tape must then be passed up on the fingers of the operator, and placed on the child's wrist *in utero*. This, of course, is not so easy as when the arm is down in the vagina. For snaring an arm *in utero*, I pass the tape through a tube of flexible metal; the arm is thus more easily caught; and when the noose is securely fixed round the wrist of the child, the arm can be brought down by traction on the tape at any period of the labour. When one arm is thus brought down, the other can easily be secured. The tape must always be sufficiently long that a portion of it hangs down externally after the presenting hand has gone up. Whenever delivery is to terminate head-last, it is always well to adopt this contrivance.

I am desirous of recording this little device, because I believe it is not mentioned in any of the English text-books on obstetric practice, nor is it taught in our midwifery schools. I learned it from my friend Mr. F. Gordon Brown, one of my colleagues in the Royal Maternity Charity. He obtained a knowledge of it while attending the lectures of M. Dubois, in Paris, in 1863.

ANTISEPTIC SURGERY.*

By S. MESSENGER BRADLEY, F.R.C.S.,

Surgeon to the Manchester Royal Infirmary, and Lecturer on Practical Surgery Owens College.

ANTISEPTICITY, like Christianity, has more professing believers than sincere disciples; still it is something for a creed to be so generally accepted in this sceptical age, and my object to-night is not to discuss the value or importance of antiseptic surgery, but rather to criticise some of the methods of carrying it out in practice. We are, perhaps, too much in the habit of confusing antiseptic surgery with Lister's name and Lister's method. Mr. Lister neither discovered the true cause of septicity, nor was he the first to suggest carbolic acid as the most convenient agent for preventing it. He has popularised the subject in the minds of surgeons, and invented an ingenious method of dressing. I do not think that more can be justly claimed for the part he has played. Years before he wrote, Pasteur had investigated and ascertained the true cause of putrefaction; and, amongst other writers prior to Lister on the subject, Jules Lemaire, in a considerable work (*De l'Acide Phénique*), insisted upon the fact that putrefaction was caused by bacteria, and that we had in carbolic acid an efficient and convenient means of destroying these organisms, or rendering them innocuous.

Again, the dry earth system of dressing advocated by Mr. Hewson of Philadelphia, the anhydrous dressings ably handled by Mr. Hamilton and others, the terebene scab-dressing recommended by Mr. Waddy, are all scientifically directed to combat or to prevent septicity. These various agents may, and indeed do, act in different ways, as I shall endeavour by-and-by to explain; but all aim at the same object—the prevention of putrefaction. The essential difference between Lister's position and that of other antiseptists lies in this: that he claims for his method an infallible and invariable result.

Lister does not deny that other plans may occasionally succeed; but he does deny to them the certainty of success which he claims for his own. He may be represented not unfairly as saying: "Of course, a patient dressed according to my method may die; he may die of hæmorrhage, or of shock, or of embolism, etc.; but he shall not die of putrefaction or any of its effects, such as septicæmia." Now, gentlemen, the object of this paper briefly is to consider whether Lister's plan is entitled to this unique position.

And here, I think, it becomes necessary for us to discuss a little in detail the true cause and nature of putrefaction; for, though putrefaction does not necessarily include septic poisoning, no form of septic poisoning can occur without previous putrefaction.

There are two theories by which putrefaction is explained: the chemical theory and the germ-theory. The chemical theory is thus described by Neudorfer, from whose recent work, *Die chirurgische Behandlung der Wunden*, I make the following free translation. "Such albuminous bodies as blood, serum, and lymph possess a very unstable chemical constitution, and are only capable of resisting change whilst contained within their proper vessels. As soon as these boundaries are passed, they undergo molecular change, fermentation, and decomposition. As excitors of these changes, we must regard:—1. All altered albuminous bodies which belong to the animal kingdom, since they have the property of inducing this molecular change and decomposition in unchanged albuminous bodies with which they come in contact; 2. Altered albuminous bodies met with in the vegetable

