pendent focus of caseation. From a vessel so infected an abundant supply of bacilli may be furnished direct to the circulation, no glands intervening to offer a barrier. Through the coats of the pulmonary vessels they have also, as already stated, been observed to pass directly into the circulation; but they probably do so to a very small extent as long as the walls of the vessels are unbroken by inflammation. But it is quite otherwise when a vein, whether in the lung or elsewhere, lies in close apposition to a caseating focus of tubercular disease. In such a case the wall of the vessel may be eroded, and the contents of the caseous abscess may be poured directly into the venous, and from this into the general circulation. This has actually been demonstrated by Weigert, in an interesting series of cases, to have taken place, and to have been followed by rapid general tuberculosis. A knowledge of this fact ought to have an important bearing on the operative treatment of tubercular caseating foci. It is very easy to imagine how easily surgical interference may give rise to this mode of general infection when we remember the constant presence of dilated thin-walled veins around the walls of caseous abscesses, and the ease with which they may be torn. Again, the rough handling of a tubercular joint, even without any cutting operation, may complete the rupture of a vein already partly eroded by its proximity to a caseating process, and may regularly inundate the venous blood-stream with the products of the latter. We have all, I suppose, seen rapid general tuberculosis follow upon some disturbance of a scrofulous joint by injury or surgical interference, and such a sudden inroad of the products of the local disease into the venous circulation would best explain such an occurrence. The beneficial effect of absolute rest too in limiting tubercular processes may be no doubt explained in part by absence of this dispersion of bacilli through eroded vessels, rest giving the latter time to erect barriers against the encroachment of the organisms by means of ordinary plastic phlebitis.

But it is to the lymphatics that we must look as the ordinary carriers of the microbes from their first landing-place; and it is fortunate that this is so, for in a large proportion of cases the lymphatic glands appear to have the power of arresting their further progress until they either die or in some way their dangerous powers are curtailed. There is every reason to suppose that in many cases the bacilli are arrested completely in the glands and never get any further. But it is equally probable that in a large proportion of cases they or their spores pass quickly through them in greater or less number, and gain an entrance into the blood. It is also more than likely that they, or their spores, often pass on from the glands into the circulation after a long and comparatively quiet sojourn in the former, perhaps amounting to years, and only become dangerous on being disturbed and widely distributed through the circulation. The importance of this power of the lymph glands to arrest the organisms and prevent their contamination of the whole blood-stream is, of course, enormous; for, once they have entered the blood, they find a most fruitful soil for their propagation, and not only this, but they are carried to every part of the system, and can select among the organs and tissues one or more spots specially suited, either physiologically or pathologically, for their propagation. That certain organs and tissues are peculiarly liable to be attacked by this disease has been well known for a long time past; but the conditions determining the point of attack have not been so well understood. Undoubtedly the view that injury to a part predisposed it to tuberculosis is quite correct, whether this injury consist of disintegration of tissue, with extravasation of blood, or disturbance of its functions, or only amount to impairment of those forces which go to make up what is briefly termed its "vitality."

But besides the influence of external agencies in predisposing to tubercular growth, there appears to be in certain spots in the body some inherent suitability of soil for the cultivation of the bacillus tuberculosis depending upon their physiological conditions. In other words, with the organism in the blood of an individual not unfavourably influenced as regards one part or another by external agencies, the bacillus appears to prefer to make its home, as a rule, in certain tissues, and to leave others unaffected.

The pia mater of the child furnishes, perhaps, the best example of what I mean, the medulla of the bodies of the vertebrae the next best. Both of these tissues in the child are as far removed from the action of injury, or, indeed, of any external influence, as possible, and yet the frequency with which they become the seat of tubercular disease is proverbial.

It would be interesting to examine the various aspects of this

particular question at greater length, but I am obliged to content myself to-day with this very brief allusion to it. Enough has been said, I hope, to indicate the line of reasoning I have been led to adopt as regards the nature of tubercular or scrofulous disease, and which I shall venture to pursue in its direct application to joint-disease in my next lecture.

ON HYDROPHOBIA AND ITS "TREATMENT:"

ESPECIALLY BY THE HOT-AIR BATH, COMMONLY TERMED
THE BOUISSON REMEDY.

By VICTOR HORSLEY, F.R.S., ETC.,

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Superintendent of the Brown Institution, Professor of Pathology in
University College, and Assistant-Surgeon to
University College Hospital.

