

in which sponges have been left behind, so that I need no excuse for the emphasis of my advice on this point.

The sponges used should be of the very best quality, should vary somewhat in size and shape, and should be perfectly free from tears or ragged points, from which pieces may become detached. When they are new I soak them for twenty-four hours in a solution of muriatic acid sufficiently strong to be disagreeably sour to the taste. This dissolves the particles of chalk with which they are infested and loosens the sand, and this must be completely washed out of them. After each operation I wash them free from color and then soak them for forty-eight hours in a strong solution of washing-soda or ammonia, to dissolve the fibrin. They are then washed repeatedly until the water comes from them perfectly clean, and after that they are placed for a week in a five per cent. solution of phenol. Finally, they are hung up in a well-made calico bag in a warm place till they are quite dry. I keep a very large stock of sponges, and they are a constant source of anxiety and care. It is to Dr. Keith that we are indebted for the free and efficient sponging out of the abdomen now always practised, and in this respect he has again largely contributed to the advanced success of abdominal surgery.

It hardly requires to be said that the selection of an anæsthetic is an important matter for the success of ovariectomy. By common consent the agent which for so many years held a supreme position in this country has given way to that which was first introduced for the purpose of procuring unconsciousness to pain in surgical operations. Chloroform, while it acts with an almost mathematical certainty upon a woman in labor, is a most unsafe and uncertain drug to use in surgical operations, save in the case of a very young child. It is not only that we have a considerable number of deaths from chloroform, but that in its use we have so much anxiety that the danger is ever present in the mind of the operator, and this distraction is destructive of that clear concentration which is necessary in the performance of ovariectomy.

Mr. Spencer Wells has for a long time advocated the use of the bichloride of methylene by means of Junker's apparatus, and this is certainly a very elegant, rapid, and, compared with chloroform, safe means of inducing anæsthesia. There are, however, two objections against its use; one is that it requires a special apparatus, and the other that this apparatus requires some one experienced in its use. In the hands of one who is accustomed to give the bichloride I think it is probably as safe as any anæsthetic well can be, but as it is wholly impossible always to obtain the services of one sufficiently skilled, I have for a long time dis-

continued the use of this agent. There are other reasons against its use which I do not think of sufficient importance to dwell upon. The agent I have employed for the last six years is free from any objection of an important character. I allude, of course, to sulphuric ether, which is now recognized as by far the safest anæsthetic in use, the kind of ether which I employ and which I prefer above all others being the anhydrous methylated ether manufactured by MacFarlane & Co., of Edinburgh, as originally recommended by Dr. Keith. The advantages of this agent are very numerous. In the first place, so far as its present use has gone, it is absolutely safe; in my own practice it has been used, on a rough estimate, between five and six thousand times, and not only have I had no accident from it, but its use gives me no anxiety whatever, and whilst I am engaged in operating my mind is absolutely at rest concerning the anæsthetic.¹ In the next place, it may be administered by any one, and when I say that in the whole of my hospital practice, for nearly three years, the ether has been administered for all operations by the sister in charge or by a nurse, I think I give testimony sufficient as to my confidence in the safety of the drug.

For its proper administration very few rules need be observed and no kind of special apparatus is needed. It is always given for me after the simple fashion which Sir James Simpson introduced for the administration of chloroform, that is by dropping it on the outside of a single fold of a towel, laid upon the patient's face. Bearing in mind that ether is extremely volatile, and that its vapor is very heavy, the following directions must be attended to. The towel used must not be too thin, because it must retain a sufficient body of ether for the continuance of the current of vapor; and yet it must not be so thick as to prevent the passage of air freely through it. The ether must be dropped on to the towel, not splashed on, but administered in a continuous stream, which must be allowed to drop from a small orifice on to the towel, above the level of the patient's nose, because the vapor of the ether will fall like a cataract over the patient's face. If the ether is dropped on the towel on a level with the patient's mouth, she will inhale, not the vapor, but a mixture of air and ether, which will act as a stimulant and not as an anæsthetic, and the ether must not be splashed on, for exactly the same reason. The towel should not be tightened over the face, but puffed out around it at a distance of an inch, or an inch and a half, from the skin, in order that it may enclose a body of vapor. The