kingdom, such as lint, cotton, charpie, and other surgical appliances; 3. Water, even distilled water, coming into contact with albuminous matter, induces the changes above referred to; 4. Mechanical causes, such as blows, etc.; 5. Dynamic causes, like pain, etc." (Page 154.) Such, shortly, is the chemical theory of putrefaction, concerning which I shall content myself with saying that this explanation is no explanation at all, since the changes referred to by Neudorfer never occur unless there is some means of access for the air. Keep out the air, and these unstable albuminous bodies never do putrefy. They may escape from their boundaries; they do escape every day in simple fractures and other subcutaneous injuries, yet we confidently predict that there will be no putrefaction; for we know that, without a *primum movens*, these fluids are incapable of change. Neudorfer himself admits as much when he speaks of these changes being excited by other albuminous bodies which have themselves already undergone a change. This fact led observers to examine the air, and to inquire whether it was the air itself, or something in the air, which induced the putrefactive change; and ere long it was discovered that, if the air were carefully filtered, putrefaction never occurred. The search for this *tertium quid* led on to the detection and indictment of bacteria as the *vera causa* of putrefaction. This naturally leads me, then, to consider the second theory of which I spoke; viz., the germ-theory of putrefaction. It is true that as yet there is a link missing. We know, from the beautiful experiments of Tyndall, that air optically pure is incapable of producing putrefaction; we know that, wherever there is putrefaction, there are bacteria; but the origin of these organisms, the germs of the bacteria, are beyond our ken, and, from what I know of the matter, are likely to remain so. This, however, no more proves their non-existence than the apparent identity of embryological cells proves their similarity: the facts equally point to the conclusion that our vision, even aided with the microscope, is very limited.

Permit me to dwell for a few moments upon certain points in the life-history of these little organisms. And, first, I would remark that, like the higher animals, they are, a mixed lot; sheep and goats are to be found amongst them. Of them, it may be truly said, "By their fruits shall ye know them"; for, while some are innocuous to the human system, others are lethal, giving rise to various specific diseases. Already the microscope has done much towards branding the various kinds of bacteria; much more, however, remains to be done in this direction.

The important fact for us to bear in mind in connection with the matter in hand is this: that, while some are harmless and others are harmful, all alike are capable of producing putrefactive change. Such tiny things as bacteria are, of course, difficult bodies to study; but we have learnt some important principles of their mode of living: *inter alia*, we know that they multiply exceedingly; we know that they prefer swimming to flying; we know what will kill them and what will keep them alive, and, of course, it is the study of this last fact that has led to the elaboration of antiseptic surgery. Bacteria depend, like we do, for their sustenance upon oxygen; but, like us again, they can only bear a certain quantity; too much kills them, and, for this reason, ozone is a powerful germicide and true antiseptic. I will, with your permission, refer to an experiment with this body, which I made last year in company with Mr. Harrison.

On February 5th, 1876, we took these two bottles, each containing some turnip-water just passing into putrescence; through the one, a stream of ozone was passed; in the other, the air was left untouched. Through each cork, bent tubes were inserted. These bottles are now before you. The fluid through which the ozone was passed is little changed; the fluid in the other is green and thick. I have little doubt that the turbidity of the ozonised solution would have been quite obviated, if, instead of corks, we had used plugs of cotton-wool, and so filtered the air which subsequently found ingress to our bottle. This experiment may, however, be taken as sufficient evidence of the antiseptic properties of ozone, and it is by virtue of their ozone-evolving properties that some agents employed in surgery have antiseptic powers; for example, Dr. Angus Smith informs me that all the essential oils give off ozone, and he has proved that they all possess antiputrescent properties. Of these, he tells me the oil of mustard, the oil of bitter almonds, and the oil of the eucalyptus globulus are the most powerful; and with these I intend to experiment. From recent investigation, indeed, it would appear that the eucalyptus globulus (I mean the tree itself) really merits its character of malaria-killer. May we not fairly infer that this is due to the ozone given off from its fragrant leaves? It has sometimes occurred to me that the sacred groves of Scripture had a similar value; if so, we may readily understand that the heathen's god would avenge the desecration of his temple, when these groves were destroyed, by sweeping as a pestilence over the land. Turpentine owes its antiseptic properties

* Read before the Manchester Medical Society.

to a similar cause. I do not know whether this is true of tincture of benzoin, but think it very likely.*

So much for the effects of too much oxygen upon bacteria; destruction equally awaits them if they get too little; by the one process, they are killed; by the other, they are starved: *voilà la différence*. It is on this latter account that all hydrocarbons, such as the fixed oils, are antiseptic, and, therefore, good surgical dressings; likewise it is by starvation, I imagine, that the anhydrous system of dressing wounds secures such immunity from putrefaction.

