cases, in the layer of true skin lying next to the rete Malpighii, numbers of these sparkling colourless bodies, which were unaffected by the material (carmine) used to stain the other constituents of the section. They found similar bodies in the liver and the lungs, but not in any other organs. They conclude from their observations, that the bodies found in the breath are the germs of measles; that they are conveyed by the blood to different parts of the body; and that the skin, and especially the lungs, are the favourite breeding-grounds of the contagium. These observations are published in the *BRITISH MEDICAL JOURNAL* for January 21st, 1882. In small-pox, minute organisms have been found in the fluid of the pustules; and in typhoid fever, Dr. Klein has shown that the lowest lesion is attended with the development of masses of minute organisms over and around the affected glands. In diphtheria, which is a disease presenting many analogies with the eruptive fevers, numerous observers have found that the mucous membrane of the part affected is infiltrated with masses of minute fungi or micrococci; and in erysipelas, an affection also closely allied to the specific fevers, micrococci have been found abundantly in the inflamed skin. The absence in these diseases of minute organisms from the blood is probably partly to be accounted for, as has been already suggested, by their minute size, and partly by the fact that the seat of the local lesion indicates the place where the parasite is propagated, and where it occurs in its perfect condition. In splenic fever and relapsing fever, in which organisms have been found in the blood, it would seem that the parasite attains its perfect condition and propagates itself in the blood itself, hence they are unattended with local lesions; while in the other specific fevers, the fungus exists only in the state of germs in the blood, and attains its full development in the parts affected by the local lesion.

Another objection which has been raised to the germ-theory, as applied to specific fevers, is that bacteria and other low forms of vegetable life are constantly found in various parts of the body without giving rise to disease; but the answer to this is, that there are harmless and injurious microphytes. As Dr. Burdon Sanderson has said, "All microzymes are not contagia; but all contagia may be microzymes"; and surely, from the analogies presented by the higher plants, this is only what we should be led to expect. It is extremely difficult to distinguish between the different species of the humbler plants, and yet these seemingly identical species may possess very different properties. So, also, the ova of even the highest animals may be indistinguishable from each other, yet we know that each ovum possesses a potentiality of its own, which leads to its development into a being similar to the one from which it sprang. As regards the eruptive fevers, I must say that I think the evidence in favour of the theory that they are produced by the growth and propagation of specific vegetable parasites in the body, is very strong.

Another class of diseases to which the germ-theory has been extensively applied, both theoretically and practically, is the group of traumatic infective diseases, including septicæmia, pyæmia, hospital gangrene, and possibly puerperal fever. So many divergent opinions have been expressed of late with respect to diseases of this class, and so many experiments and investigations, leading to different results, have been made for the purpose of elucidating their origin, that I find considerable difficulty in analysing the work done, and in forming a correct idea of what points may be considered as fairly established, and what are still doubtful. The most important recent researches are, I think, to be found in the Report of the Committee appointed by the Pathological Society to investigate the causes of these diseases, published in the *Transactions* for 1879; the experiments performed on animals by Dr. Koch, translated by the Sydenham Society in 1880; and the Report of the Committee appointed by the British Medical Association on Contagium, the last portion of which appeared in the *BRITISH MEDICAL JOURNAL* for February 25th, 1882. Koch, whose experiments are most interesting, and I think very important, found that, on injecting a large dose (eight drops) of putrefying blood under the skin of a mouse, the animal dies in from four to eight hours, and after death no bacteria or other microphytes are found in its blood or viscera; moreover, if its blood be injected into another mouse, it produces no effect. Koch considers that in such a case the animal has been killed by the chemical effect of the putrid blood—that death has been caused by a soluble poison, sepsine, which has been proved to exist in decomposing blood, and that no infective disease has been set up. If, however, a smaller

dose, one or two drops only, of the putrid blood be injected into a mouse, no effect may be produced, but in about a third of the animals experimented upon, a disease, with very marked and definite symptoms, exactly the same in all cases, ensues, and the blood becomes highly infective, a tenth of a drop being sufficient to infect, with the utmost certainty, another mouse, when injected under the skin. After death, no purulent deposits were found in the viscera, but the blood swarmed with minute bacteria, resembling those found in anthrax, but much smaller. This disease Koch calls septicæmia. He found that it could not be conveyed to rabbits, or even to field-mice.