¹ Since writing this I have had an accident (British Medical Journal, July 14, 1882) due, as I think, to the use of an inhaler.

whole of the piece of towel covering the face must be kept continually moist with ether, and in this way a continuous volume of pure ether vapor will be inhaled by the patient. After a few minutes the part of the towel in use must be changed for another part, because anhydrous ether absorbs with intense avidity the moisture of the breath, and the towel will be found coated with ice, and this, by its interference with rapid evaporation, prolongs the process: this is the chief argument against all ether inhalers. Only one other caution need be given, and that is to avoid bringing any light or red-hot cautery near the patient's face while the ether is being given, for it is explosive.

When these rules are attended to, ether will be found to be the safest, quickest, and most satisfactory of all anæsthetic agents. It has only three objections, and these are of but slight importance. The first objection is that the smell of the ether hangs about the clothes of those engaged in the operation, and this is offensive to many persons; the second is that the quantity of ether used is much larger than that of any other anæsthetic and therefore it is necessary to keep it in large bulk; and the third objection is that the taste of the ether is by no means so pleasant as either chloroform or methylene; but upon this point I have heard considerable difference of opinion. The cost of the ether is less than that of any other anæsthetic which I have used.

During the administration of ether there should be absolute silence in the room. No one should be allowed to talk; more particularly, no allusion should be made to the patient, or to the prospects of the operation, because for a long time after insensibility has apparently been reached ideas may be picked up by the patient from suggestions made at the bedside, and these often have a lasting and most disagreeable effect. This was one of Simpson's most stringent rules, and one which never ought to be broken. At the beginning of the process the quantity of ether should be small, in order to avoid giving the patient the sensation of being suffocated, because it is at this stage that the state of mind is induced which leads to struggling. Nothing makes any one struggle more than the sense of being choked. As soon as it is evident from the regularity of breathing that the patient has become accustomed to the presence of the ether vapor in the air the quantity given may be increased to that which has been already indicated, and if this condition be attended to it will be found that struggling will be altogether avoided, save in the case of children. When the patient does struggle, the only thing to be avoided is her getting hold of the towel, and it will generally be quite enough for the nurse or as-

sistant to keep the hands quiet while the administrator of the ether steadies the head. Whoever gives the ether should absolutely mind his or her own business, and take no interest whatever in the surroundings during the whole of the process; for even with an anæsthetic so safe as ether it is absolutely necessary that the whole attention should be engrossed with it.

Sickness is with ether, as with every other anæsthetic, a somewhat frequent trouble, and indeed it is difficult to see how it can be altogether avoided. It is very rare, however, that it need be a source of anxiety; but in one instance in my practice it was extremely serious, and the story of the case will give a lesson more emphatic, perhaps, than any mere directions which I can give.

A lady was placed under my care some three years ago for the purpose of having an ovarian tumor removed. She was, as usual, kept in bed for two or three days before the operation and on the morning of the operation nothing was given to her by her attendants, in order that sickness might be avoided. The operation was to be performed at nine o'clock, and she, being profoundly persuaded that upon an empty stomach she never could go through the ordeal, rose about six o'clock on the same morning, went to one of her boxes, which had been unfortunately left in her room, and in which she had secreted a quantity of biscuits and a bottle of port wine, and with these she made a hearty meal. In the middle of the operation sickness came on and she vomited a large quantity of purple pulp. She went on incessantly vomiting for eight days, and long before that time I had given up all hopes of her recovery. The straining caused by the retching tore the wound open, and upon the sixth or seventh day a large sloughy mass was extruded, which proved to be the rectus muscle of the left side, which had apparently been destroyed by the continuous vomiting. For ten days more she lay with a large, open wound, through which could be seen the movements of the intestines, and between her and another world, there was nothing but a layer of peritoneum. Fortunately, she did not open the wound any farther. She ultimately recovered, and is now in perfect health, save that there is a large intestinal protrusion at the seat of the wound. I do not think that in the whole course of my practice I have ever had a case which gave me so much anxiety, and it also gave me a powerful argument by which to impress upon others the absolute necessity of obedience.