Turning to germicides proper, we find many substances capable of killing bacteria; boracic acid, iodine, and carbolic acid are the most in vogue. I do not know, nor do I think that it is accurately ascertained, in what proportions these substances kill bacteria; suffice it to say that, from my own experiments, I differ from Lister in saying that the No. 20 solution of carbolic acid (*i.e.*, 5 per cent. of the acid) is "as certain death to them as the gas-flame".

Before passing on to criticise Lister's special method of dressing, I wish to mention one other phenomenon attending the process of putrefaction. I refer to the evolution of ammonia. This may be taken as constant. It is true that Dr. Angus Smith, who most kindly has given me much information upon the subject of putrefaction, informs me that the presence of ammonia is not an absolute proof of putrefaction, although it comes very near it; on the other hand, he says, "its absence is to be regarded as proof positive of the absence of putrefaction". Be its origin what it may, we have these two capital facts apart from all theories: putrefaction is invariably associated with the presence of bacteria; "surgical" putrefaction is invariably accompanied by the evolution of ammonia; the microscope reveals to us the presence of the one, chemistry the presence of the other.

I am inclined to think we may go further, and say that, where we have ammonia, there we shall find bacteria; where we find bacteria, there we shall find ammonia; and, the contrary to these two propositions, where there is no ammonia there are no bacteria, and where no bacteria are present there is no ammonia. To place this entire statement, however, on the basis of proved problems, further research is needed.

I now desire to address myself more specially to that method of practising antiseptic surgery introduced by Lister, with the direct purpose of seeing if its claims to the title "antiseptic" are pre-eminent; and, first, I would inquire whether it is correct to say that this plan always defies putrefaction. It may not go for much to say that, in my hands, it has occasionally, like other plans, failed to do so; but it is a good deal to say that it fails in the hands of such a master of all the details as Mr. Lund, yet such is the case; I do not say often; I do not know whether often or seldom; but, even in his skilful and careful hands, Listerism will not always succeed in warding off putrefaction. Some little time ago, Mr. Lund undertook an operation for the radical cure of hernia. I looked upon this operation with interest, as affording an excellent proof of the value of Lister's method; and, having shortly before had a case of hernia, where I had removed a large quantity of omentum, and where putrefaction had followed, I was almost prepared to accept the superiority of Lister's plan on the score of success in this case alone. I stated as much beforehand to Mr. Whitehead, and asked Mr. Lund, at the time of operation, if he would kindly let me examine the discharges following the operation, to which request he at once acceded. The operation was a long and serious one, involving the removal of a considerable portion of omentum, and the stitching together of the pillars of the external abdominal ring. Mr. Lund himself dressed the wound on every subsequent occasion. I did not think it worth while to examine the discharge for the first week or so; but, about the eighth day, I was present at the dressing, and, at my request, Mr. Lund touched the microscope-slide I held ready with the end of the drainage-tube, which he drew from the wound. At this time, the incision had nearly healed, and the wound looked very clean and healthy, though I thought there was some detectable fetor about the pus; of this, however, it was difficult to speak, owing to the prevailing odour of carbolic acid volatilised in the spray. Under the microscope, the discharge was found to swarm with bacteria, and the patient died some days later of septicæmia. Now, gentlemen, although this is but a single case, it serves sufficiently my purpose to prove that there is no infallibility about Listerism. To admit that it does fail from time to time, even in the hands of so perfect a manipulator as Mr. Lund, is to admit that it must fail, and, therefore, its claims to our acceptance as the one true method must be based upon other grounds than its universal success.