A second disease, progressive gangrene in mice, Koch found was occasionally produced by the introduction of another microphyte in the septicæmic blood. This fungus, which appears as a micrococcus, and not a bacterium, spreads locally from the part infected, producing local gangrene, but does not infect the system generally. If another mouse be inoculated with the discharge from the gangrenous spot, similar results are just as infallibly produced as in the case of the septicæmia already alluded to. A third disease, called by Koch pyæmia in rabbits, was produced by injecting putrid fluids under the skin in these animals. After death, purulent infiltration of the subcutaneous tissue was found extending to the abdomen, spreading through the abdominal walls to the peritoneum, and causing extensive peritonitis. There were also metastatic deposits found in the lungs and liver. This disease proved to be as surely infective as the others, and under the microscope vast numbers of micrococci were found in the blood, which had a tendency to grow in chains, and were clearly distinguishable from other microphytes. Koch describes three other forms of infective disease which he produced in rabbits, and which he calls spreading abscess, septicæmia, and erysipelas, each of which was characterised by the presence of a perfectly definite and distinct micro-organism. The most important results of these experiments is the clear proof which they afford of the fact, that there are several species of infective microphytes, and that each one of these has a life-history of its own, and produces a separate and distinct disease, and this is only what we should expect from the facts disclosed by the eruptive fevers. Furthermore, I think we cannot fail to be struck by the analogy which these diseases described by Koch present to many of the cases of hospital gangrene, septicæmia, pyæmia, and traumatic erysipelas which we see in the human subject; though I think it would be very rash to conclude that they are identical with them. In fact, I believe that the opinion of those who have studied this part of the subject most, is that there are comparatively but few of the diseases of animals which are communicable to man. The Committee of the Pathological Society attacked these diseases, so to speak, from the opposite side to that on which they were approached by Koch. Instead of making experiments on animals, they investigated clinically cases of septicæmia and pyæmia in man. They only met with two doubtful cases analogous to those described by Koch, in which the dose of the poison is so large, and its effect so rapid, that there is no time for infective disease to be set up; and to those cases they apply the name of septic intoxication. In other cases of less rapid septic infection, and of pyæmia examination, of the blood showed the presence of microphytes in it in some cases, but not in all; and furthermore, that no form of bacterium is peculiar to any one kind of disease. On examination of the viscera, micrococci were found either in all, or at least some, of the viscera, and great pathological changes were often manifest in the structures infected by them. Moreover, they recognise, both in respect to septicæmia and also pyæmia, two classes of cases; in one the disease is not infective and the poison does not multiply in the system, and in the other a true general infective process is established.

The committee appointed by the British Medical Association, in their report on the Life-History of Contagium, published in the *JOURNAL* for February 4th, 1882, state that, from numerous experiments on rabbits, they consider that human lochial discharge of the fourth day invariably induces septicæmia in rabbits; that the fatal dose varies from half a drachm to a drachm, according to the size of the animal; that, when allowed to putrefy, it produces local irritation, in addition to the general visceral injuries; that it is most fatal when brought into contact with the pelvic peritoneum; that antiseptic solutions mixed with it do not modify its lethal influence; and that micro-organisms are not present in it, nor are they necessary for the production of the septic properties of the solution. From experiments with other septic fluids, they infer that the presence of pus intensifies the power of a septic fluid, but that pus alone is not necessarily septic; that purulent fluid from the peritoneal cavity is more poisonous than that from connective tissue; and that putrefaction increases the septic action of a septic fluid. On examining microscopically the viscera containing secondary deposits, they found that these deposits were due

to embolism, exudation, and softening or caseation; but they failed to find any micro-organisms.

