of sewer-gas is sulphuretted hydrogen; and Senator (*Berlin. Klin. Wochenschr.*, 1868, No. 24) described, in 1868, a most instructive case in which the patient became collapsed, and nearly died, with all the symptoms of poisoning by sulphuretted hydrogen generated in his own intestines.

A particular class of nervous symptoms, in which hypochondriasis and depression of spirits are accompanied by a deposit of oxalate of lime in the urine, has been thus described by Dr. Golding Bird. "The patients are generally much emaciated, excepting in slight cases, extremely nervous, painfully susceptible to external impressions, often hypochondriacal to an extreme degree, and, in very many cases, labour under the impression that they are about to fall victims to consumption. They complain bitterly of incapability of exerting themselves, the slightest exertion bringing on fatigue. Some feverish excitement, with the palms of the hands and soles of the feet dry and parched, especially in the evening, is often present in severe cases. In temper they are irritable and excitable; in men the sexual power is generally deficient, and often absent. A severe and constant pain, or sense of weight across the loins, is generally a prominent symptom, with, often, some amount of irritability of the bladder. The mental faculties are generally but slightly affected, loss of memory being sometimes more or less present" (Golding Bird, *Urinary Deposits*, 5th ed., p. 251).

But in this condition we find the same difficulty of attributing the symptoms to the presence of oxalate of lime that we found in ascribing the depression of spirits in biliousness to the presence of bile in the blood; for, as Dr. William Roberts (*Urinary and Renal Diseases*, 3rd ed., p. 79. London: Smith, Elder and Co.) points out, "these symptoms may be present in typical completeness without oxaluria, and, conversely, oxaluria may exist in its highest intensity, and even go on to the formation of a mulberry calculus, without evoking any of the above mentioned symptoms."

In this condition then, as in biliousness, we are almost forced to ascribe the symptoms to the presence of some poison, of whose presence in the blood the oxaluria in one case, and the yellowness of the conjunctiva in the other, are merely indications. The irritability which occurs in gouty persons is another example of nervous disturbance due to the presence of injurious substances in the blood, and their action upon the nervous system. But this has been already so graphically described by Murchison in his book on *Diseases of the Liver*,⁸ that I must refer you to it, and to the classical work of Garrod, as the subject is too wide to enter upon here.

There is only one so-called minor ailment connected with digestion which I will mention now, and that is headache. As I have pointed out elsewhere,⁹ headaches are usually dependant either upon the presence of decayed teeth, or of some irregularity in the eyes, more especially in the quality of focal lengths between the two eyes, or astigmatism. The site of headache depending upon decayed teeth varies with the teeth affected. The headache depending on inequality of vision is frequently frontal or occipital, although it may also be temporal. I am at present uncertain regarding the precise way in which indigestion produces headache; but I may remark that, as a rule, in headaches of this sort, the upper surface of the eyeball will be found to be excessively tender, and that the tension within the eyeball itself appears to be increased, so that sometimes the eyes feel like marbles or metal bullets under the finger when they are pressed. Another curious point that I have observed regarding headaches is that, as persons who are subject to them in their youth grow older, bilious headache is very apt to be replaced by giddiness; and that this change occurs about the time when the eyes are beginning to become a little hypermetropic, and the person begins to find the need of spectacles for reading.

Dr. Haig has made (*Practitioner*, August, 1884) the observation that in some persons the occurrence of headaches may be prevented by using nitrogenous food very sparingly, and avoiding butcher-meat altogether. This accords with my own observation regarding muscular weakness and langour, and, I think, renders it probable that some headaches, as well as langour, may be due to poisonous products derived from nitrogenous food.

⁸ *Clinical Lectures on Diseases of the Liver*, 2nd ed., p. 565.

⁹ Headache, Neuralgia, and other Nervous Diseases connected with the Teeth, *Transactions of the Odontological Society of Great Britain*, 1880; and On the Pathology and Treatment of some Forms of Headache, *St. Bartholomew's Hospital Reports*, vol. xix, p. 329, 1883.

THE BACILLUS OF TUBERCLE.