To what, then, do we turn? Naturally, to statistics; but here we

are met with a difficulty. Lister himself, so far as I know, has not published his statistics, and so we must be content to compare the statistics of his followers with those of surgeons who dress on other plans. Well, gentlemen, I do not think that any marked superiority is to be made out for Lister on this score; *e.g.*, the results obtained by Callender are fairly equal to those of any follower of Lister. I would crave permission, by way of further information on this head, to quote my own statistics of amputations and excisions from January 1876 up to the present date, during which time I have employed open dressings, according to the method shortly to be described.

During the period mentioned, I find that I have had fifty-five amputations and excisions, with the following results. I only quote cases under my care at the Infirmary.

Amputations.	Recovered.	Died.	Total.
Forearm	3 ..	0 ..	3 ..
Elbow-joint	0 ..	1 ..	1 ..
Arm	4 ..	0 ..	4 ..
Shoulder-joint ..	1 ..	0 ..	1 ..
Hip-joint	1 ..	0 ..	1 ..
Thigh	12 ..	3 ..	15 ..
Leg	2 ..	0 ..	2 ..
Ankle	8 ..	0 ..	8 ..
Excisions.			
Elbow	8 ..	0 ..	8 ..
Hip	2 ..	0 ..	2 ..
Knee	4 ..	1 ..	5 ..
Wrist	2 ..	0 ..	2 ..
Removal of loose cartilage from knee	1 ..	0 ..	1 ..
Operation for ununited fracture of forearm.	1 ..	0 ..	1 ..
Total	50 ..	5 ..	55*

The important question here arises, Were any of these five deaths attributable to putrefaction or its septic consequences? and to this query I confidently reply, No, not one. The death following amputation at the elbow-joint occurred on the second day following the operation, and was distinctly due to shock, the patient, an old toper, never rallying. Of the three deaths following amputation of the thigh, one was the case of a woman, whose thigh I amputated, on account of gangrene sequential to typhoid fever, and in whose limb the artery was found blocked right up into the groin; the second was a case of diffuse aneurism, in which I had previously tied the common femoral and afterwards amputated; the man died of hæmorrhage, but the stump had healed; the third was an old Irish woman aged 72, who made a good recovery up to the third week, the stump being nearly healed, when she received bad news from home and sank. The death following excision of the knee was in a little girl of two years of age. She became sufficiently convalescent from the operation to be sent to Cheadle; but I afterwards learned that she died there from, I believe, diathetic causes. Such are the statistics I have to submit to you; but you may say: Well, you have a fair percentage of recoveries; but have you not exposed your patients to greater risk than you would have done had you dressed them *à la Lister*? To this question, I scarcely know how to reply better than by submitting the temperature-charts of each patient; this, indeed, I hoped to do; but some have been lost and others I never received; what I have, however, I place before you, and I think I may say that, so far as they go, they afford no evidence of septicæmia, nor, I think, will you find amongst them one which shows an unnaturally high febrile condition as consequent upon any of these capital operations.

So much for the infallibility claim and the statistical question. I now come to positive objections to Lister's method, chief amongst which I place my objections to the spray. My objections to the spray are partly theoretical and partly practical. For some time past, I have thought it probable that the spray, instead of keeping off bacteria, really drove them into the wound; creating currents and vortices in the air, it seemed likely to me that the skirts of these little whirlwinds would be caught, and any contained microzymes entangled and driven along the lines of spray; for be it remembered the spray is not a continuous sheet of vapour, but only a good thick douche, with plenty of room for thousands of bacteria to dance between each and every atom. Many of you have seen, in the experiments with vibrating sand-plates, how the light lycopodium is caught and held in the very centre of the sites of greatest vibration, while the heavier sand is driven through into the lines of rest; similarly, I thought the bacteria might be trapped by the very means adopted to keep them at a distance.

Again, I thought the water mixed with the carbolic acid might be a fertile source of mischief; it is true the solution used is a strong one; but, as I have before mentioned, I have reason to question Lister's

* Perfumes may thus come to play a part beyond "my lady's chamber"; and to surround a wound with attar of roses would be not merely æsthetical, but practical antisepticity.

