presses the child downwards, and the head is brought to bear on the lower uterine segment. When the internal uterine pressure is greater than the axial, the waters are forced downwards past the presenting part, which recedes. When, however, the axial force is the greater, and can act through the foetus, the contrary effect results; the water is forced upwards, and the head is brought into close proximity with the lower portion of the uterine walls. When the child is thus forced down during a pain, the uterine walls closely surround the head, and the membranes being still entire, the liquor amnii is divided into two portions; that in front of the head is called the forewaters. If the division be complete, then the entirety of the membranes is really a disadvantage; for now the forewaters but impede the more powerful action of the axial force. If the separation be incomplete, then the expansive action is only obtained, the internal pressure being still in excess of the axial. If the reverse be the case, the forewaters are but forced back above the head. By the mode of action, the internal uterine pressure is the force which tends to expand the lower uterine walls. Acting, in fact, like a glove-stretcher, its expulsive power can only act on the entire ovum, and is, therefore, at a disadvantage. The axial force is exerted mainly through the foetus, and can exert its full strength only after the membranes are ruptured.

It seems, therefore, evident that *the function proper of the bag of waters should be limited to that of expansion only*. But the full dilatation of the os is effected, not by expansion alone, but also by longitudinal stretching. When, therefore, we find dilatation tardy from defect in degree or direction of the power alone, and not from any inherent character of the tissues, when once it is evident that the lower segment of the uterus is well expanded, the rupture of the membranes is the most effectual means of favouring the dilatation, by bringing the axial force into full action, and this irrespective of the degree of the size of the os.

By the researches of Dr. Matthews Duncan on the Power of Natural Labour, a beginning has been made to place this subject on a more purely scientific and accurate basis; but we are not yet in a position, and it requires qualifications which few possess, to follow up the subject as he has done. He has, however, shown mathematically, what has been long practically known, that partial evacuation of the liquor amnii is an efficient way of improving the power of the uterus, even when defective in amount. "It is a common belief," he says, "that the uterine pains increase in strength after the evacuation of the liquor amnii. Whether this be true or not, as commonly believed, I do not here consider. But it is certain that, if the uterine contractions remain of the same force after as before the partial evacuation of the liquor amnii, the power of the labour or the extruding force will be increased, as the curvature of the contracting organ is increased." (*Researches in Obstetrics*, page 315).

Having laid down the basis of our knowledge, it remains only to discuss the diagnosis of the conditions which warrant us in having recourse to rupture of the membranes before the full dilatation of the os. The first point is the determination of the degree of expansion of the lower uterine segment. We have seen that the size of the external os is no criterion of expansion. The os, in fact, may be very small, and yet expansion may be complete. It is by the internal os that we can best judge, but this is hard to reach, and difficult to determine its exact site. There is one means, however, of ready access, whereby we can form a proximate opinion: it is the degree of dilatation or updrawing of the vaginal *culs-de-sac*. This is a point which has been entirely left out in the consideration of the progress of the first stage. It is a matter of common experience to find, in the class of cases where we feel something is required to promote a labour with tardy dilatation of the os, that the upper part of the vagina is well expanded and drawn up, greatly increasing the perceptible diaphragm of the cervix, which alone obstructs the continuity of the developed canal. Now, we know that the longitudinal muscular fibres of the vagina run upwards, and are continuous with those of the body of the uterus, and that the attachments of the uterus in their upper portion correspond with the internal os. This portion, then, cannot undergo expansion without carrying with it the tissues which are in connection therewith. Consequently, we find that, as the first stage of labour advances, the upper part of the vagina is dilated until it seems to coincide pretty closely with the upper part of the bony canal. When, therefore, a considerable portion of the lower segment of the uterus can be felt in the vagina, and not merely through its walls, expansion is certain to be complete, whatever may be the size of the parturient ring; and the tissues composing it are those of the cervix proper, and not the uterus. Under such circumstances, I believe the membranes may be ruptured with advantage. It is, however, unnecessary in many cases to wait for the full development of the condition above described. I have taken the extreme state as being most readily under-

stood, and indicating the direction in which our observations should be made.