A Speech made during the discussion on Dr. Kidd's paper at the Royal Medical and Chirurgical Society, London, on January 13th, 1885.

By W. WATSON CHEYNE, M.B., F.R.C.S.

Assistant-Surgeon to King's College Hospital.

I do not know, Mr. President, that, as regards the distribution of tubercle-bacilli in tubercular lesions, I have much to add to what I have already published, or to what Dr. Kidd has brought forward; but I have brought specimens to show a point as to the exact relation of the bacillus to the tubercular lesion, which, I think, is of considerable importance. The epithelioid cells of tubercle are now well known, and are always present in the centre of young tubercles. They are readily recognised in specimens stained more especially with methylene-blue, by their large, often oval, nuclei, which present a granular appearance, and are more faintly stained with the blue than the nuclei of the inflammatory cells which surround them. Now, I have always found that the bacilli are more intimately connected with these cells than with any other part of the tubercle, lying among them, and very often in them, and generally nowhere else. Further, I have been able to trace development of these cells into giant-cells in a number of instances. This is, perhaps, best seen in the lung, where these epithelioid cells are, as far as I can judge, developed in most instances directly from the epithelium of the alveoli, as shown in a specimen under the microscope. Certainly this is the case in phthisis. And there, also, the development of giant-cells from these epithelioid cells is often distinctly traceable, and is further evidenced by the fact that the giant-cells often contain a considerable quantity of carbon. Now, this fact of the relation of the bacilli to these cells is, I think, of great importance in searching for bacilli in the lesions of phthisis. Before looking for bacilli in a section of phthisical tissue, I always search with a low power for a collection of epithelioid cells, or of caseating cells, and then look at that place with a high power. This is of especial importance where fibroid changes are marked, for I have never yet found bacilli among this fibrous tissue, nor would I now expect to find them, on the view which I take of the pathology of phthisis. I believe that one reason why we have difference of opinion as to the presence of bacilli in the so-called fibroid phthisis is that search has been made among the fibrous tissue, and not in the real seat of the disease.

One word about the caseation of tubercles. I must confess that, since I have begun to think of the matter, I have become very sceptical about the view that the centre of a tubercle caseates on account of deficient supply of blood; for, among other reasons, degenerative processes are often evident even in the very early stage of a tubercle. Now, the elements which first undergo degeneration, are these epithelioid cells to which I have just referred. These, again, are the especial seat of the tubercle-bacilli. May not the bacilli have something to do with the degeneration? How do the bacilli act in producing a tubercle? It cannot be merely a mechanical action; I consider it most probably a chemical one. I have here photographs of preparations to which I made reference in my recent paper in the *BRITISH MEDICAL JOURNAL* on micrococci. Here you see a dark central mass—a mass of micrococci—a clear ring around, and then, beyond, an inflammatory ring. The course of events is clear; the micrococci, growing in mass, have produced a poison which has killed the tissue around; but further away has been more dilute, and has there set up inflammation. I need not enter into the details given in that paper, nor need I refer to several other known instances, where poisonous substances are produced by micro-organisms. Now, I conceive the state of matters in phthisis to be the following; and, of course, the view as to caseation applies to tubercles anywhere. Bacilli are inhaled into the air-cells of a lung which is in a fit state, whether by predisposition, or otherwise for their growth. They at once attack the epithelial cells, and, in the first instance, cause their hypertrophy and multiplication. The bacilli growing in these cells produce poisonous chemical substances and the cell, which, in the first instance, was stimulated to increased growth by a small quantity of the poison, soon succumbs to the in-

¹ As the accounts of my speech in the reports of the meeting are necessarily very short, and are in several points inaccurate, and as Dr. Creighton's criticism is published, I have thought it well to publish my remarks. The present paper may not be absolutely word for word, but it is as nearly as possible what I said.

