b. The action, which, I think, is occasionally though rarely seen, of the acid products of combustion upon some of the most delicate colours. This I will show you, and you will see how trivial it is. It would not be difficult to overcome this objection.

Before proceeding to draw up a card of instruction for the guidance of the sister in charge, I thought it prudent to repeat observations by the acid of this chamber on the singeing-point of various textile fabrics, and on the time required for penetration of heat into beds, etc. First, as to the singeing-point; here are tables recording these observations, not yet complete. I will sum up the results, so far as I have gone.

White wool, cotton, linen, silk, and paper, may be heated to 250 deg. Fahr. for three hours without apparent injury, although the wool will show a faint change in colour, especially when it is new. Perhaps this change is not more than the change which new white flannel undergoes when it is washed. The same may be said of dyed wools and printed cottons, and of most dyed silks; but there is one kind of white silk which easily turns brown by this heat, and pink silks of some kinds are also faded by it. The same or even slightly lower temperature will, if continued for a longer period—seven or eight hours—slightly change the colour of, but not injure, white wool, cotton, silk, paper, and grey linen. A heat of 295 deg. Fahr., continued for about three hours, more decidedly singes white wool, and less so grey and white cotton and white silk, white paper, and linen both grey and white; but does not, I think, injure their appearance materially. The same heat, continued for about five hours, singes and injures the appearance of white wool and cotton, grey linen, white silk and paper, some coloured fabrics of wool, or mixed wool and cotton, or mixed wool and silk.

These observations confirm the conclusion drawn from previous ones—that 250 deg. Fahr. was the best and most safe maximum heat to use. It is interesting to observe here that Dr. Henry (see foot-note in *Phil. Mag.*, vol xi, p. 22) had ascertained that cotton would resist this temperature. I was not aware, however, of this until the spring of this year.

It is noteworthy that the singeing of any fabric depends not alone upon the heat used, but also on the time during which it is exposed; and is influenced by the presence of other articles in the stove capable of giving up moisture. The maximum temperature which I find best for general purposes—viz., 250 deg. Fahr.—does not weaken the strength of cotton, silk, wool, or linen threads, even when continued for about eight hours; neither does it, so far as my experiments go, lessen the strength of textile fabrics composed of these yarns. This statement, however, refers only to articles which have been left aside long enough to have reabsorbed their normal hygrometric moisture. A temperature of from 293 to 300 deg. Fahr., continued from three to five hours, weakens the strength of cotton-thread; probably also that of linen to a less degree—not silk, and not wool. But the fabrics composed of woollen yarn become dusty, and, I should think, would be weaker after washing. Neither they, nor yet linen fabrics, as in bed-ticking, are demonstrably weakened by this heat.

Turning now to the difficult question of securing the penetration of heat into the inside of beds, etc., I must again notice that Dr. Henry states that this is only a question of time; and so, in the main, I find it to be by observation. The accompanying table, abstracted from my notes, will show this. But the penetration of heat depends not only upon the time of exposure, but also upon the thickness of the bed, the amount of hygrometric moisture it contains, the nature of the materials of which it is composed, and perhaps also their degree of compression, as well as on the temperature used. These conditions have to be borne in mind in working any disinfecting chamber.

No.	Name, thickness, and condition of sample.	Heat used. deg. F.	Time heated.	Results. Heat of inside. deg. F.	Loss of Weight.	Singed or not.
1	Horse-hair pillow, 5 in. thick; normal moisture	251–262 2 operations	8 hours	247	1-10th	Not
2	The same, nearly dry	247	2¾ ,,	221	1-40th	Not
3	White blanket folded in 24 layers. 4½ in. thick; moist..	248	6½ ,,	214	1-12th	A little; not injured
4	Feather pillow, 5 in. thick; moist	241	7⅔ ,,	232	1-10th	Not
5	Flock pillow, 5 inches thick; moist	237–245 2 operations	23 ,,	252*	1 10th	Not
6	Horse-hair pillow, very moist, 5½ inches thick	293	3½ ,,	178	1-8th	Singed; injured
7	The same, much dryer	296	4¾ ,,	296	1-17th	Ditto
8	Flock pillow, 5½ inches thick; moist	293–300 2 operations	10½ ,,	280	1-10th	Ditto

* Perhaps an error in instrument. The inner and outer thermometers in this case may be taken as equal.