HYDROPHOBIA being a disease of which the true pathology was scarcely known until a few years ago, indeed almost until M. Pasteur began his remarkable investigations, has always been regarded by the quack and the impostor as a fair field of enterprise. The authoritative statements employed by such therapeutical pirates to foist their methods and nostrums upon the much beguiled public commonly include the prominent expression of some popular belief. Amongst such beliefs must be placed the ubiquitous confidence reposed by the laity in the active secretion of sweat as an efficient means in getting rid of the *materies morbi* of acute specific diseases. It is a very widespread notion that if there is such a poison in the body, it can be "sweated out," just as it used to be generally believed (though now only by a particular sect) that the exercise of religious faith would, if energetically resorted to, get rid of a malady. The application of this sweating treatment in the shape of hot baths to the relief of rabies is of very ancient origin: and, under the belief referred to, it has been largely employed and advocated by irresponsible persons of every degree.

The practice of subjecting hydrophobic patients to hot air baths, however, received support from a French physician of professional repute, Dr. Bouisson. This gentleman seems to have suffered from symptoms, most of which resembled those of hydrophobia; but, from the account he gives of his trouble, his diagnosis that he was really suffering from that disease may well be called in question. Acting on the belief that he was so stricken, he entered a hot-air bath with the idea of terminating his existence, but he gradually became calmer, and soon the symptoms totally disappeared. Struck by these circumstances, he strenuously advocated the use of the hot-air bath in hydrophobia, and published cases in which he thought he had successfully combated the disease. He stated that if the patient were placed in the bath on the first day that the symptoms manifested themselves, a cure would be infallibly obtained. If only on the second day, that the cure was possibly uncertain, and that the treatment was hopeless if begun as late as the third day. Since Dr. Bouisson's papers were published, many patients, actually or supposititiously suffering from hydrophobia, or in whom the occurrence of the disease seemed possible, have been treated on this plan. All those patients who have been reported by respectable practitioners to have been suffering from genuine hydrophobia have died in spite of the Bouisson treatment.

Instances of its employment by such practitioners will be referred to presently. In spite of the failure of this procedure an attempt was made in the recent epidemic of 1885, and subsequently, to secure its trial by the profession. As might be supposed, its adoption is principally urged by those who are, for obvious reasons, opposed to the progress of medical science, the paid antivivisectionist agitators. These persons spread broadcast glowing misrepresentations of the system, and raise, as I have myself seen, many false hopes, and so cause much pain in the minds of the patient and his friends. For instance, one of their agents, a Rev. J. P. Wright, made the assertion that "Eighty cases had been already cured by this method, and only one doubtful case was chronicled." (*The Zoophilist*, January, 1888, p. 152.) It is deeply to be regretted that certain medical practitioners—to wit, Dr. Bell Taylor, of Nottingham, and Dr. Clarke, of Clapham, have countenanced the statements of these people, and advocated the trial of such treatment on man. What the "antivivisectionists" clearly desire is that the profession at large should make a series of experiments on man to see whether this Bouisson treatment is worth going on with or not. Most of us,

however, believe that we can more fitly fulfil our duty towards our neighbour by testing new remedies, not upon his body, but upon that of one of the lower animals, and it was with the object of averting such wholesale human "vivisection" that I instituted a series of experiments (that is, carried out the Bouisson treatment) upon lower animals (rabbits) suffering from rabies or hydrophobia. I was careful to carry out this treatment, not only therapeutically, but also prophylactically, but I regret to say that it favoured rather than hindered the course of the disease, death being invariably the result in each case.

The experimental method adopted was as follows. I inoculated by the usual subdural method eleven animals with what M. Pasteur calls the *virus fixe*—that is to say, the pure virus of the

tion commenced prophylactic treatment with the hot-air bath, as will be seen in Table III. This treatment antedated by two days the onset of the symptoms, and so expedited the fatal result. There is, therefore, no question in my mind but that this measure exerts a very unfavourable therapeutical influence upon patients suffering from hydrophobia by diminishing their resistance to the disease.

I will now proceed to describe in detail the method of experimentation, and afterwards will discuss the effect which it appears to have had upon the human patient in those cases where it has been tried in indubitable cases of hydrophobia.

A. Bouisson Treatment of Rabbits.—The rabbit is an animal which is sensitive to the action of heat, and in which, conse-

TABLE I.
(1887-1888)

VIRUS FIXE.