HER Majesty the Queen has been graciously pleased to present to the funds of the Booksellers' Provident Institution, through Sir Henry Ponsonby, K.C.B., a donation of £20. Her Majesty has been the patron of this society since the year 1868.

creasing amount, and undergoes caseation. Some cells, or groups of cells, are, however, stronger than the others, and go on growing, so as to form giant-cells. These, generally, also ultimately succumb, though in some instances they may get the upper hand, and the bacilli may disappear. While this is going on, inflammation occurs around, and the process creeps from air-cell to air-cell. I believe, also, that this view of the production of a poisonous substance by the bacilli, may explain the fever and wasting of phthisis.²

I must now turn to another matter, namely, the speech made by Dr. Creighton at the last meeting of the Society. I regret that I was not present at the meeting, but I have received this printed pamphlet,³ which, I understand, contains the essence of that speech.

I will not follow Dr. Creighton in the personal mode of argument which he adopts, for I do not think that the elucidation of scientific facts is aided in that way; and as regards the charge of bad faith brought against Dr. Koch, I think Dr. Koch's character for accuracy is sufficiently well established already to require no defence from me. But it will be convenient if I take the most important statements made by Dr. Creighton, as texts for what I have to say. He states that Dr. Koch has departed from the original principle of the method of cultivation on solid media; that the method employed was not calculated to separate the bacilli from the tubercular material, and that, in fact, they were not separated from it; that cultivations in fluid media would have been better for this purpose; and that, if fluids had been used, the organisms would have grown rapidly, and been no longer motionless rod-shaped bacteria.

Now, in the first place, Dr. Koch has not departed in the cultivation of the tubercle-bacillus from the principle of the method of cultivation on solid substrata. The original principle of solid cultivations is this. Given a material containing only one kind of bacterium, the question is, how to cultivate that bacterium pure, and how to continue the cultivations through any number of generations; for, with whatever care the transference from one cultivating material to another is effected, one is always liable to accidental contamination from the air and from surrounding objects. If one be working with fluids, and another bacterium fall in, the two grow together, and the pure cultivation is spoilt. On the contrary, if a solid substratum be used, the bacteria inoculated only grow where they are planted, and the accidental contamination only grows where it falls, and it is perfectly easy to ascertain which of the growths originated from the material introduced, and to inoculate fresh material from a pure part. [Here various cultivations on potatoes and gelatinised meat-infusion were shown.] Now tubercle is just such a case. By the aid of the microscope, one sees that there is only one sort of bacterium generally present in a tubercle. Here it is not a question of separating one bacterium from another, but of getting and carrying on a pure cultivation of a particular kind of bacteria which we know to be present. The original purpose of the solid method was, as I have shown, to get and keep a cultivation pure, and it is for this purpose that it is employed in the cultivation of the tubercle-bacillus; there is no departure whatever from the original principle.

Everyone now knows that the tubercle-bacillus can be distinguished from all other bacilli as yet known by its peculiar reactions with aniline dyes. The object in making cultivations from tubercle, is, in the first instance, to grow organisms from tubercle which will still present the same staining reactions as the bacilli seen in the tissue, and also to see whether any other organisms will develop. For the purpose of cultivating these bacilli, it was soon found necessary to use a material which would remain solid at the body-temperature, and for this reason coagulated blood-serum was introduced. The simplest way of getting pure cultivations of these bacilli is the following. Inoculate a rabbit or guinea-pig, in the back, with tubercular material, and after some weeks (six or eight weeks), when the animal has become tuberculous, kill it, say with chloroform, as I have always done. Have ready a large number of tubes, containing coagulated blood-serum, perhaps twenty which have been kept in an incubator for a sufficient length of time to see that they are sterile. A large number of knives, scissors, and forceps are also at hand. When the animal is dead, tie it out on its back, clip off the hair of the abdomen, and give the surface a thorough soaking with a watery solution of bichloride of mercury (1-1,000). Heat all the instruments in the flame