Another class of cases, or it may be only an additional character to those of the first, are where the action of the uterus seems to be effecting, not steady dilatation, but extreme thinning of the tissue of the cervix; and also where the head is felt to be in close contact with the parturient ring, there being little or no bag of waters.

The next point to be considered is the quantity of liquor amnii; not the actual quantity, as is generally referred to when speaking of it being present in excess, but the proportion its amount bears to the size of the child, and also to the capacity of the amniotic sac. This latter is rarely quite filled; otherwise, it would remain much more tense than it usually does in the intervals between the pains. If it be nearly or entirely distended, it will interfere with the power of restitution of form, by preventing alteration in the form of the uterus, and consequent action on the foetus, even though the actual quantity of waters is not greater than ordinary. In this circumstance, it must be regarded as really in excess, quite as much as where there is excess in actual quantity. Undue tension, therefore, of the membranes during a relaxed state of the uterus must be regarded as unfavourable to the mechanism of labour, and as warranting an earlier rupture of the membranes than under other circumstances.

The liquor amnii must also be considered in excess, irrespectively of actual quantity, if it be unduly great in proportion to the size of the child. Here, again, it interferes with the action of the force which restores form, or the axial force. If, therefore, the parts of the child be not recognisable externally with ordinary facility during a relaxed state of the uterus; if *ballotement* be unusually facile, and especially can be felt during a pain, the probability is that there is a true excess of liquor amnii; and this condition would fully warrant the rupture of the membranes before the full dilatation of the os; the other conditions being favourable to the operation.

I have discussed the subject apart from the state of rigidity or dilatation of the cervix, conditions which undoubtedly must be taken into consideration in determining any line of treatment in the first stage; but the subject of rigidity is one which requires discussion by itself, and would only tend to complicate and obscure the question.

ON THE TREATMENT OF ACUTE RHEUMATISM.*

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NEXT, what evidence have we that salicin and salicylic acid have any specific action in acute rheumatism? I take it that a specific remedy should—1. Cut short the disease; 2. Prevent relapses while under its influence; 3. Ward off complications.

1. *Influence in Cutting Short the Disease.*—We have already seen that, in a certain number of cases, these remedies have failed—salicin in 19.4 per cent., salicylic acid in 20 per cent.—and this without respect to dose.

2. *Prevention of Relapse.*—Under salicylic acid, relapse is mentioned in four cases; but these are not conclusive, as the acid had been previously stopped. My friend Dr. Shingleton Smith, however, tells me that a patient of his had a relapse while taking the soda-salt. A case of my own gives very definite evidence of the occurrence of relapse under salicin. A young man was admitted into the Bristol General Hospital, suffering from a well marked first attack of rheumatic fever. Under twenty-grain doses of salicin every hour, the temperature was normal in two days; the dose was then gradually reduced to twenty grains every three hours. The temperature remained normal for six days; but then, while the patient was still taking twenty grains every three hours (the largest dose that Dr. MacLagan gave in any of his first series of cases), there was a relapse. This lasted five days, though the dose was increased to twenty grains every two hours, and, for one day, to twenty grains every hour. For nine days, the temperature was normal, and the patient went out, still taking fifteen grains three times a day; he then was readmitted in another relapse.

3. *Prevention of Complications.*—The following have occurred:—*a.* Heart-complications:—Under salicin, endocarditis came on in three cases, pericarditis in four; under salicylic acid, endocarditis developed in one case. *b.* Lung-complications:—Under salicin, bronchitis and oedema, pneumonia, pleurisy; under salicylic acid, bronchitis, collapse, pneumonia. *c.* Hyperpyrexia:—These remedies have already con-

* Concluded from page 106 of last number.

tributed quite their proportion of those formidable cases of rheumatic fever in which there sets in a rapid rise of temperature, ending, if unsubdued, in death: salicin, four cases; salicylic acid, four cases.

