will be incomplete. Sloughing is an almost constant sequence of the injection of the permanganate. Experiments XXXIII. and XXXIV. demonstrate clearly the effect of the hypodermic injection of the permanganate. The poison was removed from the glands of a cobra and one half was injected into one dog *weighing* 50 *lbs.*, and one half into another *weighing only* 32 *lbs.* No remedy was applied in the former case, and the animal died in 6 hours 41 minutes. In the latter case permanganate was injected 5 minutes after the injection of the poison, and although the animal was much weaker and smaller, it exhibited no symptoms of snake poisoning. Another excellent illustration is afforded by experiments L. and LI. Two dogs were injected each with a 2 centigramme solution of cobra-poison. In the one case a supposed "sure cure" sent from Africa was administered almost immediately after the injection, and the animal died in 5 hours and 41 minutes. In the other case five minutes after the injection a catgut ligature was applied, and thirteen minutes subsequently—that is eighteen minutes after the injection— a solution of permanganate of potash was injected and the animal showed no symptoms of snake-poisoning. Failure is generally explainable by inefficient ligature or incom plete injection of the permanganate. Some times, however, the intervals were too long or the dose of poison too large. But on this subject I shall have something to say when dealing with the treatment of snake poisoning. The best result was that in which a quantity of poison was injected into a dog sufficient to kill it in from five to six hours, and the ligature was applied five minutes after, followed by the injection of the permanganate of potash 20 minutes after—25 minutes from injection of cobra-poison to that of the permanganate of potash—and the animal exhibited not a single symptom of snake-poisoning. It sometimes happens—either from inefficient ligature or from incomplete application of the permanganate—that a quantity of the poison will be absorbed, serious symptoms will result, but the animal will recover, as is demonstrated in experiments XXXVI., XLI. and XLIX. Or after several hours of suffering it may die, as demonstrated by experiment XLV. I have been assisted in these experiments by the Railway Native Doctor Tarini Prosono Bhuttacharjee, and I am under obligations to him for watching the animals in my absence and for handling the snakes for the extraction of the poison. I will deal with the treatment of cobra-poisoning in my next paper.

(To be continued)

REMARKS ON THE SUBJECT OF MICRO-ORGANISMS IN DISEASE.

(Read before the Gwalior District Medical Society.)

By Surgeon O. G. Wood, M. B., A. M. D.

So vast within the last few years, indeed I may say months, have been the strides of science not only in discovering the living germs of specific diseases, but also in studying their natural history; and so great is the revolution which such discoveries promise to effect in our views regarding the etiology, pathology, and treatment of these diseases, that I scarcely feel it necessary to apologize for bringing this subject before your notice this evening, albeit the facts I am about to narrate have all appeared recently in various medical periodicals. It has of course been long acknowledged that some at least of the so called zymotic diseases are propagated by a specific germ or micro-organism; but it is only quite recently that distinct proof of the existence of such a germ has been obtained, and many diseases added to the list of those supposed to be so propagated. It is principally to the patient and accurate experiments of M. Pasteur that we are indebted for most of our information on the subject, and to the astonishing results of antiseptic surgery for the rapidity with which his views obtained popularity. And now hosts of workers in the same field, whose names are familiar to all, are daily confirming and extending his results, or correcting the slight inaccuracies inseparable from every series of experiments. It is now over thirty years ago since it was first discovered that Splenic Fever in cattle is due to the presence of a living organism in the blood, but it is only quite recently that we have learned that in man Malignant Pustule and "Wool-sorters Disease" are the results of the very same organism inoculated on or inhaled by persons brought in contact with the carcase or working with the hides or wool of animals which have died of anthrax. Further experiments have demonstrated the important fact that spores of the Bacillus Anthracis may be found in large quantities in the soil in which cattle which have died of splenic fever ten years previously have been buried; and still more startling that these spores are being constantly brought to the surface from a depth of 6 feet by earth-worms; insomuch that animals grazing over the ground or fed on clover culled from it were at once attacked by the disease These experiments were so careful and so crucial that no doubt can for a moment be cast on their accuracy or the logical deductions to be drawn from their results. Now if, as we may be well assured, the recent researches of Mr. Darwin, recorded in his work on "The Formation of Vegetable Mould through the action of Earth-worms," be reliable, the tremendous importance of the discoveries of M. Pasteur becomes apparent. For instance, Mr. Darwin found that in a field near Nice the castings of earth-worms within a square foot of surface weighed 12 oz. a year, equivalent to no less than 14½ tons per acre; while in a chalk down in Kent the castings amounted to nearly 84 lbs. per square yard, or 18 tons per acre. That worms are no less active in India the following quotations from Mr. Darwin's book will sufficiently prove: "The late Mr. John Scott of the Botanic Gardens near Calcutta made many observations for me on worms living under the hot and humid climate of Bengal. The castings abound almost everywhere, in jungles and in the open ground, to a greater degree, as he thinks, than in England. After the water has subsided from the flooded rice fields the whole surface very soon becomes studded with castings." (p. 123). And again "The period during which worms near Calcutta display such extraordinary actvity lasts for only little over two

