ON POST MORTEM TEMPERATURES.

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THE medical evidence arising out of the terrible combination of circumstances known as the "West Haddon tragedy" has, it would appear, not unjustly roused the indignant comments of those best competent to criticise the opinions which were given at the inquest. The main question raised appears to be this: "How far can a high temperature after death be taken as evidence against death by syncope?" In some of the communications which have appeared on the subject, reference has been made, amongst others, to some observations of mine on the temperature of patients immediately before death, which were published in the *Guy's Hospital Reports* for 1869-70. Those cases, forty-three in number, embracing all kinds of diseased conditions, and some deaths at any rate from syncope, led me to the conclusion that, except in isolated cases of long standing disease accompanied by emaciation, the temperature is not materially lowered at the time of death. An article by Dr. Wilks and Dr. Swaine Taylor, published in the *Guy's Hospital Reports* for 1863, "On the Cooling of the Human Body after Death," had already shown that rapid cooling of the body after death did not necessarily attach to the state of syncope; and the two series of cases, taken together as complementary to each other, supplied, I had thought, all the information that was wanted on the subject. At the time when the facts relating to temperature during the last hours of life were being recorded, I was engaged also on *post mortem* temperature in its relation to rigor mortis; and in nineteen of the cases where the temperature had been recorded previous to death, the rate of cooling afterwards was noted also. Looking over my notes subsequently, it did not appear that they added anything to our knowledge of the subject, but merely confirmed what had already been determined by Dr. Wilks and Dr. Taylor. I found it difficult to draw any conclusions from them which should be at all general in their application, with this exception, which at the present juncture should be pointedly enforced, that an opinion must be formed of each case according to its own condition and that of its surroundings; that it is not so much the disease that the patient died of, which will determine the rate of the loss of heat *post mortem*; nor is it the mere cooling or the reverse which will help to an opinion as to the cause of death; but that it is rather this: a body just dead has a certain amount of heat to lose—will it part with this slowly or quickly? To determine this, its facilities for parting with heat must be ascertained, *i. e.*, the atmospheric temperature, the moisture, the wrappings of the patient, the condition of the body as to fat and muscle, the state of the skin, the time of accession of rigor mortis, and so on; and even then, all these things being estimated as much as possible at their own proper value as factors in bringing about a certain state, there would still seem to be such a margin of opportunities—mostly owing, perhaps, to states or reactions going on within the body, and therefore not perceptible to our senses—of falsifying the conclusion arrived at, that I believe it to be almost, if not quite, impossible to go farther than we had already gone in saying that, isolated instances of retention or rise in body-temperature excepted, the dead body cools slowly and progressively, and that emaciated bodies cool more rapidly than fat ones. But, to put aside this general conclusion, the opinion in question has been formed upon an exceptional case. It is necessary, therefore, to examine any observations at our command, to see in what direction they tend. It is a little difficult to deal with the opinions given in the West Haddon case in one way, for the statement upon which they seem to have been founded may bear either of two interpretations. Mr. Walker testified that he found "a high temperature of the body after death." This, I take it, might mean that the temperature was higher than that of a healthy living body, or else that it was high in comparison with that of other dead bodies. In the report of the case which lies before me (*BRITISH MEDICAL JOURNAL*, January 17th), it is not stated at what time after death the *post mortem* examination was made, neither is any mention made of the temperature during life; at the same time, it is hardly possible to form an opinion of any value upon the case without such evidence be furnished. It will be seen that thus three different aspects of *post mortem* cooling, or three different conditions under which it may have to take place, must be considered in order to come to any conclusion as to the accuracy of the opinion of the medical expert.