These various complications have occurred without any apparent reference to dose; they have come on under small doses; they have appeared under large. Hence we see that salicin and salicylic acid have failed to cut short the disease; have failed to prevent relapses; have failed to avert complications; and, therefore, I consider that we have conclusive evidence that they are not specifics. To what, then, is their power due? They probably act simply as antipyretics. They have a very marked effect in other febrile conditions. In one hundred and sixty-four cases of typhoid fever, Reiss found that the reduction of temperature by the acid was very marked, and the duration of the disease was shortened, the average being only 13.1 days. If these results be confirmed, typhoid fever is cut short by one-half, and a specific action may just as fairly be claimed for the drug in this disease. In pneumonia, the effect of a large dose was very great, the temperature falling nine degrees or more—from 104 deg. to 95 deg., or even lower. As to the removal of the joint-affection by an antipyretic, we have a parallel in the disappearance of the pain from the joints that often follows the use of the most powerful antipyretic we possess—the cold bath. Indeed, Esmarch, several years ago, treated acute rheumatism by keeping ice constantly applied to the joints, with the result of lowering the temperature, easing the pain, and shortening the disease in a more marked way than by any other treatment.

There are certain *disadvantages* connected with the use of salicin and salicylic acid: the first is, that the dose is very large and very frequent; the next is that uncomfortable and even dangerous effects are produced in some cases; these I have no time to enumerate. Salicin has been said to be free from these ill-effects, but this is contrary to my own experience. I have found it cause deafness, noises in the ears, headache, extreme giddiness, repeated nausea and vomiting; several times so severe that the powders had to be stopped.

B. Treatment of Complications, with Special Reference to Hyperpyrexia.—I now proceed to the second division of the subject—the treatment of complications. The preventive effect of various remedies I have already discussed, and have concluded that no treatment will ensure the patient against the occurrence of any given complication. We may assume, however, that any remedy which shortens the disease will in the same proportion lessen the liability to complications of every kind; and, if our future experience accord with the past, we are justified in concluding that the chance of complications is least under salicin and salicylic acid, because the duration of rheumatic fever has been shorter under these remedies than it has been under any other plan of treatment.

I will not detain you by a detailed description of the treatment of the various affections of the heart, lungs, and other parts, which may occur in rheumatic fever, and which must be managed on the principles that guide their treatment in other diseases. The one complication to which I wish to direct attention is Hyperpyrexia.

It is well known that, in acute rheumatism, as in other diseases, there may set in a rapid rise of temperature to a height incompatible with life, accompanied by evidence of profound nervous disturbance, and usually, in rheumatic fever, by disappearance of pain from the joints. The temperature may reach 110 deg., 111 deg., or nearly 112 deg. This has happened under all kinds of treatment: under alkalies, iron, quinine, bleeding, and, lastly, under salicin, salicylic acid, and salicylate of soda. Whatever view is taken of the pathology of this condition—a subject which cannot be discussed here—it has been found practically that treatment directed simply to the reduction of the temperature may save the patient's life. You are aware that a temperature above 106.5 deg. in rheumatic fever was formerly invariably fatal, and that it is to Dr. Wilson Fox that we owe the treatment by the cold bath that has succeeded where every other method has failed.

In the case which I am about very briefly to relate, salicin failed to control the disease. It is true that the dose—ten grains every three hours—was small; but the same dose has been given by others with reputed marked effect; and, in other cases in which hyperpyrexia has occurred, this objection cannot be urged.

The patient was a labourer aged 36; he was admitted into the Bristol General Hospital on May 24th, 1876, with a well marked first attack of rheumatic fever. For five days, he took salicin without improvement. On the evening of the sixth, Mr. Harsant, the house-surgeon, found him delirious, with a temperature of 108 deg.; he at once put him into a cold bath; but, as the man was apparently dying, took him out in fifteen minutes, when the axillary temperature was 104 deg. Ten minutes later, the temperature had fallen to 102 deg.

Within two hours, it was 105 deg., and the patient was again in violent delirium.