months, namely, during the cool season after the rains. At this time they are generally found within about 10 inches beneath the surface. During the hot season they burrow to a greater depth, and are then found coiled up and apparently hybernating. Mr. Scott has never seen them at a greater depth than 2½ feet, but has heard of their having been found at 4 feet." (p. 125).

Though the Bacillus Anthracis is the germ which has been most studied, and is consequently the one with the behaviour of which we are best acquainted, we must remember that it is only one of many which have been more or less carefully observed. The specific germs of malaria, of diphtheria, of hydrophobia, of fowl cholera, &c., have all been isolated and cultivated, and during their artificial cultivation many extraordinary and important facts have been elicited. Perhaps the most important of these practically is this—that during cultivation (and *en passant* we may observe that they propagate most readily in decomposing organic fluids) under certain conditions the bacteria of the disease lose much of their malignancy, become "tamed" in fact, as one writer has aptly termed it; and more extraordinary by far, that these tamed bacteria have the power of protecting the animal inoculated with them from future attacks of the disease even when introduced into the blood in its most malignant form. In other words, it has been experimentally proved that by inoculating sheep with the tamed—or as Pasteur terms it, the "attenuated" bacillus of anthrax, or poultry with the similarly treated bacterium of fowl cholera, the animals are so protected that subsequent inoculation even of the virulent germs is productive of no result whatever.

The most recent discovery of all, one which I think we must receive with some reservation until it has been confirmed by further experiments, is that of Dr. Grawitz of Berlin, that the spores of certain fungi (notably the Aspergillus and Peincillium) innocuous in their simple state, can be cultivated in such a way as to become capable not only of living and developing themselves in the living blood, but also of their causing morbid symptoms in the animal inoculated; but that the fungi produced by the spores are incapable of producing gonidia, and hence being unable to multiply in the body die out in course of time, leaving an immunity from future attacks of the same fungus in its virulent form. The uncultivated fungus, however, gives no immunity. To state this theory shortly—"a certain fungus can be, as it were, educated up to living inside the human body, and in like manner certain tissues of the human body, otherwise an easy prey to the fungus, can be educated up to resisting it." The tissues least capable of resisting the fungus are those least supplied with oxygen, and *vice versâ*. Dr. Grawitz moreover found that "the resisting power of the cells of the body is weakened in the course of their successive renewals, and is ultimately lost."