* I take this entry from my own case-book. On referring to the record of our surgical registrar, I find myself accredited with the case of a lad who died of septicæmia following amputation of the thigh. Though I have no recollection of the case, I take it to be a correct report, and wish the case to be considered along with the rest.

dictum as to the fatal effect of this No. 20 solution, as it is called, upon the lives of bacteria. Over and over again, I have seen bacteria moving as vigorously as before, after two hours' immersion in this solution. Holding these opinions, I was anxious to test their accuracy, and, therefore, I performed the following experiment.

On October 20th, 1877—that is, six weeks ago—I cut off two pieces of muscle from the leg of a girl whose thigh I had just previously amputated. One piece was removed under the spray, placed upon this board, quite new, and wiped with a sponge soaked in No. 20 carbolic acid solution, and dressed strictly according to Lister's plan. The other piece of muscle was cut off in the common air and dressed with lint saturated with glycerine and carbolic acid, in the proportion of one part of glycerine to eleven parts of carbolic acid. From that date to this, my dresser (Mr. French) has taken charge of these specimens, and dressed them each on the method first adopted about every third or fourth day. Last Friday but one, I saw them for the last time until to-night. I found the muscle dressed *à la* Lister whitish in colour, flabby, and soft; the other dark in colour, firm, dry, and semitransparent. I then Nesslerised both specimens, and found that the one dressed with the spray gave ammonia, and was therefore putrefactive, while the other gave no ammonia, and was therefore sound. Mr. French is present, and can verify what I state. I will now repeat this experiment before you, and we will note the result.

You perceive that this specimen, the Listerised one, is, as I said, pale, soft, and watery; the other is dry, firm, and nearer muscle in colour. Cutting off a piece from each, and dropping them into these portions of Nessler's solution, which you know is an exceedingly delicate test for ammonia, you notice that the first gives a brown colour, and thereby shows the presence of ammonia; the other fluid remains uncoloured, and thereby proves that no ammonia is present.* I desire to be clearly understood in reference to the bearing of this experiment upon my argument. In my opinion, it demonstrates accurately the evil influence of the spray; but it by no means proves that these exact conditions are likely to be, or even can possibly be, imitated by so treating the living organism. You cannot, in the living subject, prevent the constant access of fresh fluids to the part, and so strictly anhydrous dressing becomes an impossibility; but it is no reason because all water cannot be excluded that we should not keep as much away as possible. It must always be remembered that two factors are needed to produce putrefaction: the seed and the soil. We will suppose, if you like, that the seed is ever present, although bacteria are much more aquatic than aerial in their habits; still, granting the presence of the seed, it will remain as the seed in the hand of the mummy, unchanged by time, until it is awakened into life by the proper soil.

It is perfectly true that it is wiser and easier, to borrow an expression of Mr. Lund's, to be *antiseptic* than *antisepic*; but, for all this, it is interesting to notice how materially you will lessen a discharge, both in quantity and putrescence, by stopping the use of water in the dressings, and mopping up the secretions with dry lint. It is, therefore, perfectly consistent and sensible to dress as nearly anhydrously as possible, although we know that the animal juices themselves form a suitable pabulum for the growth of bacteria. A more curious fact than this is connected with the influence of these organisms upon the human body, which, I confess, I am totally at a loss to understand. I refer to the fact that the effect, the septic effect, produced is in direct proportion to the quantity introduced. Recalling the extraordinary powers of multiplication of bacteria, it would seem to be a matter of indifference whether one or a million were introduced into the system; not so, however; the severity of the septicæmia is proportionate to the numbers of bacteria. The most evident proof of this is conveyed by the results of vaccination, which may be regarded as a sort of benign septicæmia, one vesicle producing very little prophylactic effects on the system as regards variola, while four or five bestow an almost absolute immunity; perhaps it is less familiar, but it is, I believe, equally true, that exactly the same holds good with what, by contrast, may be termed the lethal forms of septicæmia. These two facts taken together teach us to keep a wound as free from discharge as possible, and as dry as possible.