N ^o OF EXPERIMENT	DAY OF INOCULATION	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	STATE OF SYMPTOMS ON 15 th DAY OF BATH
1	5/12								13/12		+						VERY PARETIC
2	5/12								13/12		+						MUCH LESS PARETIC THAN NO. 1
3	7/12								15/12	16/12	17/12						VERY EARLY STAGE. SOME PARETIC. MUCH RIGIDITY
4	7/12								15/12		+						MARKEDLY PARETIC
8	14/12								15/12								MARKEDLY PARETIC
9	14/12								23/12	24/12							COMMENCING PARETIC DULL AND SHAKY
11	26/12								31/12	1/1	5/1						MARKED PARETIC OF HIND LEGS
12	26/12								31/12	1/1	5/1						COMMENCING PARETIC OF HIND LIMBS
13	27/12								31/12	1/1	5/1						MARKED PARETIC OF HIND LIMBS
14	29/12								31/12	1/1	5/1						MARKED PARETIC OF HIND LIMBS
15	29/12								31/12	1/1	5/1						SHAKY AND VICIOUS EARLIEST COMMENCEMENT

disease, which, in the series that I possessed, produced its first symptom almost invariably upon the eighth day after inoculation, sometimes, but more rarely, as will be seen by Table I, on the ninth day. I also inoculated three rabbits with virus taken from the medulla of rabid dogs of the street, such virus usually producing, as is well known, its first symptom about the sixteenth day, but in certain rarer instances in the manner of the *virus fixe*—namely, from about the seventh to the ninth day. (See Table II.) These fourteen animals I placed in a hot-air bath, according to Dr. Bouisson's suggestion, directly they showed the first distinct symptoms of the disease. Finally, I inoculated two other animals with the *virus fixe*, and on the third day after inocula-

tion, the effects of the hot air can not only be readily observed but also seen to continue long after the animal has been removed from the bath. Bearing in mind the statements made by the advocates of the system as to the therapeutic advantage to be gained by employing a high temperature in a more or less continuous manner, the rabbit appeared to be an animal which specially lent itself to such a mode of treatment, for by keeping it at a high temperature for some hours and then removing it one was able as a rule to prevent the onset of heat paralysis, and at the same time to obtain the continuance of the effect of the bath, for example, in the elevated body-temperature, etc. The bath consisted of a roomy wooden chamber with glass front and window at the back,

there being at both ends a suitable aperture fitted outside with a ledge for the resting of the head of the animal in the fresh air surrounding the box. As the aperture was, of course, considerably larger than the neck of the rabbit, the interspace was blocked by soft cotton wool. My object in keeping the animal's head in the outer air while the body and limbs were subjected to the hot air was to provide against the possibility of its general vitality being depressed by the carbonic acid, etc., which was doubtless present in the heated air of the chamber, the said air being obtained from an iron funnel and pipe (this opening into the bottom of the chamber), heated by a large Bunsen burner. A thermometer passing through the roof of the chamber recorded the temperature.

conscious and dies comatose, usually in from three to four days after the appearance of the first symptom. In Table I it will be seen with what remarkable, indeed mathematical, regularity the symptoms ran their course, with a rapidity slightly (as in Table I) or markedly (as in Table III) accelerated by the treatment.

2. The effect of the bath. (a) On the normal animal. When a healthy rabbit is placed in a hot-air bath which is, to begin with, at the temperature of the room, and rapidly, that is within thirty minutes, raised to $75^{\circ}\text{C.} = 147^{\circ}\text{F.}$, the respirations of the animal steadily but quickly increase in number, the rate of the heart also being accelerated, and the temperature of the body rising to a variable height, usually one-half or more degrees

TABLE II		1887																				
RUGE DES RUES		RABIES OF THE STREETS VIRUS																				
Nº OF EXP ^T	DATE	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
5	2/12																	19/12 20/12				
																		19/12 20/12				
6	13/12																					
10	13/12																					

The animal's condition could thus be easily inspected during the treatment, and the exposure of so small a portion of the body surface as the head to the cooler air doubtless exerted none but a beneficial effect. In two instances, Experiments 6 and 8, Tables I and II, it will be seen that the animals succumbed to heat paralysis. These were cases in which the disease was extremely marked, Experiment 6 being one of those remarkable instances in which the virus from the rabid dog of the streets occasionally evokes the first symptom and causes death within the minimal period, namely, that of the *virus fixe*. In these cases I was endeavouring to counteract the obviously more intense action of the malady by more prolonged bathing, but of course the injurious effect of the bath found little resistance in the central nervous system already overwhelmed by the disease, and so produced the fatal result.