In connection with these discoveries some recent curious observations on the supposed influence of diet on the development of the Bacillus anthracis are worthy of more than a passing notice Mr. Spear of the West Riding, while engaged in investigating the subject of Wool-sorter's Disease, observed that the patients were usually attacked between Saturday evening and Monday morning. This fact, it appears, had long been recognized by the workmen themselves. From this circumstance Mr. Spear was led to conjecture that there must be something in the men's habits on their day of rest favorable to the development of the disease germs in their system; and his attention, while enquiring into the subject of alimentation, was arrested by this striking coincidence, that in nearly every case the symptoms had appeared immediately after the ingestion of an unusual amount of vegetable food, whether taken merely as supplementary to the Sunday dinner, or swallowed in the form of vegetable infusion for the purpose of curing "a cold." On following up this observation a little further he found that many relapses of the disease occurred after a full vegetable meal or an indulgence in fruit. Combining these circumstances with the fact that herbiverous animals are the most subject to anthrax, and that rats fed on a purely animal diet are insusceptible to inoculation, while the same rats restored to a vegetable regimen quickly succumb to the disease, he arrived at the conclusion that the excessive use of vegetables render the system a more suitable nidus for the Bacillus anthracis; and carrying his opinion a little further, he propounded the general theory "that alimentary substances may bring about in the body such chemical or morphological change as will render its fluids a richer field for the proliferation of disease-germs."

Doubtless, in the course of a few months, more extensive investigations will either confirm or refute these recent views; in the meanwhile it may be profitable for us to consider how they practically bear on the performance of our professional duties.

In the first place I think they must convince us how highly unsatisfactory is the present method of disposing of the bodies of those who have died of infectious diseases. Probabilities are decidedly in favour of the view that the disease germs actually increase and multiply in the fluids of the body undergoing decomposition; and that they do not become "attenuated" during that process is sufficiently demonstrated by the observations before alluded to on the buried carcases of diseased cattle. The researches of Darwin have further shown us how instrumental in bringing disease germs to the surface the constant operations of the ubiquitous earthworm must be. Cremation then appears to be the only safe method at present known of disposing of the diseased bodies of both men and beasts, and the sooner this idea is brought home to the public mind the better will it be for the public health. At present the dead are a constant source of danger to the living. But I fear many years will elapse before popular prejudice will be sufficiently overcome to permit of cremation superseding the ordinary method of interment; therefore we ought in the mean time to try and find some efficient substitute for it; and for this purpose I would suggest the practice of injecting both arteries and veins of all who have died of infectious disease with a strong solution of carbolic acid, and the free application of the same agent to the surface of the body, the mouth, nose and other easily reached cavities,

previous to burial. It is scarcely necessary to mention the importance of destroying by fire, as far as practicable, all fomites, and of scraping walls, floors and ceilings previous to fumigating a room, or to suggest that all such scrapings should be carefully collected and burned. These points are self-obvious.

2ndly.—As to the question of pathology. I think the results of Dr. Gravitz' experiments afford us some clue to the action of inoculation and vaccination, and to the mystery of the protective effect of an attack of an infectious disease. They also help us to understand why revaccination is so necessary an operation, especially during those periods of life when tissue changes are progressing most actively. It is a notorious fact that syphilis when newly introduced into a country assumes a much more virulent type than it presents when it has become indigenous to the place after a few generations; and this I believe may be explained by the influence of heredity in educating the tissues to resist the germ of disease and to diminish by means of the "taming" process its virulence by slow degrees.

3rdly.—Let us glance at the question of the rational treatment of germ-bred diseases in the light of these recent researches. And here a wide field lies open for us whose work is essentially clinical. I do not purpose here to enter into the question of inoculation, for it is too wide a subject to be discussed in a few words. At present we are scarcely in a position to calculate on the effect on the general health or duration of life which might be produced by exposing either child or adult to a series of diseases no matter how mild the induced attacks may be. But in the treatment of the fortuitously acquired disease, if we know that disease to be due to the presence of living organisms in the blood, we can no longer rest content merely to combat symptoms as they arise and leave the cure to time and nature. It becomes our duty to attempt to destroy the disease itself; and to discover the best and safest method to achieve this result should be our constant endeavour.

