CHLOROFORM ACCIDENTS.

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MR. CLOVER, in an article in the *JOURNAL* of July 15th, opposes the views expressed in my essay on Anæsthetics in Holmes' *System of Surgery*. The esteem which all who know Mr. Clover must entertain for his personal character, and his large experience in the administration of chloroform, attach a weight to his observations which compels me, though with great reluctance, to trouble your readers with some remarks in reply.

The main difference between us may be shortly stated as follows. I recommend a folded towel as at once the simplest and safest means of administering chloroform, and I hold that when this is used the respiration becomes obstructed before the circulation fails, and that circumstances occasionally arise in which the patient's life will be placed in imminent jeopardy unless the obstruction be cleared away by firm traction upon the tongue; and further that, as this interference with the breathing may occur in a very insidious manner, it is wisest for the administrator to keep his attention riveted upon the respiration rather than have it distracted by simultaneous observation of the pulse. Mr. Clover, on the other hand, regards the folded towel as a dangerous means of administration in comparison with an apparatus of his own devising, which has proved so successful in his hands that, while he has given chloroform more than seven thousand times, he has never lost a single patient, nor has he ever seen it needful to draw forward the tongue except in so far as that is done by raising the chin. But he thinks it necessary to observe the pulse, and he believes that if he had neglected it, his practice would not have been free from fatal event.

These statements of his are undoubtedly very striking, and can hardly fail to produce an impression upon your readers; but while in some respects highly instructive, they are also, I believe, calculated to mislead. When Mr. Clover tells us that out of more than seven thousand cases he has not lost one, I can but rejoice, not only because this is in itself a great and glorious fact, but because it tends strongly to confirm the proposition which I have sought to enforce; viz., that, though cases of death under chloroform must now and then occur, with which the anæsthetic has nothing to do, and though I am not prepared to deny the possibility of an idiosyncrasy of excessive rarity, and of which I have never yet seen an instance, that may make death from chloroform inevitable, yet in the main, fatal results really due to the agency of this narcotic are preventable. But when Mr. Clover attributes his great success to a special apparatus, and when, by proclaiming his own example, he practically counsels medical men in general to disregard the drawing forward of the tongue, he makes, as I believe, a great mistake, and with the best intentions, promulgates most mischievous doctrine.

With regard to the safety of the folded towel, my personal experience affords sufficient evidence. I estimate the cases in which I have been concerned in the administration of chloroform as about 3,500, and though this number is only half Mr. Clover's, the practice to which it relates is really of much greater value than his with reference to the point under discussion. In his cases, the undivided attention of the most experienced special chloroformist in the country was bestowed upon the administration. In mine, the chloroform was, as a general rule, not given by myself at all, but by young men coming fresh to the duty every three months, and whose only instructions were to watch the breathing closely, and, in case of its obstruction, remove the cloth from the face, and at once adopt means to clear away the obstacle. That under such circumstances my practice should have hitherto remained free from fatal accident is far greater evidence of the safety of the cloth than Mr. Clover's can afford respecting his apparatus. It is idle to speak of the folded towel as dangerous when it has stood a test like this.

I now come to the drawing forward of the tongue. Here Mr. Clover leaves me to choose between two horns of a dilemma. The fact that I resort to traction upon the tongue while he does not, proves, he says, either that my method of administration is at fault, or else that I resort to a needless procedure. Neither of these alternatives, however, necessarily follows. Mr. Clover, in contrasting my practice with his own in this respect, compares two things not fairly comparable, for the reasons above given. If I devoted my exclusive attention to the administration of the chloroform, I should, for aught I know to the contrary, draw the tongue forward as seldom as he. The necessity for the procedure undoubtedly implies in the great majority of cases that rather more chloroform has been given than necessary, though happily, as experience shows, not more than is safe. Accordingly we commonly see it