1. The body-heat may rise subsequently to death.
 2. A high temperature may exist before death.
 3. A low or normal temperature may be present at the time of death.
- What effect will these have upon the rate of cooling of the body? Of the first, that of increase of temperature after death, so little is

known, that it cannot be foretold under what circumstances it may or may not occur, or, supposing it to have taken place, what pathological states its existence negatives. The temperature of the body has been noticed by Dr. Dowler to rise especially in yellow fever and cholera. But it occurs in other cases also; Dr. Wilks and Dr. Taylor having placed on record, in their article already quoted and referred to in the *BRITISH MEDICAL JOURNAL* for January 24th, an instance of Bright's disease with erysipelas, where the temperature rose three degrees in the two hours between the first and second observation. I have also noted the same occurrence once in a case of extreme emaciation from the drain produced by a discharging abscess in the liver. The rise began nineteen hours after death, and resulted in an increase of 4 deg. (71 deg. to 75 deg.), and this notwithstanding a fall of 4 deg. in atmospheric temperature. Rigor mortis was disappearing at the time, but nothing indicative of commencing decomposition was observed. In a second case, where death resulted from tetanus, the temperature remained at 95 deg. in the axilla between the observation at two hours and that two hours and three-quarters after death, and at the latter period it was 97.6 between the labium and thigh.

I shall not allude further to this condition of things, for the reason given above, that we have no sufficient data on which to go in its discussion; and Dr. Moxon has already pointed out how, even supposing an increase of temperature to have occurred in the case in question after death, it is precisely in diseases where death does indeed occur from syncope that this rare *post mortem* change has been the most frequently noted. The fact is, that death from syncope includes deaths from or with high temperature and deaths at normal and lowered temperature; while, if this be so, and the fact cannot be questioned, it at once disposes of the main point at issue, and destroys the foundation of any arguments for or against death by syncope which rest upon the temperature of the body afterwards.

Restricting the term syncope, as I apprehend has been done in this instance, to a group of cases where people in advancing years die from a fatty state of the muscular fibre of the heart, it would be fair to assume that the temperature was near the normal at the time of death; and, according to the fat and good condition of the body or the reverse, so to some extent would be the rapidity of cooling. In such a case, a cool surface coupled with a flabby heart would be not inconsistent with death from syncope. Very little more than this could be said. It certainly would not be correct to say that any other state of body-heat was not associated with a state of syncope, else in many cases of phthisis, for instance, with emaciation and high temperature at death—where, also, I apprehend, death comes at last by syncope—how do the patients die?

The second point, *viz.*, the influence of a temperature higher than that of a healthy person at the time of death on the cooling afterwards, is of some importance here, inasmuch as, lacking any statement of thermometrical observations previously to the death of Mrs. Gulliver, it seems quite possible that, with a history of vomiting and drowsiness, the temperature may have been high when she died. In my notes, I find nine cases where the temperature shortly before death was over 102 deg. The average loss per hour, during the first few hours after death, was 3.37 deg. These included both well-conditioned and emaciated subjects. The average loss to emaciated subjects only, taking cases with a temperature of 100 deg. and higher, was 4.55 deg. Individual exceptions, however, were numerous: thus a robust child lost 8 deg. in an hour; another body (of a boy aged 19) lost 4 deg. in half-an-hour; and others might be given. Indeed, most bodies decline in surface-temperature very rapidly during the first hour or so after death, compared with the loss afterwards. This will be seen in the subjoined table, where, from the materials at my disposal, it has been attempted to express in figures the rate of cooling in robust and emaciated people where the temperature was normal at the time of death.

	Observations under three hours from time of death. Showing loss per hour.	Observations under six hours from time of death. Loss per hour.	Body nearly cold. Loss per hour.
Emaciated.....	4.7	3	1.12
Robust	3.5	3	1.26

It will thus be seen that the difference between the rate of cooling in the two classes is not so very marked, though it is sufficiently so to accord with the observations of previous writers on the subject.