At 10.30 P.M., I arrived at the hospital, and found the patient delirious and violently moving the affected joints. The surface of the body was very dusky, dry, and pungently hot; the face dark and the eyes staring; the pulse very small and rapid, just perceptible at the wrist; the axillary temperature 108.6 deg. From this time, the temperature was taken in the rectum. I had the patient again put into the bath. In ten minutes, the temperature was 109.5 deg.; that was the highest point. The following table shows the effect of the bath.

Time.	Bath temp.	Temp. of patient.	Pulse.
10.40 P.M.	.. 88 deg.	.. 108.6 deg. (ax.)	.. ?
10.50 "	.. 70 "	.. 109.5 " (rect.)	.. ?
10.55 "	.. 70 "	.. 109 "	.. ?
11.5 "	.. 65 "	.. 106 "	.. 156
11.10 "	.. 64 "	.. 106 "	.. 156
11.15 "	.. 62 "	.. 103.6 "	.. 156
11.18 "	.. 63 "	.. 102 "	.. 148

Time in bath, thirty-eight minutes; the temperature fell from 109.5 deg. to 102 deg., or 7.5 deg. in twenty-eight minutes, and, after removal from the bath, fell four degrees in ten minutes, thus coming down to 98 deg.; giving a total fall from 109.5 deg. to 98 deg., or 11.5 deg. in forty minutes.

During the whole time that the patient was in the bath, his aspect was that of a man who might die at any moment; he was pulseless at the wrist, and the heart was beating 156 in the minute; most of the while he was struggling more or less violently, at times quiet and almost comatose; the surface was intensely dusky throughout. Once the heart failed, and the patient seemed in the act of dying; but stimulants were applied, and the heart quickly recovered its rapid action. Towards the end, the patient struggled less, and became rather more observant. Brandy and ammonia were given freely during the bath. In twenty minutes after removal to bed, the patient was almost rational. When I left at 1 A.M., the temperature was 99.5 deg., the pulse 104, the skin very much less dusky, and the patient perfectly quiet. From this time, there was steady, though slow, progress. Although the temperature several times rose to 103 deg., no attempt was made to control it by means of ice-bags or other cooling appliances; and the result justified the treatment, as there was no elevation of temperature above 103.2 deg. There was no return of the acute joint-affection; merely slight occasional pains, chiefly muscular and tendinous. No quinine was given, nor any medicine, for nearly three weeks.

At what point ought the cold bath to be used? Dr. Wilson Fox (*Treatment of Hyperpyrexia*, p. 27) considers "107 deg. as the extreme limit to which the temperature should be allowed to rise before the external employment of cold is commenced". Death has occurred, however, before this height has been reached—at 105.8 deg. and 106 deg. Spontaneous recovery has never been known to occur when the temperature has passed 106.5 deg.; at this point, therefore, we may begin active treatment. It is important to remember that the bath should be prepared and everything got ready before this; for the final rise of temperature is often very rapid, and the patient may die before the treatment can be applied. In my case, the temperature went up 5.5 deg. in an hour and a quarter—from 104 deg. to 109.5 deg.

There is another practical point to be borne in mind: that there is almost always a considerable fall of temperature after the patient is removed from the bath, so that it is not safe to bring the temperature down to normal in the bath. I remember, in the early days of this treatment, how a friend of mine reduced the temperature of a rheumatic patient to normal in the bath, and, to his consternation, found it would not stop there, but kept going down till it reached 93.2 deg.; there it was fortunately brought to a stand. The amount of this subsequent fall varies, but the average may probably be safely taken as four degrees. In accordance with this, I removed the patient from the bath when the temperature was 102 deg., and the subsequent fall, occurring in the next ten minutes, was exactly four degrees, bringing the temperature down to 98 deg.

If it be our misfortune to meet with a case of hyperpyrexia in acute rheumatism, we have two facts to remember and to act upon—and I am induced to insist upon this the more strongly because I have lately met with attempts to disparage the treatment—1. That the patient *must* die if left alone; 2. That the *only* treatment which has succeeded is the effectual external application of cold.

In concluding my brief survey of what has been done in the treatment of rheumatic fever, I quote Sir Thomas Watson:—"You may be sure, when men's opinions concerning the treatment of a disease which is of common occurrence and easy recognition are thus unsettled and diverse—1. That no specific for that disease has yet been found; 2. That the disease is not very obedient, or not steadily obedient, to any remedial plan."