The anæsthetic should, therefore, be always given upon an absolutely empty stomach. I do not even approve of the dose of brandy and water usually given, because ether is in itself one of

the most powerful stimulants, and we have only to watch the improvement upon a feeble pulse which takes place during its administration to be quite satisfied that no other stimulant is necessary. The quantity of ether given during an operation, especially during ovariotomy, is necessarily very large, but no one need ever be alarmed on account of the quantity of ether administered. It is absolutely necessary to keep all the patient's abdominal muscles, except the diaphragm, perfectly quiet; and long before there is the least danger the patient's deep snoring will indicate that the stage of profound sleep has been reached. When the patient snores the administration should for a short time be discontinued. The quiet regularity of breathing which always characterizes the unconscious state induced by breathing the vapor of ether is quite enough to indicate to any one of any experience in its use that the patient is in a condition of profound insensibility, and this will also serve as the best indication of safety. There is, therefore, very little need for the pulse being watched, or for the conjunctiva to be experimented upon to determine unconsciousness. Should sickness come on during the operation, the ether should be pushed a little more and this will stop it, because sickness really is an indication of returning consciousness. After the operation is over, should sickness occur, it is best checked by the administration of a little luke-warm water flavored with brandy, and without sugar. When the patient is sick she should always be turned on her side, as in this position the tongue falls forward and the vomit is got easily rid of from the mouth without risk of its entering the trachea. Forceps should never be used to drag the tongue forward, after the barbarous fashion introduced by the late Mr. Syme. It is quite sufficient to turn the patient on her side, or even to turn the head to one side, if any necessity arises for this during the operation. Artificial teeth should always be removed from the mouth before the ether is given.

There is only one risk about the administration of ether, and that is the occurrence of bronchitis in old people. I have not seen this often, but in several instances I have had reason for anxiety on this ground, and in order to avoid this risk I have devised an apparatus by which the vapor of ether is given at a temperature of 33° Centigrade, that is, very nearly its boiling-point.

The woodcut on opposite page will explain the apparatus which I have devised for the purpose. Anhydrous methylated ether (.720) is placed in the reservoir, A, which will hold about ten ounces, and which is furnished with a spring pump, which drives over about a dram of ether at each stroke into the glass boiler, B.

This boiler is suspended in a hot-water tank, C, beneath which is a spirit-lamp. From the boiler an exit tube, four or five feet long, passes to a Junker's mouth-piece.

When the apparatus is to be used, the tank is filled with water, the spirit-lamp is lighted, and about three drams of ether is pumped into the boiler. Care must be taken that there is no leakage from the boiler, otherwise there will be an ignition. It will soon be found that the boiling ether gives over a large volume of vapor at a constant temperature of a few degrees below the boiling-point of the sample, which of course will vary very much, but will generally be found to be pretty nearly the temperature of expired air, 31°—33° Cels. When given at this tem-

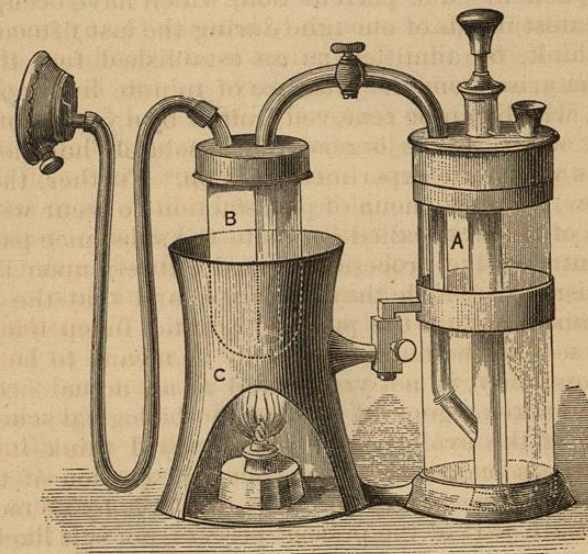


FIG. 32.—Apparatus for the Administration of Ether-vapor at Blood-heat in Cases of Old People.

perature, and free from air, the vapor is quite pleasant, and its taste or rather its comparative freedom from taste, reminded me when I tried it on myself, greatly of the flavor of nitrous oxide. It is, of course, certain that its administration will involve no risk of bronchitis.