The first and most obvious plan which occurs to the mind is that of intravenous injections of germicide substances: let us see what has been already effected by their use. It appears that the natives of Senegal are subject to a disease known as "Nelavaw," a chronic disease taking eighteen months or two years to run its course, but apparently invariably proving fatal eventually if left to itself; and until a few months ago no treatment had been devised which had any influence in arresting its progress. A French physician, a M. Déclat, suspecting it to be of parasitic origin, suggested the intravenous injection of Phenic (carbolic) acid. This suggestion was carried out by a missionary in Senegal with complete success, in every instance. Again in June last another French physician practising at Rio, on Dr. Déclat's suggestion, tried this treatment in yellow fever with striking success, curing one case where the disease was so far advanced that the patient appeared to be on the point of death. This physician states that when he employed the treatment in the early stages of the fever the cure was so easy and so prompt, that he could scarcely believe them to have been cases of yellow fever at all. These results are exceedingly encouraging, but of course the injection of so powerful an agent as carbolic acid into the circulation cannot be free from danger in itself. There are, however, several other antiseptics which, while equally destructive to low organic life, do not possess such poisonous properties as carbolic acid. Amongst these I might cite the Oil of Eucalyptus. But Mr. Lister is at present engaged in a series of experiments to determine the best, and at the same time the least irritating form of antiseptic to supersede carbolic acid. Perhaps he may chance on some substance the use of which will be free from danger.

But can we find no method of introducing germicides into the blood simpler and less disagreeable than that of intravenous injections? I think that the successful use of the Hypo-sulphites in some forms of blood-poisoning, perhaps even the success of quinine in malarious fevers, &c., suggest to us that we can effect our object through the digestive system. Let us for example take malarious fevers, as they are diseases with which we are only too familiar. We now believe that these fevers are the result of the presence in the blood of a minute fungus, the Bacillus malariæ, which in the course of its growth and reproduction produces certain toxic effects on the nervous system, causing the familiar symptoms of ague, &c.; and we all acknowledge the remarkable power which quinine possesses of controlling and finally subduing such symptoms. How does quinine act? Now without overlooking its specific action in the nervous centres, I am yet inclined to believe that much of its success depends on its powerful germicide properties. We know, as Dr. Sydney Ringer says, that "small quantities of quinia salts destroy septic germs and arrest putrefaction more thoroughly than most antiseptics, including even arsenic and creasote"; and that "quinia readily passes into the blood, and as it can be detected unchanged in the urine, sweat, and secretions of healthy persons and fever patients, probably very little undergoes decomposition in the body." It is therefore only reasonable to suppose that this powerful antiseptic with its composition and properties unchanged meeting as it circulates in the blood with the malarial spores or fungi, destroys a certain proportion of them, or at least renders the blood unsuitable for their full development, and so modifies or cures the disease. And this view is supported by the therapeutic properties of all our other trustworthy antiperiodics. Dr. Ryan of the A. M. D. made some experiments lately with various drugs for the purpose of finding a fairly efficient substitute for quinine in malarious fever, and he found it in creasote. Moreover arsenic, so powerfully destructive to organic life; Tr. of iodine, an antiseptic; oil of eucalyptus, a germicide of no mean value; salicin, &c., have all been employed with various degrees of success in the treatment of both acute and chronic malarial poisoning. The results of recent experience, as recorded in the medical periodicals in the treatment of Enteric fever, diphtheria, &c., seem to support the idea that the most successful remedies are those possessed of antiseptic properties, administered by the mouth.

But there is yet another channel besides the veins and

the stomach by which the blood may be reached. I refer to the lungs. But as the subject of the inhalation of gases in the treatment of disease is an almost unexplored field, while that of the inhalation of atomised drugs is still in its infancy, my remarks must necessarily be brief. Indeed I will confine them to one suggestion. If the observation of Dr. Grawitz be reliable, viz., that his inoculated fungi were unable to continue to exist in tissues well supplied with oxygen, such as the brain; the careful administration by inhalation of oxygen gas in the earlier stages of germ-bred diseases might be worthy of a trial. At any rate if judiciously administered it could do but little harm, while if successful, the simplicity of the method would be much in favor of its adoption.

To one other point only, in connection with the life history of the micro-organisms of disease, would I direct your thoughtful attention; namely, to the suggestive theory of Mr. Spear relative to the effect of certain forms of diet in rendering the blood a fitter field for the cultivation of the disease fungi. But until further investigations into this curious subject have been carried out, we can do little more than keep our minds alive to the possibility of certain kinds of food hastening or retarding cure, or perhaps completely counteracting all our therapeutical endeavours to destroy the cause of the disease.