This leads me now to speak first of the appearance of the symptoms calling for the treatment by the bath, secondly, of the action of the bath upon the normal and upon the rabid animal.

Centigrade. Occasionally, however, the rise in the number of respirations is preceded by a slight fall. The elevation of the temperature and acceleration of the heart and respiratory rhythm are maintained for some hours after the animal is removed from the bath, and gradually return to normal. b. On the rabid animal. The diseased rabbit when placed in the hot chamber exhibits the same series of phenomena, but, as might be expected in a more violent, that is exaggerated, manner, and this in proportion as the disease has exerted its destructive influence upon the nerve centres. Thus, in Experiment 2 (see Table IV), on the third day of treatment the rise of temperature of the body (rectal) was most remarkable. The temperature of the animal at the moment of its being placed in the bath was excessively low, it being at the end of the disease, namely, 24.5°C. In two hours and a half it had shot up to 39°C. , that is a gigantic rise of no less than $14.5^{\circ}\text{C.} = 26^{\circ}\text{F.}$

I have 69 charts of the curves, indicating the temperature of the chamber, the rise of the temperature of the body from the be-

TABLE III		1888										
VIRUS FIXE		"PURIFIED" VIRUS										
Nº OF EXP ^T	DATE	1	2	3	4	5	6	7	8	9	10	11
1	7/1											
2	7/1											

1. The initial symptoms. As is well-known, in the rabbit rabies is rarely (see Table I, Experiment 15) accompanied by the excitement so usually seen in other animals. Consequently the first symptoms observed in this animal is the paresis of the hind limbs, this occurring in all cases of rabies, furious or not. When this was distinctly present in the running or jumping movement of the animal it is expressed in Table I, under the term "marked paresis." It constituted an admirable means of detecting with certainty the onset of the characteristic symptoms of the malady. It will not be out of place to add that in the further progress of the disease the animal becomes more and more paralysed, and that from being apathetic at the commencement, it rapidly becomes un-

gunning to the end of the experiment and the variation in the respiratory rhythm. Analysis, however, of these numerous observations would take me beyond the scope of the present paper, and therefore I will content myself with simply exhibiting the chart, showing these phenomena in the instance just referred to (see Table IV).

I will now mention the general effect upon the rabic symptoms. These may almost be entirely summed up as being exacerbated. If the animal was taken out before the respiratory acceleration became excessive, it was occasionally noticed that, when previously apathetic, it might be more excitable, but, as a rule, that if the apathy were well marked to start with, the rabbit

was rendered more unconscious. In all cases it was much weakened by the heat, and this exhaustion and diminution in the vigour of the animal explains, I believe, the effect of the treatment in man in those cases where it reduced spasm.

The unsteady, shaky gait of the animal was always exaggerated, as also the paralysis.

Apart from the effects described, the usual influence of heat upon secretion was made obvious. Ordinarily there was a free flow of saliva and urine, and the intestinal peristalsis was notably increased. The hypothetical excretion of the poison might thus be supposed to have been in full activity, but the only effect upon the animal was sedative—that is, that of further exhaustion.

B. Prophylactic Treatment by the Hot-air Bath.—With a view of giving the treatment a trial before the disease had shown itself, I employed it prophylactically in two cases, as shown in Table III. In each instance the bath was employed as already described, but

patient was much comforted, became drowsy, and ultimately slept for half an hour. This result can but be considered as a very gratifying one, if regarded in its true light, namely, as a palliative not curative effect. As Mr. Southam states it did not "prevent a fatal issue."

On the other hand, instances are recorded in which the hot-air bath caused distress. Thus in a case published by Dr. Southey (*Medical Press and Circular*, 1887, p. 72) the bath was administered on the evening of the first day of the spasm; "he now had a vapour bath, during which he complained greatly of the heat, and he perspired freely." The next day "he absolutely refused the vapour bath." This patient, being a genuine case, of course died.

If an efficient palliative, the hot-air bath would be an adjunct to a sedative course of treatment; for in Mr. Southam's case it clearly exhausted the nerve centres, so that they no longer painfully reacted to reflex stimulation. The exact counterpart of this exhausting effect we have seen in the rabbit similarly treated

in each case, also, as shown in the table, the symptoms commenced on the sixth day, that is, two days prematurely, and one animal actually died at the end of the eighth day, instead of as usual at the end of the eleventh. The others died at the end of the ninth day. This indicated so unmistakably the general injurious effect of the treatment, that it was unnecessary to pursue the investigation.