I think this method will be found to be a substantial advance in the method of ether administration for old people; and if care be taken to keep the boiler vapor-tight, and the mouth-piece three or four feet away from the spirit flame, I think the apparatus will be found quite safe. Dr. Lauder Brunton made to me the ingenious suggestion of having a steam-jacket round the boiler, but that I have not found to be practicable without greatly

complicating the apparatus. The use of a hot iron bolt to boil the water, instead of the lamp, would probably be safer, but it would not be so handy, and would involve great wear and tear.

There comes in now the question of the adoption of what are called antiseptic precautions, and of these it is quite impossible to speak without a discussion, however brief, of the theory upon which they are based. This is necessary for many reasons, but chiefly because the followers of the antiseptic doctrine assert, with a vehemence worthy of the scholastics of the fifteenth century, that unless there be faith in the doctrine there can be no success in the practice.

To those who have followed closely the elaborate researches upon the phenomena of putrefaction, which have occupied some of the greatest minds of our time during the last fifteen years, it must, I think, be admitted as an established fact that these phenomena arise from the presence of minute living organisms in the air, which can be removed from it by a variety of physical means, by which the air is rendered absolutely harmless to the substances which are experimented upon. Further, that so far as we know, no phenomena of putrefaction do occur without the admission of these so-called germs to the substance putrefying, that the putrefactive processes depend entirely upon them and the organisms to which they give rise, and that the origin of such organisms within the putrefying fluid, independently of a sowing of seed in the fluid, though by no means to be regarded as an impossibility, is not yet proved as an actual occurrence. To any who takes a general view of the biological scheme from the aspect of the evolutionary philosophy I think it must be clear that the so-called "spontaneous" generation of the early forms of life is a necessary corollary, but it is by no means certain that it is a part of the present process, nor is it likely, in my opinion, that we now have upon the earth such conditions as would render biogenesis possible.

For my present purpose, therefore, it is enough for me to assume, as I do most fully, that the germ theory has been completely substantiated, and that no known process of putrefaction does occur save by the admission of resting spores or swarm spores of some of the many minute living organisms which are invariably associated with putrefactive changes. But concerning this there is another constant position associated with these phenomena. The materials upon which the experiments have been made, of infinite variety of kind and constitution, have all been dead, and no one has yet pretended that, by the admission of germs to living matter, he has produced the phenomena of the putrefactive changes which constantly result in matter which is

dead. To quote the apt illustration given by Dr. Wm. Roberts in his masterly exposition of this most difficult subject, the ordinary hypodermic morphia syringe will inoculate inevitably a sterilized solution of dead organic matter, but amongst the hundreds and thousands of hypodermic injections which are made daily, no one has yet declared a single instance of putrefactive changes resulting from it in the healthy, or even in the diseased human body.

It will, therefore, be seen that the application of the facts of the germ theory of putrefaction to the phenomena of diseases of living tissue is met at once by an overwhelming difficulty, to the removal of which none of the adapters, so far as I have seen, have as yet applied themselves. Granting that the same germs which would inevitably produce putrefaction in a dead infusion of beef are constantly admitted to wounds, there is not the slightest particle of evidence that they do produce any change whatever upon living tissue, still less is there any evidence that the changes which occur in the numerous varieties of what we call blood-poisonings, even when they are of an undoubtedly local origin, have the slightest analogy to those seen in a putrefying dead infusion. The mere presence of bacteria in the fluids of wounds, or in fluids enclosed in cavities, whilst offering many difficulties to the adapters of the germ theory, prove nothing for their position until they have shown that these organisms ever do occur in fluids or tissues which are truly living.

The difficulty, therefore, is this, that what we call vital action, for want of a name based upon a better understanding of what it is, places living tissue in an altogether different category from tissue in which the phenomena of life are no longer present.

Now, this is consonant with every-day experience. If a decaying hyacinth bulb or a rotting apple be examined, the presence of the minute forms of life is found to be absolutely confined to those parts where the changes have been effected, whilst those parts to which the rot has not extended are found absolutely free from them, and the difficulty of the adaptation of the germ theory is simply this, that its advocates have assumed that the invasion of the germs is the cause of the decadence of the vital phenomena and the ultimate death, while there is the alternative—still undiscussed and certainly undismitted—that the decadence of the vital powers, due to some cause possibly yet unknown, is that which gives the germs their potential ascendancy, and enables them to do what, during full vital action, they were wholly unable to effect.