THE ACTION AND RELATIVE VALUE OF DISINFECTANTS.*

By J. A. WANKLYN, Esq.

If we heat the infectious material of, for instance, scarlet fever to a red heat, we destroy it. There can be as little doubt that, if we bring chlorine gas thoroughly into contact with infectious material, we destroy it likewise. If we boil it with oil of vitriol, or with permanganate of potash, we destroy it. Probably, too, if we soak it in concentrated carbolic acid, or treat it with excess of corrosive sublimate or arsenious acid, we render it inert. It is, indeed, highly probable that every kind of infectious material is capable of being rendered inert by thorough contact with any powerful chemical reagent.

But if the heat be only gentle, and if the chemical reagent be dilute, there is absolutely no reason for believing that, by the employment either of the one or of the other, we are so much as contributing towards the destruction of infection. There is a difference not only in degree, but even in kind, between the action of the same chemical when concentrated and when dilute. Concentrated sulphuric acid will convert cane-sugar into a lump of charcoal, but dilute sulphuric acid transforms it into dextrine and glucose, and, curiously enough, fits it for undergoing septic changes. So, again, very dilute bleaching powder has actually been found to favour the development of certain low forms of life; and Pettenkofer, as is well known, has found that germs whose development had been arrested by carbolic acid, start into life when the carbolic acid is still further diluted.

In the practical employment of disinfectants, the fact that dilution frustrates the action of a disinfectant has been very generally lost sight of. Attempts have often been made to disinfect the atmosphere. It is even said that, during the panic occasioned by the cattle-plague, the Commissioners endeavoured to disinfect the general atmosphere of the agricultural districts by turning cattle adrift with towels soaked in carbolic acid attached to their horns. I need not insist on the futility of such a proceeding; or, indeed, on the necessary futility of any effort to eliminate anything by chemical means from the general atmosphere covering our fields or occupying our streets. But it will probably not be quite needless to insist upon the impracticability of attacking the very limited atmosphere of a dwelling-house by chemical means. Certain very simple considerations will, however, suffice to throw the utmost doubt on the utility of endeavouring to purify air which has suffered contamination.

In a well known official memorandum, drawn up, I believe, by Professor Rolleston of Oxford, directions are given for the disinfection of a room with sulphurous acid. So much sulphur (the quantity proportionate to the size of the room) is to be burnt, and doors and windows are to be shut; and the memorandum winds up with the statement that, if a man be able to abide in the room for one instant whilst the disinfection is being carried on, then the disinfection is not to be depended upon. In other words, it is admitted that, not until you have put so much sulphurous acid into the air as to make it totally unfit to breathe, have you disinfected that air. The same certainly holds generally in regard to other agents; and, in short, we cannot hope to purify the air of a room by any chemical means without spoiling the air. It is, therefore, useless to try to disinfect the air. This is strikingly illustrated in reference to printed directions relative to the practice of disinfection. See, for instance, Dr. Wilson's little card, "Disinfectants and how to use them."

"Chlorine gas, poisonous and irritating to the lungs when in excess. For an occupied room, close fire-place, windows, etc., as directed under F. Pour over a quarter of a pound of black oxide of manganese in a dish placed high half a pint of muriatic acid (spirit of salt), and leave for six hours. It bleaches, and is apt to make white-limed walls sweat—useful for cabs."