and let them cool. Divide the skin of the abdomen with a pure knife, the muscles and peritoneum with another, pull aside the wound with pure hooks, draw forward say the spleen with heated forceps, clip out a tubercle, or part of a tubercle, with pure scissors, pick it up with a pure platinum needle, pass it rapidly into the tube of coagulated blood-serum, and squeeze it out as much as possible over the surface. This process is repeated with tubercles from other places (one of the best is a tuberculous gland) in the other tubes, and then they are all put in an incubator kept at the temperature of the body. Taken in this way from the abdominal cavity of an animal just killed, there are only tubercle-bacilli in the tubercles, and, supposing the constancy of the organisms, only bacilli presenting the peculiar staining reactions of tubercle-bacilli ought to develop; and this is what actually occurs. In about ten days various spots are seen on the surface of the serum; these gradually increase in size, till in about three weeks there is a quantity of whitish crust-like growths at various parts of the serum. Of course, one or two out of the twenty tubes may show other growths, but it is evident that what occurs in the great majority of tubes is the correct result, and that the others are only the unavoidable errors of the experiments, as indeed is further evident by watching where these growths begin, which in the latter case is not necessarily in connection with the tubercle at all.

Now, in inoculating the second tube, one avoids the tubercle which is still evident on the surface of the serum, and takes the outlying growth. A needle is introduced, and an attempt made to pick up a small piece of the growth. The growth is generally somewhat brittle, and when touched by the needle breaks up into a number of little bits, some of which adhere to the needle, are withdrawn with it, and then thoroughly broken up on the surface of the second tube. Here, of course, the bacilli are more thoroughly distributed over the surface of the serum, and thus a much more luxuriant growth is obtained. In future cultivations, one always touches the growing edge of the crusts, and thus gets the most recently developed bacilli, free undoubtedly, after a tube or two, as I shall immediately point out, from the remains of the tubercular material. [Here various tubes of cultivations of tubercle-bacilli were shown, and also cultivations in flat glasses, which were placed under the microscope, and also a tube inoculated in the afternoon, to give an idea of the amount of growth commonly used. Further, a stained specimen, obtained by pressing a cover-glass on the surface of the growth; a number of colonies adhered to the surface of the cover-glass, and were stained by Ehrlich's method.] Now, under these microscopes, you can see what has happened. Spreading out from the place of inoculation, you see a series of small curved or S-shaped masses, which consist of tubercle-bacilli lying side by side, and closely matted together. A mass of these S-shaped growths, closely matted together, forms the crusts which are visible to the naked eye. You will also see that they extend to a considerable distance from the point of inoculation; but I would especially direct your attention to the stained specimen. [Here a cover-glass was pressed down on the surface of the growths, removed, some of the growth adhering to it, and stained by Ehrlich's method.] There you see that all these S-shaped growths are stained red; and you can also see that they are composed of bacilli. But further, you know that, when sputum or tissue are stained in this way, only tubercle-bacilli remain red; everything else comes out blue, but here you see only red stained bacilli; these growths consist only of tubercle-bacilli and of nothing else.

This method of growing in the form of crusts and S-shaped growths is a peculiarity of this bacillus, and is not due to the conditions under which it has been grown. These other tubes will prove this. [Tubes of blood-serum inoculated with the butyric acid bacillus, the typhoid bacillus, the comma-bacillus, and micrococcus tetragenus were also taken to the meeting, and their characteristics pointed out afterwards.]⁴

I will now say a few words on the result of attempted cultivation of these organisms in fluids. An attempt is being made in this country to bring forward cultivations in fluids as better than cultivations in solids. Now, for years before Koch published his methods, I had worked with fluid-cultivations, and I had succeeded in devising modes of avoiding error, which seemed to me to render cultivation in fluids more certain than it had ever been before. One may therefore readily conceive my first feeling of disappointment when Koch's methods were published, which, if his statements were correct, rendered my labour almost entirely futile. And one may readily imagine that it was only after very careful and thorough investigation of the respective merits of the two methods, that I became a supporter of the

² I have seen it remarked that, in tubercles in which caseation had not occurred, although they had apparently existed for some time, bacilli are comparatively few in number, and I have certainly observed this in several instances. The explanation here probably is, that a sufficient number of bacilli are present to cause the formation of the tubercle, but that the cells are stronger, and prevent luxuriant growth of the bacilli, with the result that too small an amount of the poison is produced to cause the caseation of these cells.

³ *Dr. Koch's Method of Cultivating the Micro-organisms in Tubercle*, by Charles Creighton, M.D.