If the views of the germ theorists were correct, we ought to expect that no operation could be done successfully without rigid

antiseptic precautions. The slightest cut of the skin ought to be followed by septic poisoning. There ought to be no difference in the mortality of operations in small and in large hospitals, in town and in country. In fact, if germs could have had the unbounded influence which is claimed for them by many antisepticians, surgery must long ago have been an extinct art, if, indeed, it ever could have struggled into existence.

The uniform experience of operating surgeons has taught them that the success of their work will depend upon three factors, the condition of the patient, the condition of his surroundings, and the nature and extent of the operation performed.

Of these three, undoubtedly the most uncertain factor is the first. What condition of the system it is which is favorable to operations is almost unknown. I must base my conclusions chiefly upon my own work, and in my special operation of ovariectomy I am perfectly certain that apparent perfect health is by no means a certain indication of a power of resistance to those conditions, whatever they be, which result in so-called septic poisoning.

The second of the factors, the condition of the surroundings of the patient, contains elements of far greater certainty. It has approached the position of a statistical law that the death-rate is in constant harmony with the density of the population, and that when the density exceeds a certain minimum of safety there are introduced specific septic diseases, as typhus fever, which are wholly unknown under other conditions, and which, even after the danger density has been reached, attack certain individuals only, and not all, for reasons which can be expressed only by saying, as I have already said, that the living tissues of those affected could not, and did not, resist the septic influence.

Every advance we make in sanitation shows that this factor, the condition of the surroundings of the patient, is of extreme importance.

The third factor which influences surgical success is the extent and importance of the operation performed. Everybody knows that while amputation of a finger is probably fatal in not more than one in ten thousand cases, nearly one-half of all amputations of the thigh die. Now, if the adaptation of the germ theory to surgical practice were as promising and as legitimate as some of its supporters allege, we should have had the remarkable result, previous to its application, that amputations of the finger and of the thigh ought to have approached one another in mortality to an infinitely larger extent than they have done.

If the contact of a bacterium germ upon a wound could be the source of blood-poisoning, then the size of the wound and

the nature of the operation could make but small difference in the result, and a wound into the theca of a finger tendon, and one of similar size into the peritoneum of another patient in the same ward, ought to have very similar risks. But, as a matter of fact, they do not, and we are forced to the conclusion that, even if bacterium germs lighting on wounds are the cause of much surgical mortality, that the power of vital resistance by the tissues, or the condition of the patient, and the extent and nature of the operation, are of infinitely greater importance as factors in the general result. This logical difficulty has evidently occurred to many of those who carry out Mr. Lister's adaptation of the germ theory to surgical practice. I have seen a rigid antisepticist occupy an hour and twenty minutes in making incisions a fraction of an inch in measurement, and barely skin deep, for the purpose of laying bare the tympanic membranes of an infant in whom they had been congenitally covered, the protraction of the operation being due solely to the antiseptic precautions. Such a proceeding produced in my mind a variety of emotions, chief of which were admiration for the enthusiastic consistency of the operator and sympathy for his evidently wearied audience. The just criticism of such a proceeding is the question, "Has any one ever seen such a trivial operation result fatally from septic poisoning, unless in some such hospital, as is described by John Howard in 1780, as the Hôtel Dieu, with three patients in each bed?" I certainly never have, and I have performed some thousands of them; and if it were necessary to take one hundred minutes to do what I could do in three I, for one, should seek my livelihood in some employment other than that of an operating surgeon.

The logical conclusion to be drawn from the facts is, therefore, that in minor operations germs have never, or at least hardly ever, any influence at all, and that in major operations the condition of the patient is of immense importance in enabling him to resist the influences, whatever they may be, which result in what we call the septic condition.

