Case of spontaneous expulsion of child : with remarks / by Robert Dyce.

CASE

OF

SPONTANEOUS EXPULSION OF CHILD.

WITH REMARKS.

BY

ROBERT DYCE, M.D.,

LECTURER ON MIDWIFERY, MARISCHAL COLLEGE AND UNIVERSITY, ABERDEEN.

EDINBURGH:
SUTHERLAND AND KNOX, GEORGE STREET.

MDCCCL.

[FROM THE MONTHLY JOURNAL OF MEDICAL SCIENCE, FOR MAY 1850.]

FIG: 1.

FIG: 2.

FIG: 3.

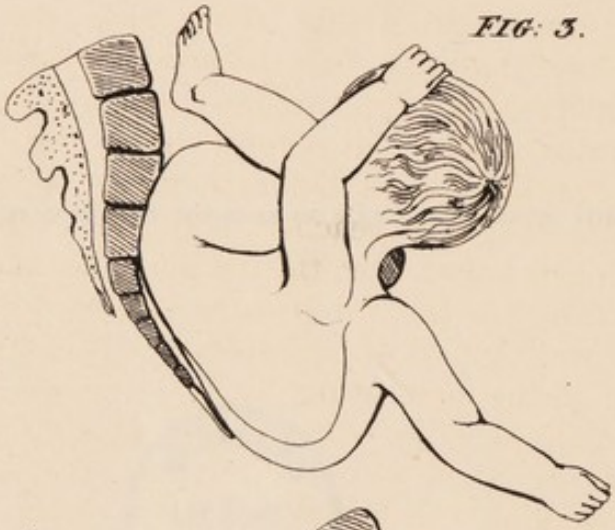

FIG: 4.

FIG: 5.

Illustrates the progress of Spontaneous Expulsion of the Child.

CASE OF SPONTANEOUS EXPULSION OF CHILD.

(*Read at the Medico-Chirurgical Society of Aberdeen.*)

I was called about midnight on Saturday, 30th December 1848, by a midwife, to visit Mrs C——, the wife of a tradesman, living in Castle Street. I was informed that labour had commenced in the evening about six hours before,—that the presentation remained long high,—that the membranes ruptured naturally,—that the waters were in great quantity,—and that several strong pains had followed after the discharge of the waters, before any part of the child could be felt,—a limb was at length reached, which was made out by her to be an arm. When I first saw the patient she had very strong forcing pains, the arm was at the top of the vagina, doubled up, so as to present the elbow. A part of the child, nearly equal in bulk, was felt on either side of the presenting limb,—viz. one part near the pubes, and the other near the sacrum, but so high that, unless I had passed my whole hand into the vagina, which I did not at the time deem essential, the individual parts could not be made out. It was sufficient for my purpose that the arm presented, and that delivery could not be accomplished without turning the child. In order, therefore, to render the operation easier, by quieting the uterine action, which was very powerful, I gave her, as soon as it could be procured, a tea spoonful of laudanum, determining to operate the moment a lull took place. This, however, never happened, for presently the pains forced the elbow lower, the hand came down into the vagina with hardly any assistance, and was ascertained now to be the right one. At this time the proportionate size or bulk of the two parts of the child became remarkably altered. The arm, shoulder, and neck, which formed one part, pressed towards the pubes, and appeared smaller; while the other end of the tumour, which was now distinctly made out to be the back of the child, along with the ribs and spine, which was twisted and bent, now came completely to occupy the hollow of the sacrum. It now became very apparent that nature was to complete the delivery herself, by expelling the child double, or by what is called spontaneous expulsion. At length, after two or three powerful pains, the shoulder was very

closely pressed, or jammed rather, against the arch of the pubes, and at length external to the vulva, while the breech pressed out the perinæum, and was expelled by a very long and powerfully-continued pain, the feet following quickly in its wake, the arm never moving from its position under the pubes. The head soon followed, and the delivery was speedily completed. The child (a girl) gasped once or twice, but could not be recovered.

The size of the abdomen indicated the presence of another child, which, on examination *per vaginam*, was confirmed, as the membranes were reached. I also discovered in the examination a circumstance by no means desirable,—viz. that the integuments of the abdomen, limbs, face, and in short the whole body, were extensively œdematous, for lying, as the patient did, with her back towards me, I had no opportunity of discovering this before.