So much for my objections to the spray. My next objection to Lister's method is, that it tends to cover up and keep confined putrefactive matter. If you must have putrefaction, by all means have it externally; it does little or no harm, if it have free vent. Whoever heard of septicæmia caused by an ulcerated leg? Yet all of us have had unpleasant experience of ulcers so foul, of putrefying sores so extensive, that they would poison an army, if the matter were properly economised; yet the owners of these dreadful legs live and move and have

their being for years and years, with a normal temperature, a clean tongue, and a healthy appetite. One of the members here present has given me permission to mention a little circumstance which is quite *à propos*. His little girl had a trifling sore upon the eyelid, which, one day last week, scabbed over, soon after which she became feverish and ill. This state of things continued for two days, when her father removed the scab and gave exit to a few drops of pus; directly after this, she felt better, her temperature fell to normal, and, in an hour or two, she was quite well. Now, Lister's dressing is, I believe, apt to lead us into a fool's paradise: everything looks so nice and is so clean, that it is hard to believe any mischief can be lurking there, and so, while our noses are effectually hindered from detecting anything wrong by the cloud of carbolic spray, we confidently reswathe the part in the voluminous folds of gauze, etc., and perhaps leave it for a couple of days or more, during which the evil is wrought, and that may be beyond repair. It is not correct to say the drainage-tube sufficiently guards against a *contretemps* of this kind; drainage-tubes only very imperfectly guard against such accidents; it is common enough to see a stream of matter follow the withdrawal of the drainage-tube, which the latter has evidently only served to keep confined. To obviate this cork-like action, I suggested the use of vulcanite tubes which were incompressible; but, though better than the others, I cannot say they are perfect. Chiene's catgut method of drainage, which acts by capillary attraction, is perhaps the most inefficient of all. By way of illustrating this part of my argument, let me remind you of Mr. Lund's case of radical cure, which, to some extent, is the "motive", as artists say, of this criticism. His patient dies of septicæmia, of which there is no outward and visible sign until the microscope is consulted, yet who will venture to say that this had not been going on for some days before detection? Again, let me recall my own case of femoral hernia, which I mentioned as having occurred about the same time, and where I had removed a large piece, a pound or more, of omentum. I do not mean to say that my case was at all comparable in gravity to his; but what I do say is this: putrefaction occurred in both; but, in my own case, the incision was kept freely open, the dressings were changed twice a day, and the patient recovered, without any septicæmic symptoms.

There are other objections to Lister's method, such as its greater expense and the inconvenience of having to keep a large stock of what may be looked upon as patent dressings on hand, procurable only through a few channels; but these are small matters compared with the two graver charges mentioned above, and with them I shall not further deal, but pass on to speak of the mode I myself employ.

It is unnecessary for me to say there is nothing original about my plan; it is necessary, however, that I describe it. In the first place, I thoroughly cleanse the skin with carbolic soap, even rubbing off the outer layer of cuticle, thus removing all clinging germs. I take care that all instruments, etc., are perfectly clean. After performing the operation, I fill the wound with some antiseptic, generally selecting No. 20 solution of carbolic acid, although many other preparations, such as chloride of zinc, or sulphate of iron, or nitrate of silver, seem to possess equal germicidal powers. I then take pains to render the wound as dry as possible, after which the sutures are introduced and the wound dressed with four or five folds of lint thoroughly well saturated in a mixture of carbolic acid and glycerine. Over all, I place either a pledget of carbolised tow, or dry lint, or cotton-wool. If the subsequent discharge be abundant, I at once liberate some of the sutures, to give free vent to the discharge, and have the wound dressed at least twice a day, otherwise the wound is dressed once a day. I carefully avoid using water in any shape or form, squeezing the matter out, not syringing it out, and wiping with dry, not with wet, lint. It will easily be understood that there are cases where the position of the pus, etc., renders it impossible to remove it altogether by pressure; this, for instance, is the case when the knee joint, or indeed any joint except the hip-joint, is opened; then it becomes necessary to wash out the discharge with the syringe. When this has to be done, I am in the habit of using a solution of permanganate of potash in preference to carbolic acid, and take care that the syringing is thorough, and performed twice in the twenty-four hours, so as to give little time for decomposition to take place. Irrigation in this case is good; indeed, is excellent; but it is somewhat messy and involves some exposure to cold, which is not always desirable or safe. There are two other conditions where the syringe comes into play, which I may take this opportunity of alluding to; I refer to the treatment of sinuses and abscesses. Speaking of the first, and supposing that all dead bone, etc., is removed from the bottom of the sinus, and that the granulation-tissue round the orifice is removed with the knife or scissors, I believe the best plan to secure closure of the entire sinus is to thoroughly flush it every day with a mixture of tincture of iodine and water (one to seven). Abscesses