I sum up in the following propositions.

1. We have no specific for rheumatic fever.
2. Salicin and salicylic acid appear to have had more influence over the disease than any other remedies, and they therefore deserve an extended trial.
3. These drugs lessen the chance of complications in the same proportion as they shorten the disease.
4. The value of statistics of limited numbers in acute rheumatism is small, owing to the uncertainty of the disease.

[Since the above paper was read, a wider experience of the effects of salicin and salicylic acid has been in accordance with the views I have expressed as to the mode of action of these remedies—that it is simply antipyretic. In cases of typhoid fever, I have met with as marked an effect on the temperature as in rheumatic fever, but without evidence of any specific action on the disease. To quote one example: in the case of a boy about twelve years old, 20-grain doses of salicin every hour for six hours reduced the temperature in twelve hours from 104.2 to 97 deg.; and a similar effect was produced on other occasions, yet the duration of the disease was apparently not influenced.—In comparing the effects of these drugs in acute rheumatism and in typhoid fever, it is important to remember that; while in rheumatic fever the characteristic lesions are peculiarly erratic and fugacious, in typhoid fever there is a definite local lesion that must run a more or less protracted course.]

CASE OF CÆSAREAN SECTION POST MORTEM: TRANSPPOSITION OF VISCERA.*

By EDWARD BUCKELL, M.R.C.S. Eng., Winchester.

ON June 14th, at 11.45 A.M., I was hastily summoned to attend upon E. G., a single woman, twenty-seven years of age, who had been taken suddenly ill in the public street, and was removed into an adjoining tradesman's shop. On arriving within ten minutes from the time of receiving the message, I was informed that she had been removed in a carriage to her friend's house, whither I at once proceeded. In the meantime, she had been conveyed into the house and placed on a bed on the first floor. On entering the apartment, I found her lying supine, with limbs extended, face, arms, and hands livid, frothy mucus running from the mouth and nostrils; her eyes staring and fixed, pupils dilated, and with no trace of pulsation or respiratory movement discernible; in a word, to all appearances she was quite dead. The person who carried her from the vehicle to the house along the garden-path believes she actually expired before she reached the house, she having given a deep convulsive sigh when midway and shown no sign or movement afterwards. However, I at once endeavoured, by recognised modes, to excite respiratory movements, applying as well sponges dipped in boiling water to the region of the heart. Having spent ten minutes in fruitless endeavours at resuscitation, and finding that the poor woman had been expecting her accouchement very shortly, I examined *per vaginam*; discovering the os uteri to be dilated to the size of a florin, the head presenting, and the membranes, which were entire, easily felt. The vagina was moist with free mucous discharge, but there had been no hæmorrhage. It was clear, therefore, that, during her morning's walk, her labour had commenced.

On applying the stethoscope to the abdomen, the pulsations of the foetal heart were clearly heard. I conferred, therefore, immediately with the friends on the advisability of performing the Cæsarean section, with the view of saving the life of the child. After some delay, it was agreed to leave the matter in my hands. In the meantime, the foetal circulation had lost much of its power and regularity; still it was readily made out. As nearly as I could reckon, the mother had now been dead twenty or thirty minutes when I commenced the delivery by making an incision with a pocket-bistoury from the umbilicus to the pubes, through the abdominal walls, exposing at once the globular surface of the uterus, which was then cut through. The placenta being found attached to this portion of the uterine wall, I passed my hand through it and seized the body of the child, removing it without trouble at once. This operation occupied about three minutes. The pulsation in the cord was scarcely perceptible, and the heart itself beat very feebly. Without delay, I commenced, at about 12.15, endeavours to excite respiratory movements by means of direct inspiration alternated with Hall's and Sylvester's modes. At the expiry of rather more than an hour—for it was 1.30 P.M. before I left the house—I had managed to induce pretty regular breathing and pulsation in the child. I may remark that considerable advantage was derived from the alter-

nate use of hot and cold sponges applied to the chest of the infant. At this date, the child is still living, and has perfectly free action of both heart and lungs. It has also made satisfactory growth, and bids fair to live.