No pain coming on in half-an-hour, the membranes were ruptured. The waters were in very great quantity. An arm again presented, but along with the head. It was attempted to keep up the limb, so as to let the head descend alone; but the pains were so violent, that both head and arm were forced into the pelvis, and expelled together. The breech remained during two or three equally severe pains at the birth, owing to the cord, which was very short, being twisted round one thigh and leg, by which it was tucked up tight upon the infant's abdomen; its removal immediately caused the expulsion of a second girl, alive and strong. In a quarter of an hour pains returned, but no part of the placenta could be felt; and as the uterus felt contracted, small, and tolerably defined, while no hemorrhage at first ensued, no interference was resorted to. Very shortly, however, the pains became very severe, along with hemorrhage to some extent, so as to lead me to fear that irregular contraction either had taken place, or was impending, and that probably one of the placentæ might be separated. The hand was immediately introduced. One placenta was found loose in the lower part of the uterus, a portion of the other in the contracted part, while by far the largest portion was imprisoned above, in the upper chamber of the uterus. The usual methods were carefully tried to separate the placenta,—viz. by patting it, by grasping it from its edge to its centre, while the uterus was steadied by the hand on the abdomen externally,—but no impression was made upon it. It was then attempted to remove it bit by bit, but so firmly was it attached, and to so very large a surface, that I for a moment hesitated what was best to be done,—whether to persevere carefully in my present proceeding, or to leave some of the lobules adhering to the uterus. Both methods were attended with danger; but knowing well the great risk there is in separating a strongly and morbidly-adherent placenta, from the difficulty in distinguishing the soft and loose structure of the womb from the mass of the placenta, I decided on the latter method. I therefore kept my fingers close to the placenta, and pinched off several of the lobules, and left them adhering to the uterus. The womb now contracted regularly, and expelled my

hand and the placenta together. Hemorrhage ceased, and the pulse, when I left at two in the morning, was 86. The woman had a most perfect recovery.

Here is a case of a very unusual description;—one presenting a form of labour of extreme rarity, while it at the same time is accompanied with so many untoward circumstances, as well nigh to embarrass even an experienced hand. It will be remarked that it was a *twin* case, in which an *arm* of each child was the presenting part; that the patient was affected with *general* as well as *special* dropsy (of the amnion); that there was *hour-glass* contraction of the womb; extensive *morbid adhesions* of the placenta; and lastly, *hemorrhage* to some extent. It is principally to the form of labour, and the mode of treating the placenta, under the state here observed, that I wish to direct the attention of the members of the Society.

That the labour is one of extreme rarity, I may appeal to the experience of those present, as well as to the works of authors on the subject of midwifery. None here, I believe, have ever met with a similar case. Davies says he never met with a case, nor did Merriman; Ramsbotham, in a foot note, mentions "being present at four cases during the expulsion of the fœtus from the outlet;" Burns informs us that in the city of Glasgow, with a population of 200,000 inhabitants, "I cannot learn that more than one case of spontaneous evolution has taken place," his remarks are hence not made from personal experience; Gooch saw but one case; Collins says, "no instance of it occurred in the hospital during my assistant or mastership;" and but one doubtful case during the residence of his predecessor, Dr Clark; "Chailly had but twice occasion to observe the mechanism of spontaneous evolution;" and Dr Douglas, to show its rarity, calculates that this form of labour does not occur oftener than once in 10,000 times. Denman has collected thirty cases, scattered over the practice of a number of men, but it does not appear that he himself met with more than one, or at most, two cases. I might extend this inquiry, but these references from the works of men of eminence and extensive experience, will be sufficient to prove that it is a *rare* form of labour.

This rarity no doubt was the reason why the true mechanism of this form of labour remained so long unknown. Denman, who was the first English physician to call the attention of the profession to it, and who gave it the name of *Spontaneous Evolution*, did not, it would seem, arrive at its true mechanism; yet his explanation was the commonly received one from the year 1772 until 1811, a period of about forty years, when Dr Douglas of Dublin made known what he conceived to be its real nature, in a short pamphlet upon the subject. Previous to this time, it was believed that the arm which presented receded into the uterus, that the child turned on its own axis, and that the breech came down in its place, just as it does when turning artificially is accomplished; but this change Douglas combated, while, at the same time, he illustrated his views of the subject

by the publication of some cases, which prove that, instead of the arm returning, it became more and more protruded, until not only it but the shoulder was external to the pubes—that there it remained fixed—that the breech, by the continuance of the pains, came to occupy the hollow of the sacrum—that this was at length born—and that the arm which first was expelled never moved, and was actually the last to leave its position under the pubes.