⁴ With regard to this paragraph, want of time prevented me from alluding to this and various other points of interest; but as I showed the tubes after the meeting, I have introduced the paragraph here.

method of cultivating on solid media, though, in my paper on abscesses, I still maintain that fluids have advantages in certain cases. Thus, when I came to work at tubercle, one of the first things I did was to work with fluids, thinking that perhaps Dr. Koch had limited himself to cultivating on solids. Indeed, before Koch published on tubercle at all, I had already planned out a series of experiments on tubercle, in repetition of Schüller's, which would have been begun in earnest in the spring or summer of 1882. But, during the winter of 1881, I had made several preliminary experiments by cultivation in fluids; I had introduced portions of tubercles from tubercular guinea-pigs into various infusions, and kept them at the body-temperature, without in any case getting any development, thus at once disproving Schüller's results. Indeed, anyone who has worked practically at these matters will see at once where Schüller's errors lie. Since the publication of Koch's paper, I have often inoculated tubercles directly into cultivating fluids, but always with the same negative results; although in some instances, where portions of broken-down cheesy glands were introduced, there was slight development at the bottom of the flask; and I have also got slight development where broken-up crusts of growth on blood-serum have been introduced into infusions. In no case, however, was there any turbidity of the fluid, nor any growth of which use could be made. It is a great pity that Dr. Creighton's criticism comes at a time when one is no longer working at this subject, and when, for my own part, I had come to regard the causal relation between the bacilli and tuberculosis as settled; had it come earlier, I could have shown flasks in support of my statements.

But with the fact that these bacilli do not grow in fluids, Dr. Creighton's whole argument falls to the ground. It was not possible to separate the bacilli from the tubercular material in any other way than was done, and the question is, was that method sufficiently satisfactory? I maintain that it was. You have seen, in these little vessels, how the bacilli spread away from the tubercular material, which cannot follow them. When, then, the marginal growths are picked up, it is clear at once that we have got the bacilli free from the tubercular material; and if this process be continued through a number of successive generations, there can be no possible doubt that the tubercular material has been got rid of; and much more certainly got rid of than could be the case by the method of fractional cultivation as described by Dr. Creighton. Thus, let a flask of meat-infusion be inoculated with a crushed tubercle, and let us suppose for one moment that the tubercle-bacilli could develop freely in the fluid. You remove a drop of this fluid for inoculation, but in the fluid are floating bacilli, and also the *débris* of the tubercular material. Who is to say that you do not take a considerable quantity of tubercular *débris*? And who is to say that, in every successive inoculation, you do not take *débris*? In the fluid, the bacteria and the tubercular matter would be floating about together. On the solid, the tubercular matter is fixed, while the bacilli spread away from it, and may be removed without touching the tubercular matter at all. And further, the stained specimens of the growth, to which I have previously alluded, and to which I would earnestly beg your attention, show absolutely that all the tubercular material has been got rid of, and that the growth consists only of tubercle-bacilli; for if any remains of tubercular material were present, I know from practical experience that they would, with this method of staining, have come out blue.⁵ And the experiments on animals with these bacilli show definitely that they are the real virus. Introduce a piece of tubercle and a mass of bacilli of equal size into two animals, and the latter will be far advanced in tuberculosis before the earliest symptoms are evident in the former.

I think, Mr. President, I have said enough to show that the statements which I took as texts are each and all erroneous; that Dr. Koch, in cultivating the tubercle-bacillus, has not departed from the original principle of cultivation on solid media; that the method employed does actually separate the tubercle-bacilli from the tubercular material, and is the best mode which can be employed; that fluid media are unsuitable, because the micro-organisms do not grow, or grow only with difficulty, in it; and that it is not correct to say that, if portions of tubercle be introduced into cultivating fluids, bacteria grow rapidly, and are no longer motionless rod-shaped bacilli. All the assertions I have made can be supported by facts, and to-night I have brought facts before the meeting in support of most of them. I should like to see some facts brought forward by those who take the opposite view; surely they might show us something in support of their statements.