I will now give two instances in which the bath has been described to have produced an effect upon the hydrophobic human patient. The only genuine case I am aware of in which distinct "benefit" can be attributed to the bath alone is that reported by Mr. Southam, of Manchester. In Case v (? Case vi) *BRITISH MEDICAL JOURNAL*, 1881, p. 815, it is expressly stated that after exposure to the hot air the spasms became much less marked, the

The consideration of this point suggests that this is the time to ask ourselves upon what definite plan we ought to treat this disease. Are we to go on attempting to cure it by experimenting upon human beings with substances like curare, or, as suggested by the antivivisectionists, with the hot-air bath; or shall we adopt what seems to me to be the only justifiable course, namely, assuage with all the narcotics at our disposal the sufferings of the patient, until we shall have found out the real antidote by experiment upon the lower animals, especially upon that one (the rabbit) in which the disease takes its painless form, paralysis, a form of which that remarkable veterinarian, Mr. Youatt, said with prophetic force thirty-seven years ago: "I very much regret that I never instituted a course of experiments on the production and treatment of rabies in this animal. It would have been attended

with little expense or danger, and some important discoveries might have been made" (*The Dog*, by William Youatt, 1851). Surely the public, at any rate, who place themselves in our hands, under these the worst of circumstances, would unhesitatingly choose to be helped to die painlessly. And in following such a course no practitioner would be violating, by neglect, the great ethical principle of his profession, that he should endeavour as far as lies in his power to preserve life as well as alleviate pain. For there is no single case of reputed cure of developed hydrophobia by drugs or other means that will bear close investigation. Neither has anyone succeeded in arresting by these methods the march of the symptoms when once distinct in an experimental animal, although an enormous series of drugs have been tried, notably by Mr. Dowdeswell (*Proceedings of the Royal Society*). Nor have workers in this direction been able, by the use of drugs, to imitate in the slightest the protection afforded by M. Pasteur's system of inoculation. Of the ability of that system to overtake and check the development of disease, if applied within a reasonable time after inoculation, and under the ordinary circumstances, there is no longer any doubt, but it is powerless against the developed symptoms. There remains for us, therefore, the determination of our action in those cases where M. Pasteur's system has failed, or where it has not been applied. I hope I have shown sufficient reason that that action should, in the present state of knowledge, be simply palliative (by the employment of the most powerful narcotic drugs), and that all tentative treatment by drugs or the hot-air bath, being quite powerless to cure, and, consequently, cruel, should be abandoned.

A NOTE ON METHYLENE AND OTHER ANÆSTHETICS.

BY SIR SPENCER WELLS, BART., F.R.C.S.,
Surgeon to the Queen's Household.

EARLY in May I received from Berlin a sample of "methylene-chloride," manufactured by Riedel, of that city, with a paper on some of the different specimens of that anæsthetic by five other chemists, from a recent number of the *Pharmaceutische Zeitung*. I was informed that Professor Bardeleben had tried, and had been quite satisfied with, Riedel's methylene. Inhaling a little myself, it appeared to me very much like the methylene most used here, but rather stronger; so I asked Dr. Day to make some comparisons of it with that to which we have been accustomed for many years past. And having to operate at Manchester on May 13th, and at Liverpool on the 14th, I took some with me. At Manchester I had to do splenectomy; and Dr. Scott, who had administered ether in Edinburgh for Dr. Keith for two years, preferred ether to anything he had not used before. So he gave ether, with most satisfactory result. I never saw more quiet or complete anæsthesia, and I am glad to say that the patient has done well; but for two or three hours after the operation she had a flushed face, hot forehead, and excessively rapid pulse. I could scarcely count it, but it was certainly over 150. I supposed that this might be due to the altered circulation following removal of the large spleen; but Dr. Scott said that Dr. Keith had remarked to him upon the occasional occurrence of very rapid pulse after his operations. As I could not remember such rapidity after methylene, I was somewhat confirmed in my dislike to ether. I have seen it used alone, or after nitrous oxide, and I have never been quite pleased with the effects. Once Dr. Matthews Duncan and I simply examined a lady after the use of gas and ether, and for twenty-four hours afterwards she was in a state of hysterical, almost maniacal, excitement, which would have been alarming after any severe surgical operation; and more than once I have had to deal with troublesome bronchial irritation after the use of ether. At Liverpool, on May 14th, I performed ovariectomy assisted by Dr. Grimsdale and his son, and Dr. Rawdon (who had been accustomed to the use of methylene) used the sample of Riedel's preparation which I had with me. At first the action seemed rather quicker than usual, but towards the end of the operation, although it was unusually short, it was difficult to send the vapour through the tube of Junker's apparatus, and Dr. Rawdon noticed that the rapid evaporation had led to the partial closing of the tube by a sort of snow, or frozen vapour. This I had never noticed with the London methylene. The patient went