Now if we take a room, say 13 feet by 13 feet and 13 feet high, or of a capacity of about 39 cubic metres (and that is not a very large room), and if we calculate what proportion by weight the chlorine liberated by the quarter of a pound of oxide of manganese will amount to, we get about 3 parts of chlorine in 1,000 parts of air. In point of fact, however, the proportion of chlorine in the atmosphere of such a room would never reach anything like 3 per 1,000, inasmuch as walls are not impervious, and during the six hours the air would have changed, and 3 parts per 10,000 would probably be nearer the true

* Read before the Public Medicine Section at the Annual Meeting of the British Medical Association in London, August 1873.

proportion. But this is the room not fit to inhabit by reason of the presence of chlorine. The minuteness of the dose of chlorine which the inhabited room receives may be left to your imaginations. To me it seems that the wisdom of the physician who places his little saucer with bleaching powder and muriatic acid in the chamber of his patient, is comparable with that of the cattle-plague commissioners who tied the carbolised cloths to the horns of the cattle.

Experience confirms that which an appeal to first principles suggests; and we are informed that, during the Franco-German war, although the hospitals stank of carbolic acid, yet wounds were not healthy. Although I believe that the purification of air which has once been defiled is a hopeless task, yet it by no means follows that disinfectants have nothing to do with purity of atmosphere. It is open to us to abstain, in a very large degree, from rendering the air impure.

By the efficient application of disinfectants to foul surfaces, we may hinder defilement of the atmosphere of our dwellings. One of the main functions of a serviceable disinfectant is that it shall be antiseptic; that it shall postpone decomposition and putrefaction until a convenient season. A good disinfectant should not itself defile air, neither should it be dangerously poisonous or corrosive. There is a very common substance which has long been used to hinder putrefaction. It does so only in a concentrated form. It has no smell; it is not poisonous. It can hardly be said to be corrosive. Its name is common salt. I hold that this substance and its analogues—the chloride of calcium and the chloride of magnesium—are the most available general disinfectants.

A SAFE METHOD OF INDUCING PREMATURE LABOUR.*

By BEVERLEY R. MORRIS, M.D., Nottingham.

It is not my intention to enter upon the general subject of the induction of premature labour, or of abortion, as the case may be; nor of the rules, as to the amount of deformity, or of other causes, which may justify any operative interference. I will suppose that it has been decided to operate; and the question which then naturally arises is, What is the safest method of producing the expulsion of the fœtus? The ordinary means have usually been either the rupturing of the membranes, thereby causing the death of the child, and its ultimate expulsion as a foreign body; or the separation of the membranes from the uterus to a greater or less extent; or galvanism, but how applied I am not aware, except that is external.

The rupture of the membranes or their separation is, without doubt, attended with considerable risk to the mother. The fatal cases are, I believe, over 6 per cent.; while the mortality of the children is 40 per cent. in the later months, and, of course, 100 per cent. at the earlier dates. This serious mortality, even in the most skilful hands, must make us anxious to lessen the danger to both mother and child, if it be possible. I fully believe that this may be done. The indication seems to be to imitate the natural process of parturition as closely as possible; while, at the same time, the means used must be available, with certainty, whenever it may be thought desirable. I think that all this may be effected by the means which I am about to submit to the section.

The process that I have to describe is an adaptation of the third method of inducing labour—that is, by galvanism; but, as far as I am aware, by an entirely different application of the principle from any before attempted. The principle involved was introduced by Mr. Dancer of Manchester many years ago, for the purpose of arresting *post partum* hæmorrhage, and this it undoubtedly effected satisfactorily; but the apparatus was so cumbersome, that few practitioners could carry it about with them, and, probably from this cause, I do not think it was ever generally used. The instrument invented by Mr. Dancer was so arranged that one pole of the galvanic current could be introduced into the uterus, while the other was applied over the abdomen; it was so constructed that either a continuous or an interrupted current could be applied. The instant effect was a powerful contraction of the uterus, and a consequent cessation of hæmorrhage.

The application of this principle to the induction of premature labour was made by Mr. John Varley, surgeon, of Nottingham, through whose kindness I am enabled to exhibit the instrument as modified by him. The mode of using this instrument is to insert the metallic point within the os uteri, and then, placing the other pole to the abdomen, pass a slight continuous current through the uterus for ten minutes or a quarter of an hour. This induces a dilatation of the os, which is further increased by substituting a larger conical point, and again continuing a gentle current for a few minutes. In each case in which this method has been used so far, labour has followed in two or three days; but, should this not be the case, it will only be requisite to apply the current daily until it does. The safest way is to expose the os uteri by a speculum, and then insert the point of the instrument through the speculum, which may then be withdrawn over the instrument. The great portability of the instrument and battery will allow it to be readily carried in the pocket, and it is always ready for use at a moment's notice; and the induced current seems to me amply sufficient for the purpose. The arrangement for giving a continuous or broken current is very simple, and entirely and instantly within the operator's power.

The instrument consists of a metallic sound, covered, except at the point, with a non-conducting material, and having a metallic connection at the handle, and so arranged as to be either broken or continuous by a touch of the finger. This intermediate part is connected with one pole of the battery; while the other pole is attached to a metallic tube or conductor for external application, either direct, or through the hand of the operator. The extreme portability of Gaiffe's battery led Mr. Varley to this adaptation, which, I trust, will soon be widely used, and lead to a safer and more efficient method of inducing uterine contraction than any at present in use. It is manifest that there are other cases in which it may be most usefully applied, as, for instance, in sluggish or atonic labour, and other similar states.

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CASE OF TWIN PREGNANCY COMPLICATED WITH HYDROCEPHALUS.

By ARTHUR S. UNDERHILL, M.B., M.Ch.T.C.D., M.R.C.S.

On the morning of June 1st, I was summoned to attend, in her second pregnancy, Mrs. B., aged 28, a handsome, well-developed woman. It was stated that she had been in labour all night, the pains recurring at intervals of every five minutes. On examination, I found the os uteri to be fully dilated, and a hand, funis, and foot, presenting. I ruptured the membranes, brought down the foot, and delivered, the placenta coming away in about ten minutes. The child was well formed, rather above than under the usual size of a single birth. I was some time in restoring vitality, as the funis had been necessarily compressed during delivery. On placing my hand externally, the abdominal tumour seemed to be very little lessened, the uterus feeling hard and resisting. There being little hæmorrhage, I examined for a second child, but could detect no definite presentation. As the parts were exquisitely tender, and the perinæum very deep, I determined to employ friction externally to the uterus, and to wait. In about an hour's time I gave tincture of ergot in tea, with a little brandy, which brought on some feeble contraction. This I repeated in about another hour, after which, feeling no presentation, I introduced my left hand, and found a globular mass blocking up the upper outlet of the pelvis. I could feel no bone and no suture; and, not being quite certain whether I had to deal with an abnormally sized vertex, or a buttock, I considered it advisable to wait another hour, at the end of which time, as the woman was very faint, and uterine action had entirely ceased, I attempted version; but finding it exceedingly difficult to pass my hand round the mass, I applied the long forceps. On employing gentle traction, I felt that the blades of the instrument were slipping off, so I applied them again, with a similar result. I then tried them in the oblique diameter, again unsuccessfully. By this means I had brought the mass down a little lower, so that I could feel the edge of a parietal bone. With a long pair of tailor's scissors I now punctured the head by the side of the parietal bone, when a large quantity of fluid and brain-matter escaped. The head collapsed, and was shortly afterwards expelled. On examining the child, I found that complicating the hydrocephalus, were a large spina bifida, double talipes equino-varus, general flexion of all the muscles, and the lower extremities very much shrivelled.

The case is an interesting one, particularly as it was a twin pregnancy. The talipes was evidently due to reflex action from the spinal lesion, and not from position. The concurrence of hydrocephalus with hydrorachis is not uncommon, since the anatomical relations of the brain and spinal cord allow the cerebro-spinal fluid to communicate. In searching through many records of obstetric cases, I have not been able to discover any similar to this one. It is interesting also, from the fact that no hæmorrhage followed the birth of the first child. This has an important bearing on the plan of treating some cases of placenta prævia, by removing the placenta first, as recommended by the late Sir J. Simpson.