Lastly, there are the remarkable experiments of Rosenberger, noticed in the *Lancet* for February 18th, 1882. He exposed septic fluids to a temperature of 140° Cent. (284° Fahr.) for two hours, for the purpose of destroying microphytes in them; and then injected animals with them. He found that, though the virulence of these poisons had been weakened by the boiling, yet it had not been destroyed. If a sufficient dose were given, septicæmia was produced, and the blood of animals thus killed proved to be highly infective. Moreover, he found that every variety of septic fluid produced, after boiling, exactly the same form of organism as was produced when the unboiled fluid was used.

Now, on reviewing these various experiments and observations, both clinical and microscopical, what conclusions can we arrive at respecting the relations of the germs of micro-organisms to these traumatic diseases? The most popular theory at present seems to be, that they are the sole causes of this group of diseases. It is said that all putrefactive changes are owing to the presence of germs derived from the atmosphere in the substance undergoing putrefaction. This, however, has been stoutly disputed by some observers. Still I must confess that, to my own mind, the balance of evidence seems to be in favour of those who attribute all putrefactive changes to the agency of germs; but, even if this be granted, it does not follow that the poisonous properties of septic fluids are necessarily due to putrefactive changes. The experiments of Koch and others seem to show that a chemical poison may exist in septic fluids, which may act just as a dose of strychnia or aconite; and, if this be so, it may account for the development of septicæmia and pyæmia in some cases in which it would be difficult to suppose that any germs could have obtained admittance into the fluid the source of the infection. But, if it be granted that some cases of septic poisoning may be due to a chemical rather than an organised poison, still, I think it must also be allowed that there is a strong probability that many forms of septic disease are owing to the growth of microphytes in the body, acting in the same way as in the case of the eruptive fevers.

The experiments of Koch and many others; the discovery of micrococci and bacteria in the viscera in cases of pyæmia and septicæmia; and many of the clinical phenomena of diseases of this class, especially the highly infective character of some of them, all seem to point to this conclusion. The failure to detect these germs in some cases in which they might be expected to be found, may possibly be due to the great difficulty which there is in demonstrating them under the microscope, partly owing to their minute size, and partly owing to their being obscured by the tissues in which they are embedded. Thus, Koch found that it was only by using a peculiar method of staining, and a special mode of illumination, that they could be detected in his specimens. Lastly, as regards the origin of these germs, I think that the balance of evidence is still in favour of the view that they are introduced from without, as in the case of the eruptive fevers. Rosenberger believes that their development after the injection of virus, sterilised, as he thinks, by boiling, can be explained only on the assumption that they arise from non-specific germs pre-existent in the blood; that the virus has introduced a chemical or unorganised poison, which has led to this remarkable development. But this view seems to me to introduce a theory so utterly at variance with all the laws regulating the development of plants and animals, that I think more evidence is required before it can be accepted.

Passing on now from the consideration of these septic diseases to ordinary inflammation, we find that some observers are inclined to attribute all acute inflammations to the action of germs. Dr. A. Ogston, in a paper in the *BRITISH MEDICAL JOURNAL* for March 12th, 1881, has clearly shown that acute and pyæmic abscesses always contain micrococci; and that pus from such abscesses, when injected into the subcutaneous cellular tissue of animals, as a general rule produces acute inflammation, accompanied with blood-poisoning, and ending in abscess; but that its effects may be resisted by an unusually insusceptible animal, especially if the dose be minute. He found, on the other hand, that the pus of chronic or cold abscesses never contained micrococci, and was harmless when injected into animals. He discovered micrococci in suppurating wounds, but found that Listerian dressings prevent micro-organisms from gaining access to wounds. He also proved, by experiment, that micrococci do not produce putrefaction, and that they develop best when removed from the atmosphere.