* I ought to state that on the Friday mentioned above I did not direct Mr. French to uncover or recover the Listerised specimen under the spray; therefore the experiment before the meeting: did not be taken into account

are, in my opinion, much better and more successfully treated on Calender's plan of superdistension, especially if pressure be exercised afterwards, than by the method of Lister. This at least is my experience, which in this respect, I believe, is in accord with that of some of my colleagues. You, of course, will ask me what proof I have that such a plan as the one I have detailed merits the term antiseptic equally with Lister's, and my answer is: I have given you my statistics, which, I think, will bear comparison with those of his followers; but I chiefly rely upon experimental proof.

You will please to bear in mind the two mutually supporting evidences we have of putrefaction: the presence of bacteria and the exhalation of ammonia. I make no mention of such questionable evidence as the smell or colour of the discharge.

Well, gentlemen, submitting my cases to this double test, I believe I am entitled to say that I practise antiseptic surgery; e.g., among several similar experiments, permit me to mention the following. A fortnight ago, I took the first four operation cases in the female wards, and, with two of my dressers (Mr. Challinor and Mr. Pownall), examined the discharges by the double test of the microscope and Nessler's solution. The cases chanced to be an amputation of the thigh performed three weeks before, a carcinoma on the chest, a lipoma of the neck, and an excision of the elbow, all operated on ten days prior to the examination. Besides these, we examined a case of ligature of the subclavian artery with another of my dressers, Mr. Williams, in the men's wards, of a week's standing. In none of these were bacteria to be found, and in none was ammonia to be detected. But understand my position: I do not claim for this plan that it will certainly prevent putrefaction; for I know that, in some cases, putrefaction does occur, and, in my opinion, do what we will, ever will occur from time to time. What I allege of it is, that it is just as efficient as the more costly and cumbersome plan of Lister; and, in my opinion, this is no small boon. To the army surgeon, toiling in such a terrible war as is now raging, the simpler method I advocate is surely incomparably better, inasmuch as it is practical, while the other is not. But, even to the surgeon engaged in hospital and general practice, the advantage of simplifying Lister's plan is considerable; for I am daily more and more convinced that such matters as the mysterious spray, and the patent character of the dressings, act as a real hindrance to the comprehension and general adoption of the true principles of antiseptic surgery.

OBSTETRIC MEMORANDA.

THE TREATMENT OF OBSTINATE VOMITING OF PREGNANCY BY DR. COPEMAN'S METHOD.

ON the evening of January 25th, 1878, Mr. X. called on me, to ask me to go next day some distance into the country to see his wife, a lady aged 33, who, he said, was about two months gone in pregnancy and was troubled with constant vomiting, which had continued for the two months, and for which she had been treated by her former medical man, going up to London to stay on purpose to see him (she had lived in London till about four months since). Since her return home, however, she had become so much worse that her doctor advised her to get some one near to see her.

On the morning of January 26th, I went to see her, and found that she had never been strong; she had had five children and five miscarriages. She was somewhat stout; of strumous diathesis. It was a habit of hers to vomit on the slightest provocation; bad news would make her vomit, also her monthly periods. Four years since, the vomiting was so severe that, when she had reached eight months in pregnancy, she had become so exhausted that, as a last resource, under consultation, premature labour was procured. She had vomited during the last two months, and throughout the day and night for the last week, having no respite whatever. Once she had fainted, and several times had been quite prostrate and cold. She could keep nothing down at all; vomited every four minutes food or mucus, and, if there were nothing to bring up, simply retched violently. She looked much worn; pulse 120 (normally a quick pulse) and very feeble. I prescribed some oxalate of cerium in pill, and the usual diet in very small quantities, often repeated, and gave her an opiate at night to try to obtain some rest from the sickness. I promised to see her again in two days, so as to give the remedies a sufficient trial. On January 27th, however, at twelve o'clock, an urgent messenger arrived to summon me directly. I took with me all I needed to procure a miscarriage. I found her very prostrate; vomiting incessantly every few minutes through the day and night; she had vomited just before I entered her room, and vomited