Gooch followed some years after, in 1819, and, in his usual graphic style, described a case which confirmed the views of Douglas, and has since that time been received by the profession as the true mechanism of spontaneous expulsion. I need not transcribe his case, as we find it detailed in the "Med. Transactions of the College of Physicians," vol. vi., but I beg to recommend its perusal as being a most perfect account of the process of what is called spontaneous expulsion, and one which coincided in every particular with the case I have just read.

It has been mentioned by some authors, notwithstanding the evidence adduced by Douglas, Gooch, and others, that there may be some cases of this form of labour where the arm does actually recede as the breech comes down, hence that Denman was perfectly correct in the description he gave of the mechanism of his form of labour; and in corroboration of this position, a case is brought forward as related by Boer, where the hand which had appeared externally did distinctly recede on his attempting to introduce his hand for the purpose of turning, and the breech commenced its descent so as to occupy the hollow of the sacrum. Upon this case Rigby remarks: "That this is very different from a case of spontaneous expulsion,—the child had not yet begun to press against the brim, or to assume any definite position,[1]—it lay completely across the pelvis, and that, as the pains increased, the breech, being nearest the brim, descended, and the arm in consequence receded." Gooch, who also notices this case, believes it to have been only a breech case, the hand having accidentally slipt down into the vagina; because he says in every instance in which this process (spontaneous expulsion) has taken place, the arm has been protruded up to the shoulder; whereas in this, the fingers never descended lower than to become visible externally. Many years ago, while quartered at the Cape of Good Hope, I met with a case in many respects very similar to Boer's, and which at the time made a strong impression on my mind, as to the correctness of Denman's description. The patient was the wife of a soldier of the 75th regiment, in labour of her first child, and at or near the full time. The midwife in attendance, on finding it a case of difficulty, called me in. I learned that the patient had been many hours in labour, and that an arm presented. The arm I found in the vagina, and the hand appearing externally. I attempted to introduce my hand for the purpose of turning, but the increase of the already

[1] Query,—Is it possible for an arm to be appearing externally, and the child not pressing against the brim?

powerful pains, and consequent resistance, rendered this impossible. A large dose of laudanum (80 drops) was given, and farther attempts delayed for half-an-hour. At the end of this time, although the pains were only moderated, not subdued, I proceeded to the operation; but what was my surprise to find that the arm had disappeared, and that in its place the breech had come down, and was already close upon the perinæum, and was soon after, by very severe pains, expelled, with the legs doubled up as in an ordinary breech case, without the least assistance upon my part.

I readily concede that this is a very distinct case from the one heading this paper, and which has called forth these remarks. But is it not equally possible that there may be more than one form of spontaneous expulsion? That the case just narrated, with Boer's and others which might be mentioned, may be *one form* of spontaneous expulsion, or really "evolution," the same which Denman observed; and that the cases described by Douglas and Gooch may be another form of the labour, and, strictly speaking, "expulsion;" and that whether the one or the other form is to terminate the labour, in short, whether the arm is to recede into the uterus, or remain protruded until the completion of the labour, will depend on the following circumstances:—First, upon the size of the child; second, upon the position which the child has assumed at the brim; and thirdly, and especially, upon the state of uterine action.

With regard to the last of these causes, I fancy every man who has had much experience in midwifery practice will admit, that he has not unfrequently met with cases where, during some part of the labour, and more especially the latter stage, the pains have not all been equally effectual,—that even though there was the same amount of suffering, there was not an equal advance in the labour; nay, the patient herself will often tell you that such and such pains, though as strong and as ill to bear, are not doing her so much good —they are felt to be different. This, I believe, is arising from the uterus acting *unequally*,—that one part is acting more energetically than another, and thereby in a manner counteracting, or at least interfering with and deranging, the effect of the whole,—for to be effectual in forwarding labour, every part, both back and front, must equally do its duty. Now if this is a fact in respect of common labour, may it not equally operate in the form of labour now under consideration?