I hope it will not be supposed by the way in which I have spoken,

⁵ It was remarked after the meeting that, if the virus were gaseous or fluid, it would not stain blue. It is, however, generally admitted, and has been proved in a large number of instances, that contagia are particulate.

that I object to fair criticism; far from it. But a purely literary criticism of experiments ought not, I think, to be dogmatic, for dogmatic statements as to what would have occurred if certain things had been done are, in my opinion, only warranted when the critic knows from personal experience that he is correct, or when he quotes from researches about the accuracy of which there can be no possible doubt.

EXCISION OF THE CÆCUM FOR EPITHELIOMA: DEATH ON THE THIRTEENTH DAY.

By WALTER WHITEHEAD, F.R.C.S.E., F.R.S. EDIN.,
Surgeon to the Manchester Royal Infirmary, etc.

I AM not aware that the cæcum has hitherto been removed during life. Mr. John Marshall (*Lancet*, 1882, May 6th and 13th) collected seven cases of colectomy, including one of his own, and he also mentions other attempts which were unsuccessful. With one exception, the descending colon was the site of the operation. Mr. Treves (*BRITISH MEDICAL JOURNAL*, December 16th, p. 1208) has also recorded an instance of resection of two inches of the descending colon for epitheliomatous stricture through an incision in the middle line; the patient died in twelve hours.

In placing on record what I believe to be the first instance of excision of the cæcum, I have felt it my duty to enter more fully into details than a case of more ordinary occurrence would warrant.

The patient was J. W., aged 38, tramcar-driver, married. His father was killed at the age of 62; his mother died when 64 years of age; his nine brothers and sisters are alive and healthy. He had always been healthy himself previously to his present illness; for ten years he was a free drinker, generally taking spirits.

His present disease manifested itself eighteen weeks before admission, with diarrhoea accompanied by pain in the right lumbar region. The pain was paroxysmal, and extended towards the umbilicus, and appeared to be aggravated by a distended bladder or loaded rectum. These symptoms persisted, with only a few consecutive hours of intermission, supposed to be due to remedies taken. At the end of ten weeks, a small tumour, of the size of a walnut, was discovered in the right lumbar region. It felt hard, was freely movable, and free from pain on pressure. The patient at this time was troubled with a constant desire to pass urine, and had to urinate seven or eight times during the night. The urine he described as clear and free from smell. The action of the bowels was irregular; sometimes they would act three or four times in the day, and on other days not at all. The motions were generally pale in colour, and clay-like in consistency. He had no pain on defæcation, and the evacuations never contained either blood, pus, or mucus. The tumour rapidly increased in size, and the paroxysms of pain in frequency and intensity. In eighteen weeks, he lost forty pounds in weight, although his appetite and digestive powers remained fairly good.

He was admitted into the Manchester Royal Infirmary on October 24th, 1884. His complexion was sallow, and he wore an anxious pinched expression. In the right lumbar region there was a mass, about four and a half inches long by three inches broad, and apparently about the same in thickness. The long axis was in the transverse direction. The tumour was free, movable over the whole of the right half of the abdomen, and moved in harmony with respiration. It felt hard and lobulated, was dull on percussion, and slightly tender on pressure. No other enlargement could be felt in the vicinity of the growth. The liver was normal in size. The patient complained of periodic attacks of colicky pains, sometimes of a mild, and at other times of an intense, character. During these attacks, the tumour became much more prominent, and the left half was then found to be tympanitic. There was frequency of micturition; without, however, anything abnormal being detected in the urine. The motions were natural.

The clinical features of the case undoubtedly pointed to a tumour of the colon in the vicinity of the cæcum; and most probably a rapidly growing epithelial new formation. The tumour being movable, it appeared reasonable and justifiable to attempt the excision of the whole disease. It was decided beforehand to make the incision along the outer border of the rectus muscle in preference to that for lumbar colotomy, as it was believed that it would allow greater facilities for dealing with any enlarged mesenteric glands which might be present, and to be better adapted than a median incision for the establishment of an artificial anus. The prudence of this decision was confirmed during the operation, and in the subsequent progress of the case.