Having already said that the rate of cooling is variable in different bodies, it is only another mode of expressing the same fact, to say that the time when a body will be perfectly cold is not to be predicted with any certainty. I have watched bodies, at intervals, through periods of 105 and 116 hours, well into a state of decomposition; and they had not then, as gauged by a thermometer on the surface of the body, reached the temperature of the atmosphere. That period, included

between 30 and 45 hours, would, however, take in the larger number of cases. I have noted no instance of very rapid cooling.

In dealing with instances of death by emaciation, the *post mortem* temperature in cases of syncope, such as that of Mrs. Gulliver may be supposed to have been, has been practically disposed of, and requires no special treatment. Amongst my notes, I find three cases—two of old people brought in dead and one a case of aneurism with death by hæmothorax—which would all be in point, and in none of them was the decline of temperature more than ordinarily rapid. As to drawing any conclusions, then, as to the cause of death from the temperature of the body afterwards, whether high or low, such a course is surely not justified by what knowledge we possess upon the point.

With reference to rigor mortis, it appears that, under the external conditions in which these observations were taken, its supervention was somewhat earlier than is usually stated. The average time in twenty-eight cases gives five hours; and this, probably, is near the time required for its complete development. The condition, however, takes usually from one to two hours at least to mature; and the average time for the commencement of rigor mortis, as evidenced by stiffness of various parts of the body, may, I think, be correctly stated at from three to four hours. It would not appear to be dependent upon the temperature of the body; for, though the greater number of cases are found with a surface-heat of about 88 deg., eight occurred out of fifty-three in which the thermometer stood at 90 deg. and upwards when the rigor was distinctly marked. Two of these were cases of tetanus, and are therefore open to the objection that theirs was rather the muscular spasm said to simulate rigor than the actual rigor itself; one had a temperature of 95 deg., and the stiffness ensued within ten minutes of death; the temperature of the other was 94 deg. It is possible that the early supervention of rigor mortis might have some influence in lowering quickly the surface-temperature of the body.

OBSTETRIC MEMORANDA.

TREATMENT OF *POST PARTUM* HÆMORRHAGE.

XI.

THE following brief notes of a lamentable case of *post partum* hæmorrhage will serve as an appropriate introduction to the description of a mechanical contrivance, which I beg to suggest as a means of meeting the perils of such formidable cases.

A finely developed lady in mid-life, who had passed safely through five previous deliveries, became again pregnant. One evening at full time, the pains becoming regular and severe, her medical attendant was sent for. The membranes had given way about a week previously. About midnight, the os being fully dilated, the pains strong and frequent, but the head making no progress, a dose of ergot was given, the pains soon became violent and incessant, and in half an hour the child was born nearly asphyxiated. The separation of the placenta was followed by the pumping out of an immense quantity of bright fluid blood. The medical man, an experienced practitioner, tried the usual remedies of cold water and compression of the uterus, but without avail, the patient dying within half an hour of her delivery.

In this case, then, although under the powerful effects of secale, hæmorrhage occurred. Again, had it or had it not any share in producing the asphyxiated condition of the child? But what I would especially draw attention to, is the difficulty that occurred in procuring ice. The neighbouring fishmonger, although told by the husband that his wife was dying, withdrew his head from the window, telling him that he would not be disturbed. If this difficulty can occur in one of the busiest suburbs of London, how would it fare with a provincial practitioner? It seems, excepting in large towns, that ice must be almost excluded from our list of remedies; and even in them, how often is it available? Ergot, if the evidence of the case I have related go for anything, is not to be trusted. No doubt, Nature's remedy of fainting is very valuable. Would it, let me ask, be advisable to raise the woman, so that fainting might occur as soon as possible?