on remarkably well after the operation, so on May 16th Dr. Day used the new methylene for a lady in London for whom I performed ovariectomy. This was also a very short operation, but the patient's face was flushed and her head hot for several hours afterwards, though neither pulse nor temperature was much raised. On the 22nd I did a laparotomy at Gloucester with Mr. Edis, and Mr. Cuthbert preferred to administer chloroform, as being more accustomed to it. It answered admirably, and the patient, like the others, went on quite well. This I have often observed when chloroform has been given for me. The reasons I gave up the use of chloroform as a rule were not only the reports of the deaths to which it had led, but my own observation. I have only once seen a death from chloroform; that was in Paris, when Dr. Waters of Chester and I stood beside Malgaigne while he amputated at the shoulder-joint. But I nearly lost a patient in the Samaritan Hospital while I was removing a breast, Dr. Snow Beck giving chloroform; and twice in private practice, while Mr. Clover was giving chloroform vapour from his bag, I have had to suspend the operation for a time, and resort to artificial respiration during ovariectomy.

It was these cases which led me, at first with the help of Dr. Richardson, and afterwards of Dr. Junker (who devised the admirably simple inhaler which is called after him) to use methylene as a rule, and other anæsthetics only exceptionally. Once in Pomerania, when about to remove an enormous ovarian tumour, none of my German colleagues would give anything but chloroform, to which they were accustomed. The patient was very weak, and I very much preferred methylene; so I showed an intelligent nurse how to administer it, and she did it quite effectually and safely. I mention this to show that any supposed difficulty in the administration of methylene by Junker's apparatus is a mistake. Since 1872 I have preferred methylene to any other anæsthetic.

In the Address on Surgery at Manchester in 1877, I said: "Given properly diluted with air, the vapour of chloromethyl has, in my experience of more than ten years with more than 1,000 operations of a nature unusually severe as tests of an anæsthetic, proved without a single exception applicable to every patient, perfectly certain to produce complete anæsthesia, relieving the surgeon from all alarm or even anxiety; and its use has never been followed by any dangerous symptom which could be fairly attributed to it." And I went on to say: "I wish I could speak as confidently of the chemical composition of the fluid sold as bichloride of methylene as I can of its anæsthetic properties; but whatever may be its chemical composition, whether it is or is not chloroform mixed with some spirit or ether, or whether it really is bichloride of methylene, I am still content with the effects of the liquid sold under that name, when properly administered." And more than ten years' additional experience has confirmed me in the opinions so expressed. Professor Tyndall told me that no chemist could make pure bichloride of methylene under ten shillings an ounce, and that when made it would not keep a week. Others said a mixture of chloroform and methylic alcohol would act as well, but on trial it did not; neither did the A.C.E. mixture of the Committee of the Royal Medical and Chirurgical Society; neither does this new preparation of Riedel's. Last week Dr. Day gave it for me in a case of amputation of a breast. The tube became so choked with snow before I began the operation that the patient could not be made unconscious, and we had to give chloroform, neither of us very willingly, as we had just heard of a patient having become so near death under chloroform that the surgeon had to do tracheotomy with a pocket-knife before animation could be restored.

Surprised at the obstruction of the tube by the new preparation, I wrote to Professor Bardeleben, and he replied on May 29th as follows: "We have made the same observation on artificial snow, and the obstruction of the tubes in using Riedel's methylene-chloride with Junker's apparatus. No doubt it is dependent on the cold produced by the rapid evaporation. We have another objection to make to this new anæsthetic substance. The anæsthesia is effected very slowly, and is not of long duration; and the nausea and vomiting begin often before complete anæsthesia has been attained."

Accordingly, until some better anæsthetic is brought forward, I shall remain content with the bichloride of methylene, made as first proposed by Dr. Richardson. I have never known it to fail, and I have never once been alarmed, or even made uneasy, by its effects in any one of more than 2,000 operations, many of them of unusual severity or duration.