resorted to especially at the beginning of a session when the junior officers to whom the duty of giving the anæsthetic is entrusted are fresh at the work; and Mr. Clover is wrong in supposing that traction upon the tongue is of very frequent occurrence in our practice. I am not sure that it has been done once during the last two months, although we have had about as many operations as days. But it is of the utmost importance that students should learn the art of chloroform administration, and we regard the opportunity which they have at our Scotch schools of being instructed in this art as one of their most valuable privileges. With such beginners and with the large number of practitioners throughout the country who, unless the benefits of chloroform are to be most needlessly and lamentably restricted, must be called upon to give it without much experience, the necessity for traction upon the tongue will from time to time occur. And I have seen enough to make me perfectly sure that lives will be needlessly sacrificed if it be not resorted to. When, therefore, Mr. Clover virtually recommends medical men generally to follow him in abstaining from this practice, he gives about as pernicious a piece of advice as can well be given with reference to the administration of chloroform.

Mr. Clover gives a new theory of the obstruction to respiration under chloroform. He regards it as an act of deglutition imperfectly performed. Swallowing, he tells us, is often excited by chloroform, and "there is reason to think that the co-ordination of the movements which constitute deglutition is interfered with," so that the act "lingers in the second stage." Surely this is a mistake from beginning to end. Is there any reason to think that the act of swallowing is thus disturbed by chloroform? On the contrary, it continues to be performed in patients deeply under the influence of the anæsthetic, so that not only are the saliva and buccal mucus prevented from accumulating in the pharynx, but blood flowing into the mouth, unless the hæmorrhage be free, is safely carried down into the stomach. Were it otherwise, we should have to refuse chloroform in many cases in which we now employ it. Nor is it true that chloroform "excites swallowing." So far, at least, as I am able to judge, what it does excite is a free flow of saliva, and the secretion is either got rid of by swallowing, or, if the patient be an adult male, accustomed to another sialogogue, tobacco, he causes inconvenience to the attendants by ejecting the liquid in the manner usual with smokers. It would be as rational to say that chloroform excites spitting, as that it excites swallowing. That the obstruction to the breathing has nothing whatever to do with deglutition is plain enough from other considerations. In my article on anæsthetics, the obstruction to the respiration under chloroform is shown to be of two totally distinct kinds, one of which is removed by drawing forward the chin or turning the head to one side; while the other refuses to yield to such treatment. The former obviously depends on relaxation of the lingual muscles. I lately had occasion to remove a tumour affecting the central part of the lower jaw, dividing, of course, the attachments of the muscles by which the tongue is protruded; and the condition of the patient during the first few days after the operation reminded me exactly of the state of a person with the tongue relaxed by chloroform. He was threatened with suffocation if he lay back with the occiput towards the pillow, but breathed freely when the face was turned well to one side, or when the tongue was drawn forward in the gentlest manner by a string attached to it. An experiment that may be made on any patient affected with this variety of obstruction from chloroform is of itself conclusive. Supposing the obstacle to have been removed by the gentle traction on the tongue, effected through the medium of its muscles by pulling forward the maxilla by the beard, it will at once return if the chin is allowed to recede, and the same thing will recur time after time as often as you please. To imagine that a fresh act of semi-deglutition takes place every time you relax your hold would be absurd.

The same argument applies to the other kind of obstruction to the breathing under chloroform. This requires firm traction upon the tongue to dispel it, and it recurs whenever the traction is suspended in a manner obviously unconnected with any act of deglutition. Mr. Clover, indeed, seems never to have witnessed this variety. But before expressing an opinion upon the nature of "the obstruction Mr. Lister dwells upon," he was surely bound to read the account I gave of it. If he will turn to either the article in the first edition of Holmes's *Surgery* or its present revised form, he will find it stated that "I have more than once seen a person holding the end of the organ considerably beyond the lips without any good effect, and, placing my hand on his, have given an additional pull that has re-established the respiration." It was this observation that first attracted my attention to the subject. It struck me as something very remarkable that when the tongue was already carried to the extreme degree of extension, additional traction, though it did not alter the position of the organ, should clear away the obstruction. This seemed quite irreconcilable with the prevalent idea that mere falling back of the tongue was the only thing at fault, and I