There remains one other most valuable proceeding, namely, pressure externally, and the introduction of the hand into the uterus, pressure being made by the fist on the site of the detachment of the placenta. It is on this principle I would act, but, instead of the fist, I would introduce an India-rubber bag, which should then be distended with water, ice-cold if possible. Surely, this would make more effectual pressure on the placental site than the fist; like the fist, it would tend as a foreign body to induce contraction; but, unlike the fist, it would not tire, neither would it exclude the simultaneous application of cold, which, if ice were not procurable, might be ensured by some freezing mixture, and

which, though inadmissible for direct injection into the uterus, might, of course, be used for distending the bag. Next comes the question of what is to occur when the uterus begins to contract. It evidently would not do to leave an unyielding substance in the uterus when it begins to contract; to get over this difficulty, Messrs. Louis Blaise and Co., of St. James's Street, have undertaken to add a discharge tube fitted with a valve, which opens so soon as the uterus begins to contract on the bag. I do not know whether this plan of combined external and internal pressure have been tried; if it have, I should much like to know with what result.

I may add, that Messrs. Blaise and Co. manufacture for me Dr. Barnes's bag for dilating the os uteri with a hollow style passing right through the bag up to its fundus, through which it is injected, and which also serves to introduce it; by this means, I do away with the pocket on the outside of the bag, also with the extra instrument which is used for its introduction. Not only is the bag thus simplified, the means of its introduction being an integral part of the bag, and the expense diminished, as no second instrument is required, but a much more important advantage is gained, namely, facility of introduction, for, by putting the bag on the stretch over the inside style, it may be almost as easily introduced as the uterine sound, and thus becomes available in those cases of early abortion where the os is small, and where Dr. Barnes confesses that the bag, as he uses it, is impossible of introduction. Another no small advantage is, that the bag can be much more easily kept *in situ*, and the chances of its slipping out of the os during dilatation entirely avoided.

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XII.

In a tolerably large midwifery experience, I have had the good fortune to see comparatively little of *post partum* hæmorrhage—a result mainly due to my invariable practice (when there is time) of placing an abdominal binder round my patient, and directing the nurse to keep up firm and continuous pressure upon the uterus; and, if there be a history of previous flooding, I give a dose of ergot (liquid extract of the *British Pharmacopæia*). I have within the last few days attended a lady who, from dread of the treatment pursued at her last confinement, when there was alarming hæmorrhage, had fallen into a low nervous condition, which caused great anxiety to her friends. In this, her third delivery, she had done well, with no flooding whatever.

With regard to the question of the injection of astringents into the uterus, I cannot say much, but should not hesitate to use the perchloride of iron where the ordinary means had failed. Two years ago, I was called to a patient who had been delivered four hours previously, and found that she had been flooding for some considerable time, the uterus continually relaxing as soon as the hand was removed. After repeated doses of ergot and the other usual means had been exhausted, I injected (in the presence of another medical man) two ounces of the tincture of perchloride of iron in eight ounces of water, which not only controlled the bleeding, but caused firm and persistent contraction of the uterus. No bad symptoms followed, and she made a good recovery.

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THERAPEUTIC MEMORANDA.

CHLORAL HYDRATE AND CAMPHOR: CROTON-CHLORAL.

I.

LAST year (*London Medical Record*, May 7, 1873), attention was drawn to the fact that the intimate mixture of equal parts of chloral hydrate and camphor will produce a clear fluid, which is of the greatest value as a local application in neuralgia. I have now employed this preparation for several months, and have induced many professional friends to use it also. Having in every case found great, and often instantaneous, relief follow its application, I think the members of the Association may be glad to have the opportunity of adding to the very uncertain stock of antineuralgic remedies which we have already at our disposal. Its success does not appear to be at all dependent on the nerve affected, it being equally efficacious in neuralgia of the sciatic as of the trigeminus. I have found it of the greatest service in neuralgia of the larynx, and in relieving spasmodic cough of a nervous or hysterical character. It is only necessary to paint the mixture lightly over the painful part, and to allow it to dry. It never blisters, though it may occasion a tingling sensation of the skin. My friend Mr. George Wallis allows me to say that he has found it of great service as a remedy which patients can apply themselves for the relief of toothache; and to its success in this respect I can also personally testify. In the original article, the com-