In the *Transactions of the Obstetrical Society of London*, ten cases of the ordinary Cæsarean section are recorded, with two recoveries of the mother; of six living children removed three survived the month, and may be living now. The child in this present case commenced life some time after her mother had ceased to breathe. Instances are rare in this country of the *post mortem* performance of the Cæsarean section; the circumstances under which it is likely to be successful rarely occur. Many diseases fatal to the mother are so to the child. Death from causes not directly affecting the child's life is generally sudden, and the doctor is rarely summoned in time to attempt the rescue, though he should consider this his first duty after finding that the mother has irretrievably gone.

In Cazeaux's *Midwifery* (8th edition, Paris, 1870, p. 1,070), five instances are given of children removed alive from the uterus from ten to thirty minutes after the death of the mother, and it is there said that "after half an hour success becomes very rare". From other less accurately determined accounts, it would not appear impossible that intrauterine life may be maintained under some circumstances for two hours or more after the death of the mother.

POST MORTEM EXAMINATION.—Eight hours after death, I made a *post mortem* examination of the body. The woman, who had been a domestic servant, was of middle height, fair complexion, fairly proportioned, and well nourished. The mammary glands were fully developed and rather hard. On the right side there was a second smaller nipple about one inch and a half below the normal one, but it did not seem supported by any corresponding glandular structure. On removing the sternum and portions of the ribs in the usual way, the heart was discovered to the right side in place of the left. The liver was transposed, occupying the left hypochondrium, the right lobe becoming the left and the left the right. The stomach was found on the right side, with its natural relation reversed, *i.e.*, the cardiac end being to the right of the pyloric. The spleen was placed on the right side instead of the left. The thoracic aorta was on the right side of the spine, with the œsophagus in front and to the left. The small intestines terminated on the left side, the colon ascending from the left and terminating on the right, the sigmoid flexure turning towards the rectum from right to left. Of course, the ilio-cæcal valve and vermiform appendix were in the left side. The time allowed for the autopsy was so limited as not to admit of more minute and careful investigation; but I forwarded the heart and lungs to Dr. J. C. Ewart, the curator to University College Museum, from whom I have since received the subjoined notes. I will only add that there existed a considerable thinning and dilatation of the convex wall of the aorta, rendering the semilunar valves incompetent, and hence probably the sudden death.

There existed considerable pleuritic adhesions, more particularly on the left side, those on the right being more recent, and not by any means so extensive. During life, the woman is reported as having complained of shortness of breath occurring in paroxysms, often preventing her from lying down in bed, more especially so on first retiring; otherwise she had enjoyed good health. Her mother had, it appears, also died suddenly from hypertrophy of the heart, it having been found enlarged on *post mortem* examination, but not displaced.

Dr. Ewart writes:—"The heart is so transposed that the left ventricle is at the right side and in front, the right ventricle at the left side and behind. The right auricle, larger than usual, is at the left side and in front, its auricular appendage overlapping the root of the aorta. The left auricle is at the right side and behind; the pulmonary artery passes from the left side across and in front of the root of the aorta, and then, lying parallel to the ascending part of the aorta, passes to the lungs. The aorta springs from the posterior part of the left ventricle under and at the left side of the pulmonary artery, and, bending first to the left and then to the right, passes backwards and downwards to disappear behind the root of the right lung. The vena cava inferior enters the posterior part of the right auricle in the usual way; but there are two *superior venæ cavae*, the normal right one entering the auricle at the left side, and the other arching round the right side of the left auricle to pass into the right auricle posteriorly. All the valves of the heart are normal in number and position, with the exception of the aortic semilunar valves, which are rough and very much thickened, and apparently only two in number. The right ventricle seems normal, but the left ventricle is dilated and its walls are considerably hypertrophied. This is perhaps the result of the abnormal condition of the aortic valves. At first it might appear that we have been describing a condition of things similar to that mentioned by Mr. Quain in his work on

* Read before the Southern Branch.