felt anxious to know more of the true nature of the phenomenon. I found that I could imitate in my own person the profound stertor which commonly precedes and passes into the obstruction in a patient under chloroform, and that I could still produce it when my tongue was protruded beyond the range of the teeth; but that if I seized its tip with a handkerchief and pulled it forward sufficiently strongly to cause a painful sensation in the *frænum linguæ*, the snoring became impossible. The effects of drawing forward the tongue being thus exactly the same upon the artificial stertor as upon that under chloroform, I could not doubt that the two were identical, so that I had the opportunity for further investigation in my own throat. By digital examination I ascertained that the epiglottis was not folded back in this stertor as it is in swallowing, but that the seat of the vibrations was more deeply placed, somewhere about the orifice of the larynx. I next made an attempt to see its exact mechanism in an animal under chloroform. The experiment was performed upon a sheep in a slaughter-house in Glasgow; and though it was abruptly cut short by the advent of an official who threatened to have me up before the Provost and magistrates if I persisted in such cruelty to animals, I saw enough to interest me greatly. The animal having been brought deeply under the influence of the anæsthetic, I made a free transverse incision in the throat so as to enable me to look fairly down upon the orifice of the larynx. But, to my surprise, instead of seeing the vocal cords, I saw only a pair of vascular valves flapping with great rapidity, and concealing entirely the true vocal apparatus. I then availed myself of the laryngoscope for further observation in my own person, and at once ascertained what the valves I had seen in the sheep really were, viz., the thick, pulpy, vascular folds of mucous membrane surmounting the apices of the arytenoid cartilages; and I saw that if I coughed, these folds were brought into contact in the middle line, and, at the same time, were carried forwards to the base of the epiglottis, so as to obstruct the exit of air till the expiratory effort had continued sufficiently long to produce a due outburst, when the obstruction was removed. Thus the strain in coughing is not allowed to fall upon the delicate mechanism of the vocal cords, but is borne by these valves of mucous membrane. When anything irritating is present in the air inhaled, these valves guard the inlet to the larynx and render it as small as possible, as I had seen in the sheep during the inhalation of chloroform; and they constitute the mechanism of what I have termed true laryngeal stertor. On producing this stertor while examining my throat with the laryngoscope, I saw these pulpy folds vibrating at the base of the epiglottis; and on increasing the stertor to complete obstruction, I observed them closely applied and motionless. Then, on drawing forward the tip of my tongue with one hand, while the other held the laryngoscopic mirror, as soon as the traction was carried so far as to become painful to the *frænum linguæ*, while the epiglottis was not moved forward in the slightest degree, the arytenoid folds receded from its base in spite of the effort of my will to the contrary, rendering the obstruction and the stertor alike impossible. Thus the view to which I had been previously led became established, viz., that the firm traction on the tongue operates not mechanically, but in a reflex manner through the nervous system. And this conclusion has since received confirmation from a remarkable case (to which I have drawn attention in the revised article) where, the chest continuing to heave, the obstruction to the respiration remained in spite of firm traction on the tongue, but yielded at once to a dash of cold water, which could only operate through the nerves.

I cannot but regard these facts as of vital interest to the administrator of chloroform; and my apology for bringing them at such length before your readers must be that, though they are duly recorded in the article which Mr. Clover criticises, he does not appear to have noticed them, but confuses together the lingual and laryngeal obstruction in his very crude theory of imperfect deglutition.

Long as this communication already is, I must crave your indulgence while I add some remarks on Mr. Clover's statements regarding the influence of chloroform on the circulation. He is "convinced that the chief cause of danger is the effect of chloroform upon the heart;" and he alludes to fatal cases which have been reported by others where the pulse is said to have failed before the breathing; and he also mentions some experience of his own tending in the same direction. On Mr. Clover's own observations in this respect, I place implicit reliance; and to these I shall have occasion to refer again. But I confess to feeling little confidence, as a general rule, in the reports of deaths under chloroform. I know from experience how insidiously the laryngeal obstruction may supervene, and how readily the heaving of the chest under such circumstances may be mistaken for efficient respiration; and I have described in detail in my revised article, a case illustrating very strikingly how a fatal event commencing with asphyxia might be mistaken for a death from the heart. (See *Holmes's Surgery*, 2nd Edition, vol. 5, p. 497.) I know, also, how conflicting the evidence of bystanders com-

monly is with regard to the precise succession of the highly important events that are crowded together in the brief period of excitement immediately preceding the patient's dissolution; and we know, further, the strong tendency that exists in the mind of any one in whose hands such a case has occurred to twist the evidence unconsciously to himself in favour of inevitable syncope, rather than preventable asphyxia; and when we bear in mind how liable the attention of the administrator is to be distracted by the interesting circumstances of the operation, to suppose, as some appear to do who write upon this subject, that no such thing as death from chloroform through negligence ever occurs, and that all the whitewashings of coroners' inquests are strictly accurate, is to carry charity far beyond the bounds of probability. It must also be borne in mind that death must sometimes occur under chloroform as a pure coincidence; and, indeed, it is remarkable how many cases of this sort have been substantiated by indisputable evidence (*Vide op. cit.*, p. 484). In one or other of these ways, I believe the great majority of alleged instances of death from chloroform by syncope may be accounted for.

But to return to Mr. Clover. He remarks that, "supposing the larynx to be obstructed at a time when the heart is acting well, and when not more than four or five per cent. of chloroform-vapour is in the chest, the narcotism will diminish" through diffusion of the chloroform among the tissues. But he forgets that meanwhile the narcotism from asphyxia will be rapidly increasing and that asphyxia, together with a full dose of chloroform is an exceedingly formidable combination. That my dread of it is no groundless apprehension, is sufficiently proved by the case lately referred to, as detailed in my revised article. In that instance, the breathing being obstructed, but the chest still heaving, the face passed through the condition of asphyxial lividity into what was physiologically identical with *post mortem* pallor; and although the heart had not yet failed, it was obvious that death was imminent, when traction upon the tongue happily restored the patient to life. But I cannot believe it to be really needful to convince Mr. Clover that asphyxia under chloroform is dangerous. His own practice of raising the chin, which he has "never known to fail" in relieving the obstructed breathing, seems sufficient evidence that he is in reality quite alive to the risk.

Mr. Clover proceeds to contrast what is observed when a patient holds his breath while inhaling air containing only four per cent. of chloroform, with what would be likely to occur if the proportion of the narcotic vapour were as high as ten or twelve per cent. I am compelled to enter an earnest protest against the idea here conveyed, that anything approaching such high percentage is likely to occur when a folded towel is used. To suppose such a thing is not only to make a purely gratuitous assumption, but to ignore entirely a very careful set of experiments which I made several years ago and published in the article referred to (*vide op. cit.*, p. 486), in order to ascertain the amount of chloroform really given off by the towel. The circumstances which led me to enter upon those experiments were not very dissimilar from those which have now arisen. Dr. Snow had ascertained, by observations upon the lower animals, that in chloroform-poisoning, if more than about five per cent. of the vapour existed in the air, the creature died from primary failure of the heart; but, if the percentage were kept below that figure, the respiration failed before the circulation. And with respect to the latter fact I beg leave to refresh Mr. Clover's memory. Having come to this conclusion, Dr. Snow invented an inhaler, as Mr. Clover has done, to regulate the proportion of chloroform to the atmosphere; and, finding it answer well on a very extensive trial, and forgetting, like Mr. Clover, how largely his success might be dependent on care in the administration, he jumped to the conclusion that fatal events in the hands of others were due to primary failure of the heart, in consequence of too large a proportion of chloroform assumed to be present when the folded towel was employed. Meanwhile the folded towel had long been used in Edinburgh with a success even greater than that attained by Dr. Snow; and, when anything indicated an overdose, it was not failure of the heart, but an obstructed state of the respiration. How was this inconsistency to be explained? Were Dr. Snow's experiments fallacious? or was he wrong in assuming that the cloth involved a risk of too great percentage of chloroform? In the hope of clearing up this difficulty, I resorted to experiments; and, carefully avoiding all apparent sources of fallacy, I ascertained that, if a drachm and a half of chloroform were used, the whole amount given off by the under-surface of the cloth at the temperature of 70 deg. F. during the first half-minute after the liquid was poured upon it, was only such as to produce a percentage of 4.5 to the air inhaled by an average adult, supposing all to enter the lungs. But in reality a considerable quantity is not inhaled at all, part of the heavy vapour falling down beside the patient's face, and part being blown away by every act of expiration. It would be difficult to estimate the

precise amount of this loss; but that it must be very great is evident from another consideration to which I directed attention—viz., the safety with which chloroform is given to infants by means of the towel, without any precaution to adapt the quantity of liquid used to the small capacity of their lungs. If anything near the whole of the chloroform that comes off were inhaled by children, its proportion would be enormously high and necessarily deadly. Their immunity from danger proves, therefore, the greatness of the waste of the anæsthetic.

From these facts it seems probable that, even when the chloroform has just been poured upon the towel, the amount actually inhaled does not exceed that which Mr. Clover uses as a constant quantity throughout the administration. And, considering how rapidly the rate of evaporation from the cloth diminishes, it is manifest that the average quantity taken into the lungs when this method is employed is very greatly below that supplied by Mr. Clover's apparatus. Mr. Clover may perhaps reply that this is impossible, seeing that patients cough more with the towel than with his apparatus, implying that what they breathe is more pungent, and therefore stronger. But greater pungency of the inhaled air does not necessarily imply a larger proportion of the irritating material. Any one who has watched the clear blue Rhone flowing side by side with the milky Arve, far below their place of confluence near Geneva, will readily understand this. When the chloroform has just been poured on the middle of the cloth, its vapour falls in very large proportion from the moistened part upon the mouth and nose; but from the sides of the towel, and from beneath its edges, pure air rushes in simultaneously, and the two fluids pass imperfectly mixed into the fauces and trachea. On the other hand, the atmospheric gases and the chloroform are perfectly blended in Mr. Clover's balloon; and just as a glass of spirits and water well stirred tastes milder than the same ingredients unmingled, so the gases inhaled from Mr. Clover's apparatus appear more bland, though in truth more potent, than the unmingled gases beneath the towel.

I have been accustomed to regard Mr. Clover's apparatus as in his hands a harmless luxury; but when I find him at the outset of his article denying broadly that in deaths from chloroform the respiration is affected before the circulation, and when he relates in illustration a case "out of many which tend to show that the pulse must be watched", where during tranquil inhalation the pulse gradually failed and became imperceptible while the breathing continued free, though this also afterwards ceased, in spite of discontinuance of the inhalation, so that the patient seemed for a while really dead, I am led to doubt whether experience so entirely contrary to ours with the towel may not be due to the larger proportion of chloroform given. These statements of Mr. Clover make me more than ever satisfied that the cloth, though a rough and ready and somewhat wasteful means of administering chloroform, is the safest of any yet devised.

But even if Mr. Clover should reduce his percentage of chloroform, so as to ensure primary failure of the respiration rather than of the heart, it would not follow that his apparatus would be a desirable thing for general adoption. Its only advantage is the avoidance of irritation of the air-passages by the pungent vapour; and this, after all, is but a trifling inconvenience, and patients seldom complain of it. On the other hand, not only are Mr. Clover's bag and mouthpiece a cumbersome means of effecting the object, but, though perfectly safe in his hands, they might not always prove so in those of others. I lately heard of a case in which this apparatus being employed, and the patient showing no signs of being influenced by the anæsthetic, it was found on investigation that the introduction of chloroform into the balloon had been omitted. Now, it is certainly full as likely to happen that too large a supply might be introduced through ignorance or hurry, in which case a more efficient means of quietly destroying human life could hardly be devised. For an agent adapted, like chloroform, for universal employment by medical men, it is of the utmost importance that the means of administering it should be as simple and uncomplicated as possible.

Before concluding, it may not be deemed out of place for me to advert shortly to Mr. Syme's experience with chloroform, more especially as my practice is simply that which I learnt from him. Though I may hope that I have done something to elucidate its theory, I have added nothing to it. It remains as it was at the outset, except that, instead of the tongue being drawn forward on every occurrence of obstruction to the breathing, it has of late years been found that the slight puncture inflicted by the artery-forceps may often be avoided by the means before referred to. With Mr. Syme, from first to last, a cloth on which the chloroform was freely poured was the only means employed. The breathing was taken as the only guide, while no inquiries were made into the state of the heart; and the administration was conducted by junior officials of the hospital. Yet in a surgical career which has been estimated as involving the giving of chloroform

upwards of seven thousand five hundred times, he did not meet with a single death from the anæsthetic.

I venture to hope that this discussion, though I fear it has been on my side carried to a tedious length, may tend to increase the confidence of the profession in this safe and simple practice. Should this prove to be the case, I shall not have abused the patience of the reader.

INTESTINAL OBSTRUCTION FROM A KNOT ON THE LOWER PART OF THE ILEUM.*

BY MICHAEL W. TAYLOR, M.D., Penrith.

MRS. S., aged 40, the wife of a small farmer, was a full-sized, finely made, robust woman; she had been the mother of several healthy children, the youngest of whom, aged fifteen months, she had suckled until quite recently. She had led an active and regular life, and possessed all the attributes, and indeed had been in the enjoyment, of perfect health, and had never been subject to more than the most trivial derangement of the bowels. On Thursday, November 17th, 1870, whilst engaged in baking, on lifting a heavy pan from the ground, she was sensible of having strained herself on the left side of the abdomen, but the pain passed off directly and she ceased to regard it. Another circumstance which ought to be mentioned in this place as a possible cause of the intestinal entanglement, the results of which were soon to be developed, was, that on this evening at supper, and also on the following morning, she feasted very freely on boiled cockles, which to her were a very unusual kind of regalement. I have no evidence to show whether the slight "overreach" or this indigestible meal was the cause of her illness; but on the evening of Friday she was seized with a cramp and twisting pain in the abdomen, recurring acutely for a time, and abating for intervals of twenty or thirty minutes. During the night she vomited frequently: what was rejected was not food that had been taken, but a yellow bitter fluid, having the colour and appearance of bile. A dose of castor-oil which was given to her was speedily vomited.

She was first visited in the forenoon of Saturday, November 19th. Her expression wore an anxious appearance, as of a person suffering from abdominal distress. The gripping pain was referred chiefly to the epigastrium; it was intermittent, and there were periods of tolerable ease; at other times she turned from side to side, as if suffering severely. There was but little tenderness or distension over any part of the abdomen—indeed, pressure somewhat relieved her. There was no increase of temperature, nor of rapidity of pulse beyond 80. The tongue was coated white. The last motion which passed from her bowels was on Wednesday. There was no abdominal protrusion, nor abdominal enlargement, nor thickening to be felt in any situation. On viewing the case at this period, the symptoms accorded with the presumption that the attack might turn out merely as one of colic, from the irritation of indigestible matter, and might pass off with the removal of the offending cause. To effect this, blue pill and colocynth were given, with alkaline and sedative draughts; and frequent hot linseed-meal poultices were ordered. These pills and a portion of the medicine were retained, and it seemed that during the remainder of the day she felt herself easier and thought herself better, although no evacuation from the bowels had been procured. The night, however, was one of great suffering, from the frequent and violent recurrences of the pain and vomiting, so that a summons came to visit her at five on the morning of Sunday, the 20th. The matter vomited was green bilious fluid; it was rejected from the stomach with considerable force. The pain was still referred to the epigastrium, over which she acknowledged a slight amount of tenderness, from which, however, the rest of the abdomen was free, although it was now becoming slightly rounded in contour and tympanitic on percussion. An enema of one pint of warm water and eight ounces of olive oil was injected, which had the effect of relieving the lower bowel of a considerable quantity of fecal matter in the form of scybala and pellets, but not of a loose nor fluid motion. Two pills, with a quarter of a grain of morphia in each, were left for administration every two hours. Shortly afterwards she obtained ease; she was able to take a little beef-tea; the paroxysms of pain and sickness ceased, and she obtained some sleep. On visiting her in the evening, the pain in the epigastrium, though not so severe, had become more constant, and more aggravated by pressure; there was more general distress and restlessness, and great thirst, dry skin, dry tongue; pulse 64, hard. She had vomited recently, from time to time, a quantity amounting to a washhand-basin full of brownish-yellow fluid, with a distinct fecal odour. There had been no further evacuation from

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