twice during the few minutes I was then talking to her. She was so ill, and her husband had such unpleasant recollections of her former illness, when she was almost *in extremis* before premature labour was induced, that I obtained his consent to do what I thought necessary. On proceeding to examine the os, I found it very hard and cartilaginous, but I dilated it with my finger as well as I could. I then left her, begging her to take the nourishment I indicated, and prescribed some bromide of potassium, but with the understanding that I should be fetched if urgent symptoms appeared. Not expecting success, I asked to be allowed to obtain a skilled nurse for her, and made arrangements to go next day. On January 28th, I arrived at three P.M., and was rejoiced to find that, from the time I left on the previous day at two P.M., the sickness had ceased. She had eaten and slept well. This morning, she vomited once when she first put her foot to the ground for her bed to be made, but not since. She had eaten a mutton-cutlet for her lunch at one o'clock.

January 30th. I did not see her at all yesterday; but ascertained, on visiting her this morning, that she had had no return of sickness whatever, and only retched once when she first got up to dress this morning. She is able to be up and to eat fairly well, and, as the sun was shining brightly, I ordered her a little drive out; and regarding her as convalescent, at all events for the time, I have ceased to attend.

CLEMENT DUKES, M.D., B.S.,

Medical Officer to Rugby School, etc.

POST PARTUM HÆMORRHAGE.

VARIOUS remedies have from time to time been recommended in the JOURNAL for the treatment of *post partum* hæmorrhage; but I do not remember to have seen large doses of turpentine ever advocated. Some time since, when discussing the subject with a friend, he told me he knew an old practitioner of large midwifery experience who never went to a case without taking two draughts, each containing half an ounce of spirit of turpentine; and he never knew them fail.

On February 10th, I attended a Mrs. W., aged 36, a stout woman, but of loose flabby muscular fibre. The labour progressed favourably, and the placenta came away about five minutes after the child was born. I had previously given a full dose of ergot (as she flooded last time), and kept my hand upon the uterus externally. In a few minutes, flooding commenced profusely, and continued notwithstanding the use of ergot, cold-water injections, kneading the uterus, removing clots, and keeping my hand as a plug, etc. The uterus would contract a little, then become again relaxed; the hæmorrhage continuing as badly as ever. She became cold and almost pulseless. At this stage, I gave half an ounce of turpentine beaten up with an egg; and in less than five minutes I felt the uterus contracting. It soon became firm, hard as a ball, and all flooding ceased. The patient rallied very quickly, and from that time has progressed most satisfactorily. I therefore consider the treatment by turpentine worthy of a trial, and shall hope to see the result of other cases recorded, as I feel convinced that the remedy is a good one, and to be relied upon.

JAMES POLLARD, Torquay.

THERAPEUTIC MEMORANDA.

TREATMENT OF DELIRIUM TREMENS.

SURGEON-MAJOR WILLIS's note on the treatment of this disease with thirty-grain doses of powdered capsicum is interesting and of much practical value. But on what therapeutic principle are we to account for the capsicum having so powerful an effect? I have recently had similar effects with less than five-drop doses of the tincture of capsicum. Can it be that in both cases the capsicum, as an acceptable and innocent substitute for alcohol, merely put the long splint, so to speak, on the patient's stomach and nervous system, thus securing his quiet acquiescence in what was for the time the *rest of total abstinence*? I confess it was with this impression that I administered the drug; but some special features of the case, and a desire to present to the patient a palatable beverage as nearly as possible in the form of his accustomed tipple, suggested the following prescription.

℞ Sodæ phosphatis 3j; syrapi ferri phosphatis, syrapi limonis, āā ʒiiss; mucilaginis acaciæ ʒij; tincturæ cardamom. co. ʒj; aquæ ad ʒxxiv. M.

Sig.—*The wine of capsicum.* A wineglassful as required. This was put in a common wine-bottle, and was much relished by the patient, who took it at once, and next day passed into a refreshing unbroken sleep of nearly twenty-four hours' duration.

D. TOLMY MASSON, M.D., Edinburgh.