I find that in every recorded case of spontaneous expulsion, the action of the uterus is described as being impetuous. The whole organ is acting not only energetically but uniformly and equally,—the pressure upon every part of the child will thus be uniform;—hence the arm once down in the vagina will be kept from receding by the contractions of the anterior fibres;—nay more, by the impetuous uterine action will be forced even lower, while the more flexible parts, so to speak—the back and breast—are driven down to the back of the pelvis by the equally strong action of the posterior fibres. Thus, by

the combined and uniform action of the whole organ, Douglas' form of spontaneous expulsion is accomplished. But in the other form, or Denman's, I conceive that the uterus, although it may still be acting energetically, is still not acting equally; that while the posterior fibres are strongly contracting and forcibly pressing the breech into the back of the pelvis, the anterior fibres may be acting much less strongly,—consequently the resistance they offer is not only not sufficient to keep the arm down in the vagina, but actually allows itself to be overborne, and the arm to be pushed along with the shoulder and head beyond the brim, so that it recedes from its situation in the vagina, while the breech, by the continued strong action, occupies its place;—thus forming Denman's "spontaneous evolution."

But besides this unequal uterine action, I consider that the position which the child assumes at the brim has an important influence over the two forms of labour,—viz. upon the relative proportion which the body, on one side, and the neck and head on the other side of the presenting part, bears to the centre of the brim of the pelvis. If, for instance, in a case of arm-presentation, a greater proportion of the neck and head be perceivable on examination than there is of the body on the other side of the presenting arm, and the uterine action is strong and uniform, then one of two things will happen: if the child is at the full time and large, it will be jammed into the pelvis, and delivery will not be accomplished *but* by turning; while if the child be immature or small, then the arm will remain down, be protruded along with the shoulder externally, and the breech falling into the hollow of the sacrum, spontaneous expulsion, as at present understood, will be accomplished.

If, however, on the other hand, more of the body of the child is perceivable on the one side of the presenting arm than there is of neck and head on the other, the shoulder perhaps just resting on the brim, then I imagine the unequal action of the uterine fibres, favoured by this position of the child, will enable the arm to rise, and eventually to disappear, while the breech, with the legs doubled up, will descend and fill the pelvis, and form spontaneous evolution. I would still, therefore, recognise and retain Denman's name of spontaneous evolution for the cases in which the arm recedes; and Douglas' and Gooch's name of spontaneous expulsion for those in which the arm remains down.

It has been a question with some authors, seeing that occasionally an otherwise impracticable form of labour may be terminated by the natural powers alone, whether it is right to wait for this process.

Denman, amongst others, was very favourably inclined to this delay; but it is now an established rule in midwifery, that we are not warranted in waiting for it, because the delay must be extremely hazardous to the mother, and almost certainly fatal to the child.

There are, however, though rarely, cases occurring, and the one now read to the Society corroborates the observation, that "under certain concurring circumstances, a fair opportunity may be permitted

for the accomplishment of labour by the natural powers alone;" and it is important to note the combination which here existed, and which made its completion so probable. It will have been remarked, that the labour, from the moment I saw the patient, was almost impetuous, the intervals of ease were very short, the pains were very violent, and at the same time most efficient, and the woman was straining every nerve by her own exertions, and all this, too, after a large dose of T. opii. had been exhibited. There was thus everything in favour of the process going on; besides which, there were one or two other no less important points which materially aided,—viz. the immaturity of the child, as she had only completed her eighth month—the woman being well made—her former labours being quick and easy —and its being a twin case—of course this latter point could not be even suspected. It is a curious circumstance, however, that the greater number of cases in which this form of expulsion has taken place, have been twin cases. Thus every particular noticed by authors as conceived requisite to favour this process, existed in my case. There was a full-sized pelvis—an arm or shoulder presenting —an immature child, or twin case, which amounts to nearly the same thing—powerful and uniform action of the uterus, while the woman was bearing down with all her might—the parts disposed to relax freely, and well lubricated with mucus—and lastly, it was not a first child.