In any examination of the question there will of course be the primary difficulty, that it is by no means agreed as to what constitutes a major operation, and that between different operations which are admitted as major, there are known to be very different rates of mortality. Thus, amputation of the leg is proved, in my book on "Hospital Mortality," to be more than twice as fatal when performed for accident as when performed for disease. It must be perfectly evident, therefore, that any examination of this question must be conducted upon the usual rules of statistical investigation, the chief of which is that similar and not dis-

similar accidents should be grouped together. Any mere statement, therefore, of the general percentage of deaths from septic diseases on the general hospital or other population are absolutely worthless unless they be most carefully analyzed, and they are, of course, open to the still further objection that what constitutes a death from septic disease is by no means a perfectly accepted definition.

There is a popular belief that statistics can be made to prove anything, than which there is no popular belief more erroneous. Statistics alone seldom prove anything, certainly they never explain anything. Thus, the Registrar-General's tables tell us that there are certain death quantities which are perfectly constant, and they establish the fact that half of all our human mortality occurs before the fifth year of life. But this neither proves nor explains the cause of this mortality, nor does it even explain its factors, until a more careful analysis of individual cases is made. Therefore, nothing whatever can be proved for or against the adaptation of the germ theory to surgical practice by mere statistical statements. But in spite of this, statistics may be made to show exactly in what direction analysis of individual instances should be made, and, therefore, they alone are capable of forming the first step of accurate inquiry. First, let us ascertain, as fully as possible, what the facts are, and then analytical arrangements of them will certainly afford a more or less complete explanation of their method of production.

Thus, it must be evident to every one that a large group of one hundred ovariectomies must present features more similar to those of another set of a hundred than can probably be got in any other surgical comparison which is possible; and it is a probably correct assumption that if the same surgical skill and patience, the same attention to minute details, and the same state of the surroundings were common to the two groups, their resulting mortality would be identical or nearly so. But if there is one thing we value more than another, as being likely to contribute to success in surgical operations, it is personal experience; and we, therefore, may fairly expect that with each succeeding hundred ovariectomies the mortality will diminish, owing to the increasing skill of the operator. And this is the case notably in the practice of Dr. Keith, who, beginning with eleven per cent., went successively down to eight and six before he began to use antiseptics; and of my own experience I can only say that, while I had nineteen deaths in my first fifty operations, I had only three in my second fifty, and in all my subsequent practice these good results have been fully maintained; indeed, have been excelled.

A recovery after an ovariectomy is the sum of a number of details, all of which were efficient. A death, on the contrary, may be the failure of one only, and that may be or may not be under the control of the surgeon. Thus, of the three fatal cases in my second fifty, two were deaths due to details wholly beyond my control, and having no relation whatever to either the antiseptic system or any other of the operative details. The third death was due, as far as I could determine, to the irritative effects of thymol, used with full antiseptic details. Two of these deaths were antiseptic out of twenty-nine cases treated antiseptically; while of twenty-one cases treated without antiseptic precautions, I had only one death, and as the patient died within three hours after the operation, the want of antiseptic precautions could have had nothing to do with her death. From this group of cases, therefore, the argument would be wholly against the antiseptic system, and though my impression is that the conclusion would be a just one, yet the argument is absolutely fallacious, as all such are.

In the discussion of this question, which occurred some months ago, the only statistical argument of the slightest importance was given by Mr. Spencer Wells, who said that a very marked improvement had occurred in his results since he had used antiseptic precautions. But nearly concurrently with his adoption of germicides, he adopted the intra-peritoneal method of dealing with the pedicle, a method which has been superlatively successful in the hands of Dr. Keith, and to which, chiefly, I attribute my own rapidly increasing success. Thus, Mr. Wells' mortality improvement argues nothing in favor of antiseptics, but far more, in my opinion, for the short ligature.

The greater part of what I have just said upon this most interesting and important subject, is taken from a paper which I read before the Royal Medical and Chirurgical Society of London, in February, 1880, and which is published in vol. lxiii. of the "Transactions," and its general conclusions are quite in harmony with the opinion expressed by Mr. Spencer Wells, in his lectures delivered before the Royal College of Surgeons. I give his opinions as they are reported in the medical journals at length, for they are very important. He says: "If (say the supporters of Listerism) in one of the most serious operations ever performed upon the human body, where the largest serous cavity is opened and the freest access is afforded to any infective organisms which may be near the patient, Mr. Wells can obtain eighty recoveries out of one hundred operations; and as on two occasions he has had long series of successful cases—one of twenty and two of twenty-seven—without a death to break the run of success: