In deducing the effects of alcohol from this comparison, it is necessary to allow for the somewhat lessened exposure to weather, and also to accidental violence, involved in the alcoholic pursuits—exemptions which, so far as they go, would diminish the liability to tubal nephritis and to lardaceous change. This may help to explain the comparative rarity of these disorders with the alcoholic class. The increase in fibroid and chronic tubal changes must be referred to the liquor alone; but this increase is not great.

Taking all the facts together, I do not see how it can be doubted that the effects of alcohol in causing renal disease is less than used to be asserted: that to paraphrase Dr. Roberts, if he will allow me to do so, this agent does not rank higher, but lower, than other circumstances—to wit, external temperature and the absorption of lead—as a determining cause of Bright's disease. Although we can now and then trace the existence or aggravation of renal symptoms to intemperance, we do so with far less frequency than with disorders which have other sites. Pathological and clinical evidence combine to show that the evil influence tells more upon the nervous system, the liver, the blood-vessels, and probably in the production of tuberculosis, than upon the kidneys. The facts adduced are consistent with those formerly advanced, and with the quoted conclusions which have been the object of attack.

THE ADMINISTRATION OF ETHER, AND ITS SUPERIORITY TO CHLOROFORM AS AN ANÆSTHETIC AGENT.

By J. MORGAN, M.D., F.R.C.S.I.,
Surgeon to Mercer's Hospital, etc., Dublin.

As the question of the superiority of ether over chloroform is daily attracting attention, any contribution towards our more efficiently applying this anæsthetic, and proving by facts its advantages, must be interesting. In the last two issues of the JOURNAL I have seen some remarks on the subject, and some more particularly in the last number, which are apposite, as they bear on the inconveniences which are met with in the method which has been most generally used, of administering the ether to a patient by pouring it over a sponge or towel. Although, no doubt, etherisation can be successfully induced by this method, every one who tries it will, I am sure, agree with Dr. J. W. Haward in his observations on the inconveniences caused by the administration of ether in the rough and ready method referred to, and then by force applying it to the patient. Mr. Clover, also, in his observations intimates that a mixture of air, as with chloroform, is desirable. This may be doubted. Dr. Squibb and Dr. Lente of New York, in common with others, believe that the more freely ether is administered the better will be the result. In this opinion I concur, and have found by experience that it undoubtedly acts best when not diluted with air.

In the pages of the *Medical Press and Circular*, I have written a series of papers, explaining the advantage of the method of using ether which I had arranged, and with which I had operated for a considerable time before, and which, I must add, I find to be perfection as an anæsthetic apparatus. It consists of an inhaler, furnished with a flexible diaphragm, which corresponds with the inspirations and expirations of the patient when being applied. The ether vapour is diffused over the interior by a framework covered with thin flannel, and is conducted to the mouth by a flexible tube and well-fitting mouthpiece. The ether is poured in, and its vapour is breathed over and over by the patient, as the flexible diaphragm allows sufficient mobility, and the effect is made known by the production of stupor and insensibility in a few minutes, varying from three to five or seven. By confining the ethereal vapour in this way, the odour or stupefying effect is not extended to the bystanders as it is in the rougher method of pouring the ether on a sponge or towel, and with even the assistance of a cone of pasteboard. The surgeon has usually to change his dress to get rid of the odour. There is also no necessity for violently overwhelming the patient; on the contrary, the patient may with a certain amount of confidence apply the mouthpiece himself at the commencement—a proceeding which renders the administration much more agreeable.

There is no chafing or irritation of the skin of the face or neck, as happens from the application of ether by the sponge or towel. There is no waste of ether as with the inhaler: two, three, or four ounces will suffice, where ten or fifteen ounces would be used by the sponge. The etherisation can, with a little caution, be used by the inhaler with candle or artificial light, where the more wasteful and diffused application by the sponge would be inadmissible, from the danger of igni-

tion. By the more direct application of the ether-vapour by the inhaler, there is less excitement than when the sponge or any other method which allows air to be more or less freely used with the vapour. The excitement and tediousness as well of etherisation being, I find, proportioned to the amount of air which is admitted with the ether-vapour, or, in other words, to the perfection of its administration: in fact, there is hardly any excitement stage; in the majority of cases, none at all. Sickness of stomach follows much the same rule; but if the stomach be empty at the time of etherisation, there is no sickness usually. In excitable females or nervous persons it does occasionally happen—certainly not more so, if as much, as in chloroformisation under like circumstances.

A remarkable advantage of ether over chloroform is that of its allowing a speedy reaction. After etherisation, a patient is in a few minutes perfectly restored; and, if the operation of it be not too severe, he will even walk from the operation-table.

We have in ether an agent which is proved by statistics to be eight times more secure than chloroform, and to be the safest of all anæsthetics in use. It has held its own for a quarter of a century in America, and confidence in its powers is unbounded. Certain inconveniences are to be remedied, such as—its diffusion among the bystanders; the local irritation caused by contact with the fluid; the waste of fluid; the excitement of delay; the danger of its use with artificial light. These are avoided by the use of the inhaler. There is a precaution worth bearing in mind, that the patient's head should be always well raised, specially in elderly and apoplectic individuals. Congestion of the face is to be watched in such cases, and discretion used in pushing the influence of ether or of any other anæsthetic.

I have used ether in hundreds of cases, and I have seen no ill effects. The day I etherised six patients in periods varying from three to five minutes, the anæsthesia was in all cases perfect and most satisfactory; the patients sometimes becoming insensible while applying the mouthpiece themselves, while the inhaler gets rid of all the inconveniences that have been found in the rougher and more wasteful method of applying by the sponge or towel.

The appended woodcut illustrates the mode of administration and the position which suits best.

ON ANÆSTHETICS.

By THOMAS JONES, M.D.,
Assistant-Physician to Victoria Hospital for Sick Children; formerly (for eleven years) Resident Medical Officer and Administrator of Anæsthetics to St. George's Hospital.

FULLY impressed with the truth of the main points ably insisted upon in the leading articles in this JOURNAL of the 2nd and 16th instant, on the question of ether and chloroform as anæsthetics, I am induced to state, as briefly as I can, the views which I have formed after having had somewhat extensive practical experience on the subject. And as another reason for troubling the readers of this JOURNAL with these remarks, I might mention my anxiety to keep up the interest of the profession in the superior claims of ether as an anæsthetic (so thoroughly do I believe in this fact myself) until all surgeons, and all engaged in the administration of anæsthetics, are induced to try it, and persevere in its use until they become proficient in its administration. I am quite sure that, if thus fairly tried, ether will soon replace chloroform in this country as the anæsthetic in general use.

I. In both public and private practice, I have administered chloroform to more than six thousand cases. I have used in its administration Snow's and Ellis's inhalers, Clover's bags, Skinner's apparatus,

and simply a towel or a piece of lint. Whichever instrument be used, and however much care be taken, I have come to the conclusion that, out of a large number of cases, alarming symptoms are sure to occur in a few; and, if these symptoms be not observed in time, and attended to, fatal results will follow; and in a still smaller number of cases death will inevitably take place.

From the study of carefully recorded deaths from chloroform, I think they may be classified into avoidable and unavoidable. The unavoidable deaths are those which happily occur very rarely, in which, owing to some inexplicable cause (generally attributed to idiosyncrasy), the agent kills almost instantaneously, from its paralyzing influence on the heart through the cardiac ganglia. I do not think that any amount of care in measuring the chloroform with the air, and in its administration, can avert such deaths. If sufficient amount of chloroform be given to produce narcotism, there is sufficient in the system to cause death in these cases. A case of this kind, I am sorry to say, but happily the only one, occurred in my practice some years ago; and, as it well illustrates this class of cases, I will briefly give a few particulars of it from notes taken at the time.

CASE I.—Henry C., aged 30, a healthy-looking and muscular man, with ruddy complexion, from the country, was admitted into St. George's Hospital, on September 16th, 1863, suffering from necrosis of the lower end of the tibia of fifteen months' standing. On the 24th, he was brought into the operating-theatre, and chloroform was commenced (Snow's inhaler being used) at 1.17 P.M. His pulse was full, regular, and strong. He showed no apprehension of the chloroform. He breathed it unusually well; the respirations were regular and his pulse continued remarkably good; the latter was continually watched, not only by myself, but also by another gentleman. The chloroform was administered in a very diluted state for the first four minutes, the expiratory valve being kept open. Very slight muscular rigidity, which now appeared, rapidly passed off; the eyelids were still sensible to touch, and the pulse and breathing continued good. At 1.22 (five minutes from the commencement of the inhalation), the operation of trephining the tibia was commenced. He did not flinch when the first cut was made. The chloroform was continued, off and on, until 1.23 (six minutes from the commencement), when, without any change whatever in his general appearance, the pulse, which was, up to this time, full, regular, and about 80 in the minute, instantaneously stopped, as if he had been struck by lightning. The breathing, however, continued unchanged for several seconds; then it became slower and more superficial, and soon ceased. Before it stopped, however, cold affusion was applied to the face and chest, and ammonia to his nostrils. Immediately afterwards, galvanism was resorted to—one pole applied over the cardiac region, and the other on the neck—and, in a short time, needles were passed into the substance of the heart, and electricity passed through them. The tongue was also pulled forwards, and Silvester's method for artificial respiration tried and persevered in for more than an hour, but not the least evidence of returning animation was observed from the moment when his pulse ceased to beat; in fact, I was convinced that he was then dead, and that no effort to restore him would prove of any avail.

On *post mortem* examination, the right ventricle was found uncontracted; the left partially contracted. The blood was universally fluid. The valves were healthy. When the various parts of the heart were examined under the microscope, the fibres of the left ventricle were seen to be perfectly distinct. Both sets of markings were clearly seen, and there was not a trace of fatty change. In the right ventricle, the fibres were still distinct, but there was a slight amount of diffused granular fat; it was pretty evenly spread. None was collected in the substance of the fibres, and probably it was not more than is perfectly consistent with health. The appearance of the cardiac muscle to the eye was quite natural. The lungs were natural, except a small extravasation of blood just under the pleura over the lower lobe of the left. The lower part of the right was congested, but did not sink.

Here, then, is a case of a young man with a perfectly healthy heart (or one so slightly diseased that one could not positively affirm that any degeneration existed at all) dying instantaneously without showing any warning of danger whatever, and after very short and tranquil inhalation of chloroform. But this case may be objected to as an illustration of one of unavoidable death, on the ground that the quantity of chloroform and air was not accurately ascertained by measurement previously to inhalation. Judging from the fact that the valve for the admission of air in the inhaler was kept open for four minutes, and after then only slowly and partially closed, and from the time it took to narcotise him, together with the manner in which he came under the influence of the anæsthetic, I feel convinced that he did not get more than the prescribed amount (5 per cent.) determined

by Snow as being within the limit of safety. When chloroform is given from a bag in the proportion of 3 to 4 per cent. by measurement (a plan first adopted by Snow, who discontinued its use as being too troublesome), I have found it to affect people in many different ways. From the effects which I have observed under these circumstances, I have been long of the opinion that chloroform, like some other medicines, produces effects on certain persons quite out of proportion to the dose administered. I think that, if Snow had experimented on a sufficient number of animals and creatures of different species, he would have found one in which chloroform-vapour would prove fatal with less than the quantity determined by him as being within the limit of safety.

Avoidable deaths are those which are caused by suspension of the function of respiration, and those which are caused by the gradually paralyzing influence of the vapour on the heart. In both these modes of death, timely warning, I think, is given in all cases by dangerous symptoms supervening; and, if these only observed and attended to *immediately*, fatal results may be averted. Fortunately, the most common evil effects of chloroform are those which arise from interference with the breathing, owing to partial or complete suspension of function of the ganglia concerned in respiration, causing sometimes immediate cessation of the movements of the thorax, falling back of the tongue, laryngeal obstruction, and possibly other conditions. The symptoms that arise in such cases are so obvious—such as stertor, ineffectual efforts to breathe, or complete cessation of respiration, causing lividity—that, with common care, it is difficult to imagine how they can be overlooked until too late to apply the remedies, which, if used early, are attended with certain success: I mean suspension of the inhalation, forcibly pulling forward the tongue, artificial respiration, cold to the face and chest, galvanism, etc. Of course, in these cases, it would be of no use to wait till the pulse becomes affected before interfering.

Avoidable deaths from chloroform also take place from its acting on the heart primarily. Unlike the cases alluded to, in which the chloroform suddenly suspends the function of the cardiac ganglia, these are cases in which the dangerous effect of the agent is *gradual*, and are, therefore, cases in which timely warning is given in the condition of the pulse. I do not mean the various alterations in the pulse during the first stage or before the influence of the mind is removed, but subsequently to this. If I find, after consciousness is abolished, that the pulse becomes irregular and intermittent, or decidedly fails in power, and that these conditions increase instead of improving as chloroform is carefully proceeded with, I always discontinue its use. I have seen such cases in which I felt sure that, if it were persisted in, dangerous results would follow. The same rule applies to cases with heart-disease. If an irregular, intermittent, or otherwise abnormal pulse, become worse as narcotism is induced, I also always stop the anæsthetic.

I never, however, object to administer chloroform on account of heart-disease, but carefully watch the influence which the agent exerts on that organ. I have given chloroform to patients suffering from the worst forms of heart-disease, and kept them under it for an hour, with the best results; in fact, it is generally admitted that cases fitted for operation are also suited for chloroform. The following is a very striking case in illustration of this class of cases, which happened in my own practice. Although the heart was extensively diseased, I have seen similar effects in cases where the heart was perfectly healthy.

CASE II.—Richard T., aged 64, very corpulent, and with a congested state of vessels generally, admitted for stricture of the urethra and stone in the bladder, was placed on the operating table on September 24th, 1863 (the next case placed on the table following the one that died just related). His pulse was weak and irregular, and the heart was found on auscultation to be very weak. The chloroform was administered in the usual manner, but, as he became narcotised, the pulse became irregular in its irregularity and much weaker, and afterwards his breathing began to be embarrassed. I allowed him to recover a little, and, as soon as these symptoms passed off, I resumed the administration slowly; but as soon as he began to become unconscious, the same phenomena were observed. Thereupon I left off the anæsthetic and told the surgeon to proceed with his operation; which was forcibly dilating the stricture by Holt's dilator. The attempt to rupture the stricture failed, in consequence of the point of the instrument becoming entangled in a false passage. The patient was sent to bed. Another unsuccessful attempt was made in four days, this time without chloroform, of course. Eight days after the partial administration of chloroform, the patient died very suddenly. He had just been to the water-closet, and on walking back to his bed lay down upon it, and made some exclamation, which attracted the attention of the nurse. She summoned the house-surgeon, who was in the ward at the time; but, on examining, he found him already dead.

On *post mortem* examination, the pericardium was found to contain

about an ounce of fluid. The heart weighed fourteen ounces and a half. There was no excess of superficial fat, but the muscular tissue had a yellow colour and a flabby consistence. Under the microscope, the fibres were very fatty; a string of fat-globules or of fat-crystals often occupied the centre of a fibre. The left ventricle was quite uncontracted. The valves were healthy. The blood was everywhere fluid. The lungs were healthy.

Mr. Holmes exhibited the hearts of the above two cases before the Pathological Society. In the abstract of the cases published in the *Transactions*, I find that in the case of Henry C., he says that the "breathing and pulse ceased simultaneously." This is certainly an error: the patient continued to breathe for some time after the pulse had stopped.

In relating the case of Richard J., Mr. Holmes says that "the patient took chloroform well, and was kept under its influence for the necessary time—about seven minutes." This also is an error. The facts are as stated above. No doubt seven minutes were spent in the attempt to administer the chloroform, but, owing to the failure of the pulse, it had to be left off, and a second attempt was made with the same result. But the patient never was completely anaesthetised.

In this case, the heart was certainly very much diseased; but, as I have said before, I have seen cases with healthy hearts in which the pulse presented precisely the same characters. I attach comparatively but little importance to the state of the pulse before inhalation. What I am careful in watching is the effect produced by the chloroform, and especially at that stage of anaesthesia where unconsciousness comes on. A pulse that is weak before chloroform is commenced may improve under its effects, especially when the influence of the mind is removed. But I am quite sure that in some cases the converse is equally true; namely, that a pulse which is good at the commencement of inhalation, becomes weak, irregular, or intermittent, as narcotism is produced. If, instead of improving, the pulse become worse as consciousness is abolished, I think it shows that the chloroform acts dangerously on the cardiac ganglia. In all such cases, chloroform should be discontinued. I fear many deaths which might have been avoided occur from this cause, through not attending to the timely warning which the pulse gives. It is impossible to estimate the amount of mischief effected by the directions given on this point by Mr. Lister, in his article on Anaesthetics in Holmes's *System of Surgery*, when one considers the deservedly high reputation of the author, and the importance of the work in which the article appeared. The burden of Mr. Lister's remarks seems to be on the importance of devoting the whole attention to the breathing to the exclusion of every other duty. Now, I admit, as all who are at all familiar with the subject must admit, that obstruction to the breathing is the most common of the dangerous effects of chloroform. But any embarrassment to the respiration, and the evil consequence that may result therefrom, are so obvious to the most casual observer, as Dr. Snow long ago pointed out, that I cannot imagine why Mr. Lister should labour so much to prove a point which every one already admits.

With reference to the pulse, Mr. Lister states (*System of Surgery*, p. 495, second edition) that "the safety of the patient will be most promoted by disregarding it altogether." Now, it is very remarkable that during the last ten years, from the time when the article containing this paragraph was written, there seem to have been many more deaths from chloroform than in the previous decade. How much of this might be due to the doctrines inculcated by Mr. Lister, is of course impossible to determine. It is, however, a significant fact, that two deaths (both of which were published in this JOURNAL) occurred from chloroform during the last year at the Edinburgh Infirmary, where, it is said, no deaths had happened before. And we find that, since the article referred to was written, Mr. Lister himself has met with two or three cases which almost proved fatal. These cases seem to have made some impression upon his mind, and to have somewhat shaken the views he enunciated in the original article; for we find that he admits, in the last edition, that "with these examples before us, of deviations from the usual order in which the various functions are affected by chloroform, no one can say it is impossible that here and there an individual may be found so constituted that, without any undue proportion of the narcotic vapour to the air inhaled, the cardiac ganglia may fail before the respiration is interfered with." This is just what I have endeavoured to insist upon; but also I maintain that this failure of the cardiac ganglia may show itself gradually as well as suddenly, and that the pulse, therefore, becomes a most important signal of approaching danger. It is to be hoped that, in the next issue of his article, Mr. Lister will be induced, after increased experience of chloroform, also to admit the importance of carefully watching the pulse as well as the respiration. I confess experience has taught me that chloroform, even when most care-

fully administered, is more dangerous than is generally supposed. If I were, unfortunately, compelled to take an anaesthetic, nothing would induce me to take chloroform.

[To be continued.]

ON DILATATION OF THE CERVIX UTERI BY BOUGIE.

By J. H. AVELING, M.D.,
Physician to the Chelsea Hospital for Women.

I HAVE read with great interest the papers by Dr. Matthews Duncan which have appeared in the last two numbers of the JOURNAL; for, having partly gone over the same ground some years since,* I was anxious to see how far his observations and conclusions agreed with mine. With regard to the relative values of tangle- and sponge-tents, we are, in the main, agreed; but against the practical conclusion at which he arrives I must raise my feeble protest. The authority of Dr. Duncan is deservedly so very great, that I tremble to think of the mischief which must be caused should medical men generally adopt his method of forcible dilatation.

The bougie, he thinks, is the best instrument for dilating the cervix uteri in gynaecological practice. I beg respectfully to differ from him for the following reasons.

1. The uterus is not a body rigidly fixed, but one which, under very slight mechanical influences, tumbles about hither and thither; and this extreme mobility renders it utterly impossible for an operator to be certain that he is applying the wedge-force of the bougie in a proper direction.

2. The dilating action of a metal wedge grazing along the mucous lining of the canal is objectionable and unnecessary, as it may be avoided by using tents and other dilating instruments.

3. The uterus may be in a spongy and flaccid condition, capable of being bent upon itself by the pressure of the point of a bougie acting on either side. In such a case, the extreme liability of the bougie penetrating the side upon which it impinges is obvious.

4. The cavity of the uterus varies in length, so as to make it difficult to determine whether the point of the bougie have arrived at the internal cervix or the fundus uteri. If it, by mistake, be "urged through" the latter, serious consequences might follow.

5. In narrow strictures a thin bougie must be used, and the more resisting the stricture and the thinner the bougie, the greater is the risk of the uterine walls being penetrated.

Dr. Duncan uses the sound in such a heroic manner, that it can only be his very great skill which prevents him from doing much mischief. He says: "I have observed that, in using great force to pass a common uterine probe, the instrument is bent." I have always learnt and taught that no force should be used in passing the uterine sound, and I cannot help thinking that the pressure of "four pounds" (the amount required to bend a sound) is far greater than should ever be employed. The bougie he also urges forward with equal confidence and force. In passing it through the internal os, he says, "the difficulty may be extreme." I am satisfied that I have frequently applied a forward pushing force at the point of a probe equal to 4 lbs., or even more, and with the wedge or conical pointed bougie a force probably exceeding 200 lbs., on the square inch of surface of part undergoing distension. "When a great force is applied, the sudden or jerk-like advance of the bougie through the internal os, and the smearing of the instrument with blood, show that the part has not been gradually expanded, but lacerated—an accident which, I think, it is desirable to avoid."

Dr. Duncan tests the value of the different modes of dilating the cervix uteri by three considerations: (1) The amount of force exercised; (2) the amount of expansion produced; and (3) the amount of time required for producing the expansion. In considering the force exercised, its direction, as well as its amount, must be taken into account. It is true, as Dr. Duncan says, that the force of the bougie is "limited only by the strength of the instrument," but its expanding force, even when exerted in a correct course, is not so direct as that of tents and other dilating instruments. Then the amount of expansion produced by bougies is much less than can be effected by tents and dilators; and the amount of time required to produce the dilatation by Dr. Duncan's method is scarcely less than that required by other plans; for, by a number of "sittings," or by using a "large instrument very long and perseveringly, he only produces a dilatation of about a quarter of

* "On the Relative Value of the Various Substances which have been used in Dilating the Neck of the Womb; with a Plan for Deodorising Sponge-Tents."—*Obstetrical Transactions*, London, December 4th, 1867.