Morar, 26*th January* 1882.

THE BACILLUS MALARIÆ.

By Surgeon-Major C. F. Oldham.

The results obtained by Professors Klebs and Tommasi-Crudeli during their investigations into the cause of "Malaria," interesting as they were and conclusive as they appeared to some, are not borne out by Indian experience.

Already Deputy Surgeon-General Moore has protested in your columns against the tendency, apparent in some quarters, to accept the Bacillus malariæ as the embodiment of "malaria." To his observations I now add a supplement.

That bacilli and other similar organisms may exist in the soil of the Agro-Romano, and other malarious localities, is not unlikely; but they do not cause climatic, or so-called "malarious," fevers.

The identification of "the marsh poison" with the germs of living organisms has been announced before.

So long ago as 1859 Dr. Mitchell of Philadelphia ascribed intermittents to cryptogamic spores. And in 1866 Dr. Salisbury published a circumstantial account of his discovery of the cause of these fevers in the spores of Gemiasma, which he obtained from the soil of malarious localities.

These discoveries, however, have not stood the tests of time and further investigation; and, in fact, are almost forgotten.

The objections which applied to the discoveries of Dr. Salisbury and others, apply also to those of Professors Klebs and Tommasi-Crudeli, so far as "malaria" is concerned.

In each case the germs were procured from the soil, and they required moisture for their development. Climatic fevers however prevail where these conditions do not exist—where there is neither soil to support the vegetation, nor moisture to develope them.

In the great sandy desert of Northern India, and other similar regions in Asia, in the Sahara, and amongst the bare and sun-baked rocks of Beloochistan and Aden, the same fevers occur as in the marsh.

In some very hot and dry situations, such as low ranges of rocky hills, the fevers disappear on the setting in of heavy rain.

I have seen intermittent and remittent fevers rife in the Bhawulpore State, on the border of the Indian desert, at a time when there had been no rain for four months. The drought was excessive, even for this parched region; all vegetation was burnt up; and the difference between the wet and dry bulb thermometers was as much as 20° F.

I think it must be admitted that these fevers could not have been caused by the Bacillus malariæ; of which Professor Tommasi-Crudeli says that the absence of a moderate degree of permanent humidity is sufficient to arrest or render impossible its development.*

There are many other objections to the Bacillus malariæ, which it will not be easy to explain away.

The nocturnal energy of "malaria," and its comparative quiescence by day, when the most deadly swamp may be visited with impunity, can scarcely be reconciled with the supposed identity of the poison with living organisms. Then, autumn is so generally recognized as the "malaria" season, that autumnal fevers and intermittents are synonymous. But most forms of vegetation are then undergoing decay. Even the Bacillus malariæ seems to follow the ordinary rule for vegetable growths, as we are told that, in summer weather the air over malarious ground becomes so thickly peopled with bacilli that they can be collected in very large quantities in the perspiration of the forehead and of the hands.† Why then should autumn and not summer be the fever season? It may be observed that notwithstanding the enormous development of bacilli just alluded to, neither the professors nor their friends appear to have been attacked with fever during their investigations.

It is well known to those with tropical experience that a sudden change from great heat to comparative cold, as from the plains to a hill climate, is frequently followed by an attack of intermittent fever. And it is generally admitted that the recurrent attacks of this disease may be brought on by cold alone, without any fresh exposure to "malaria." How are we to account for this, under the supposition that any living organism is the cause of the fever? Does the Bacillus malariæ acquire renewed life and vigour from *cold* as well as from *heat*?

Then in India we know well the powerful influence of good shelter, and judicious clothing, in preventing climatic fevers. But these would avail little against the invasion of living organisms.

We are told that the Bacillus malariæ, having invaded the body, and caused the fever, disappears from the blood as the paroxysm reaches its acme; only its spores

* *Practitioner*, October 1881.

† *London Medical Record*, December 1880.