The exact relations of micro-organisms to inflammation has been very recently considered by Dr. Burdon Sanderson in his Lumenian Lectures, recorded in the *BRITISH MEDICAL JOURNAL* for last April; and he lays down the following propositions. 1. The exudation of normal inflammation is non-infective. 2. No organisms endowed with inflammation-producing, or phlogogenic properties, exist in the atmo-

sphere, or ordinary aqueous liquids, with which our bodies come in contact. By this proposition he does not mean to throw any doubt on the supposed existence of specific contagia, but simply maintains that these contagia are non-phlogogenic. 3. Whenever an inflammation becomes infective, it owes that property to a chemical change in the exudation liquid, of which the presence of microzymes is a necessary condition. 4. Micrococci which have found their way into the circulation may serve as vehicles for the dissemination of infective malaria. Taking the observations of Drs. Ogston and Sanderson together, I think we may consider it to be fairly established that simple ordinary inflammations are not caused by microzymes, but that inflammatory products are a very favourable soil for the development of micro-organisms, and, if they gain access to them, an infective disease is at once set up, and a comparatively harmless process is converted into a dangerous one.

The bearing of the germ-theory on diseases of the respiratory organs affords a good example both of the rapidity of scientific progress, and also of the wide application of this theory to the etiology of disease. It might at first sight appear that phthisis could have but little in common either with the eruptive fevers or the traumatic infective fevers. That some forms of disease resembling phthisis, and affecting animals, such as the *Perlsucht*, which attacks cows, are dependent on micro-organisms, has been known for some time. It has also been proved that, by making dogs inhale the sputa of phthisical patients, tubercular disease is set up in their lungs; but when I wrote this part of my paper a few weeks since, I was obliged to own that phthisical disease in the human subject had not yet been proved to be caused by microphytes. Dr. Koch, however, in a paper read before the Physiological Society of Berlin, on the 24th of last March, has proved that a peculiar microphyte, the tubercle-bacillus, exists in the walls of tuberculous cavities, in the sputum of phthisical patients, in degenerated scrofulous glands, in fungous joints, and in the bones of tuberculous cattle. These microphytes can only be made manifest by a peculiar method of preparation, a modification of that which he used in the experiments already alluded to. He found that these microphytes, when coloured with methylene blue, did not lose their colour under the action of vesuvium. In this respect, they differed from all other microphytes, except the bacilli of leprosy, which acted in the same way. This fact is in itself an interesting one, as showing that these minute organisms so closely resembling each other, may yet possess very different chemical properties. The tubercle bacilli are very small rods, in length about one-third of the diameter of a red blood-corpuscle, and in breadth about one-sixth of their length. To prove that these microphytes are the actual *materies morbi* of tuberculosis, Koch made numerous careful culture experiments, and found that if animals were inoculated with a minute portion of the cultivated material, tubercular disease was invariably produced in them, even in those animals which are not usually the subjects of tuberculosis. These experiments of Koch have already been confirmed by independent observers, and their importance for human pathology can scarcely be overestimated.

With reference to pneumonia, the etiology can scarcely be said to be so clear as in phthisis. I feel sure that I have more than once seen outbreaks of pneumonia which owed their origin to air contaminated with sewer-gas, in which several members of a household were attacked at the same time; and some remarkable cases of this kind have recently been described in the medical journals. I think it highly probable that such cases have a parasitic origin; but this, so far as I am aware, has not yet been proved. I know that micrococci have been detected in the lungs in some cases of pneumonia, but this fact is not sufficient of itself to prove that these are the causes of the disease. This question of the etiology of pneumonia is now under the consideration of the Collective Investigation Committee of the British Medical Association, and I trust that some light may thereby be thrown upon it. Ordinary coryza has been attributed to germs; and in the case of influenza, this cause may be the true one; but it has yet to be proved.

In connection with this group of diseases of the respiratory organs may be considered some curious observations of Sternberg, Pasteur, and others with reference to the saliva. Dr. Sternberg describes a fatal form of septicæmia in the rabbit, induced by the injection of human saliva under the skin. This he proved to be caused by the presence in the saliva of a micrococcus, a living organism, which, when subcutaneously injected, multiplies in the connective tissue, and also in the blood shortly before and after death. It does not exist in the saliva of all persons, but only of some, and it is only in those cases in which it is present that the saliva possesses poisonous properties. I need scarcely stay to point out the important bearing of these facts on the etiology of such diseases as rabies and glanders, in both of which the saliva has a peculiarly virulent character, and both of which are probably due to parasitic fungi.

Another disease which recent investigations seem to have clearly proved to be due to microphytes, is ague or malarious fever. A fungus has been detected in the air and soil of malarious localities, and more recently in the blood of patients suffering from ague, when examined during the cold stage of the disease. An attempt has also been made to place rheumatic fever in this class of diseases, and to prove it likewise has a parasitic origin. At present, however, this must be considered as unproved, and there are many features of the disease which certainly do not resemble the characteristic symptoms of diseases caused by parasitic fungi.

In a paper read before the North Wales Branch of the British Medical Association in September 1881, Dr. Roberts of Manchester has described some cases of disease caused by parasitic fungi in the urine. He first notices the well known cases of incipient and ammoniacal decomposition of the urine, due to the presence of bacteria, and then gives notes of some rarer cases, in some of which he has found bacteria, which, without causing decomposition of the urine, have given rise to violent bladder symptoms; and another, in which persistent nmatria seemed to be dependent on the presence in the urine of a peculiar micrococcus. In the first cases, salicylate of soda; and in the last, oil of turpentine and oil of eucalyptus—all, acknowledged antiparasitic remedies—were found of much use.

Lastly, I must make a passing allusion to skin-diseases. Of these there are some, such as favus and pityriasis versicolor, whose parasitic origin has long been recognised, but every day is adding to their number. Quite recently, I see that leprosy and acute pemphigus have both been attributed to the presence of parasitic fungi. I have not made any allusion to certain rare or local diseases, such as filarial disease, etc., which have been proved to be due to parasites, either animal or vegetable, because they but rarely come under the notice of the general practitioner in this country. At the same time, I would remark, in passing, that it is quite in accordance with the germ-theory that some of these disease-producing parasites, like higher plants, should be either limited to certain portions of the earth's surface, or require certain climates for their development.

Now, let me make a few remarks on the practical result of this germ-theory. Of what use has it been to medical practice? Here, I think, we must all agree that, whether true or not, a theory which could produce such a practical result as antiseptic surgery has been of incalculable benefit to medical science, and it was in consequence of his firm belief in this theory that Professor Lister was led to introduce the method of treating wounds with which his name is connected, and which has met with such general acceptance throughout the surgical world. Whether we attribute the success of this method simply to extreme care and cleanliness, or whether we admit the existence of germs, we must in either case, I think, feel grateful to a theory which has led to practical results of such acknowledged utility.

Another most important benefit derived from the germ-theory is the guidance and direction which it has given to our efforts to guard against and prevent the spread of contagious diseases. In our choice of antiseptics we are guided mainly by the effects which these substances have in destroying germ life; and in dealing with sewage, both in the mass and in individual cases, our principal aim is to destroy those germs which we believe it to contain, and which are so injurious to health. Acting on this principle, Dr. Carpenter of Croydon has made some experiments, which I think have clearly proved that the most efficient agents in nature for the destruction of living germs are the spongioles of living plants; and just as Darwin and others have shown that the leaves of the Sundew and other insectivorous plants can destroy, digest, and assimilate both living and dead organic matter brought into contact with them, so Dr. Carpenter has shown that the spongioles of Italian rye-grass greedily seize upon and appropriate the organic germs which sewage contains. It follows from this that the most efficient method of dealing with sewage generally is, when the local conditions admit of it, to apply it to the roots of plants; or, in other words, to utilise it, and render it innocuous by means of a sewage-farm. Here, I may remark that if any present are interested in this matter, they may, if they think fit, avail themselves of the opportunity which their visit here to-day affords them, of inspecting the small sewage-farm connected with this town, which has, I think, up to the present fulfilled very satisfactorily the objects for which it was established. In medicine proper, the influence of the germ-theory has perhaps been less strongly felt than in surgery and sanitary medicine. The difficulty seems to be to discover agents which may destroy the germs which have got into the system without at the same time injuring the tissues of the body; but I cannot help thinking that it may ultimately prove that some of the drugs which have been proved by experience to be efficacious in the treatment of certain diseases may owe their virtue to their power of destroying germ life, as, for instance, quinine in the treatment of ague.

I see also that the bold experiment of injecting carbolic acid into the blood in cases of yellow fever has been made; and, it is said, with very marked success. If this should really prove to be the case, may we not hope, by working in this direction, to find some remedy for typhoid fever and other kindred affections? The use of continuous antiseptic inhalations by means of naso-oral respirators in cases of phthisis, is now being extensively tried, and would seem to have met with some measure of success. I have myself tried it; but not yet in a sufficient number of cases, or for sufficiently long, to enable me to decide on its merits. The practice of inoculation with attenuated virus, which has been introduced by M. Pasteur with such remarkable results in the case of chicken-cholera, and splenic fever in sheep, is another legitimate result of the germ-theory; and we can scarcely doubt but that the time is not far distant when this principle, which has been long empirically adopted in the case of vaccination, will be still more extensively employed in the treatment of diseases affecting the human subject.

There are, I fear, at least two classes of persons to whom this germ-theory of disease will be peculiarly distasteful, and they are the anti-vaccinationists and the anti-vivisectionists; for there are at least two points which the investigations connected with this theory have tended more clearly to establish, and these are: 1, that the practice of vaccination rests on a thoroughly scientific basis; and 2, that by experiments on living animals, and on living animals only, has it been possible to establish truths, and introduce remedial measures, which have already been the means of saving a vast amount of animal life, and which promise to confer incalculable benefits on the human race. I rejoice, therefore, to think that the medical profession are now taking steps to prevent the enactment of such laws as must effectually check the progress of scientific research, and will compel earnest English students of physiology to exile themselves to foreign countries, in order that they may pursue their search for truth, and devise means for the relief of suffering humanity.

For my own part, I cannot help hoping that the germ-theory is likely to do for medical science, what the theory of evolution by natural selection has done for biology—that it will lead to the opening up and investigation of new and rich fields of research in various directions, afford a clue to the etiology of many obscure diseases, and last, but not least, lead to the introduction of new and efficient methods of guarding against and combating disease; and it is this feeling of the importance and many-sidedness of my subject, which makes me painfully conscious of the very inefficient manner in which I have treated it. If, however, the remarks which I have made should have the effect of eliciting the opinions of my hearers on this important subject, I shall feel that I have not spoken in vain, and I shall hope that those who have so patiently listened to me will forgive me for having trespassed so long on their time and patience.

SOME OBSERVATIONS ON RHEUMATISM AND GOUT, WITH A NEW DEPARTURE IN THEIR CURE.

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SINCE the days of Sydenham, himself a martyr to gout, and the first English physician who pointed out the distinction between that disease and rheumatism, these painful disorders have afforded ample scope to the theorist and the practical observer; and yet, after the lapse of two hundred years, though in possession of more accurate knowledge than our predecessors of their effect upon the tissues, upon the structure of the heart, and the articulations; and though we can trace to their agency a number of secondary affections consequent upon cardiac injury, it cannot be affirmed that, in the treatment of these diseases, we have made correlative advancement. We are still at variance upon the etiology, the pathology, and, above all, upon the procedure most likely to effect a cure of arthritic ailments. The trusted remedy of to-day is discarded for a newer favourite on the morrow, and we seek in vain for some specific, some all-powerful agent, that will act like sulphur on the skin, or mercury in enthetic disease.

The remedy at present in the ascendant is a certain Galenical preparation, which, at its introduction in this country, was announced to be as sure a specific for acute rheumatism as quinine for ague, or ipecacuanha for dysentery, as well as a certain preventative of cardiac complications; and although time and sad experience have rather rudely dispelled these illusions, and none of its specious attributes remain but its sedative influence over pain and, pyrexia, the majority of the profession, if we may judge by the laudatory contributions with