The next point I notice is the treatment of the placenta under a state of *extensive* morbid adhesion. It will be observed, that the ordinary methods recommended in this unfortunate state were each tried, but ineffectually; and that, after some consideration, I determined upon leaving a portion behind, rather than run the great hazard of injuring the uterus. It is to this point especially that I wish to direct your attention; and the question naturally arises, Which method will be attended with least danger—whether, by perseverance, we are to endeavour to remove every portion of the placenta? or whether, seeing that there is difficulty, we are cautiously to separate the adhering portions from the rest of the mass, and allow them to remain to be thrown off by an after process? My own opinion is, and I have formed it, not from the result of this case, but from the result of other and fatal cases, where an opposite treatment was pursued, that it is decidedly the best practice, if the placenta cannot be removed by moderate pinching, bit by bit, that the *adhering* portions be broken off from the placenta, and left attached to the uterus. I state this most unhesitatingly, as I know that an opposite practice—viz., that of attempting to remove every portion clean from the uterus—is, in a great majority of cases, attended with fatal results. Why this should be so appears to me pretty evident. We are apt to forget, in our anxious endeavours to relieve the patient (who may, perhaps, be flooding copiously at the time), the soft and spongy nature of the inner surface of the uterus. The placenta at its edge may be very undefined and thin—in short, it may

be insensibly lost in the walls of the uterus. The adhering portions are also most generally indurated, and we unconsciously—nay, unavoidably—peel off the soft portion which bound the morbid hard parts. Now this, in nine cases out of ten, is the uterus itself, injury to which, I say, cannot be avoided, and must hence be attended with very serious, if not with fatal, results. Indeed, wherever the adhesions are firm and extensive, it is almost impossible, if the uterus is to be preserved entire, to prevent our leaving some portion of the placenta behind. I would therefore recommend, in all cases similarly circumstanced, that it be made a rule of practice, not to be anxious to separate the adhering portions, but to remove so much as can be effected easily, and to leave the firm adhering portions to be thrown off or not, as may be; and my own experience warrants me in saying confidently, that this may be done with a far greater amount of safety to the patient, than if, by continuing our efforts, we succeed in bringing away the mass entire.

This, however, is a practice which by many is looked upon as extremely hazardous, and as placing the patient in imminent peril. But let us examine the danger. How will the case terminate, if a portion of placenta is left in the uterus? Firstly, the placenta may never be discovered; secondly, it may come away without any trouble; and thirdly, it may not come away until some time after, when it will be putrid, accompanied with a fetid discharge, and very probably with serious constitutional disturbance. The last of these three terminations, as bearing against our principles, is the most important, as there can be no doubt but that now and then portions of retained placenta contaminate the system as with a virulent poison, and place the patient's life in the greatest danger. But, although this untoward result is occasionally noticed, yet it is most encouraging to know that far more often the absorbing power of the uterus is quite equal to the removal of the retained portions, and that at the ordinary period the general health is restored, as well as the uterine function itself. On this point, Rigby, in his medical reports for the year 1834, has collected some remarkable cases. He says, after describing one case:—"The portion of placenta left adhering to the uterus was very considerable, being at least one-third of the whole mass—so large, indeed, that had I not been convinced, not only from my own experience, but also from the numerous facts recorded by Naegele, Salomon, and others, of the absorbent action of the uterus, I should have been induced to pass my hand again, and attempt the separation of this portion; but I considered myself justified, from knowing these facts, in leaving the case to nature." He goes on to state that there was no fœtor of the lochia, which were sparing; and it was ascertained, beyond all doubt, that no solid substance had come away.

Collins, in his "Practical Treatise on Midwifery," is very decided on this point, and says:—"When the after-birth is retained by morbid adhesions, it is recommended by most writers to remove as

much as can be effected by gentle means, leaving the remainder to be thrown off in the discharges. In this we concur."—And in the detail of cases under the head of "Retained Placenta," Nos. 60 and 100, are practical illustrations of the rule. He took away as much as he could, cautiously, and left the rest adhering, from an unwillingness to use violence. Both patients were discharged on the tenth and eighteenth day respectively.

Burns is no less explicit; he says:—"If the adhesion of any part of the placenta be very intimate, we must not, in order to destroy it, scrape and irritate the surface of the uterus, but ought rather to remove all that does not adhere intimately, leaving the rest to be separated by nature."

Here, then, are evidences of the safety of the practice, where no bad results followed, and yet where no vestige of the retained portion has ever appeared. But there are still more remarkable cases, where not only a portion, but the whole placenta, has been retained, and perfect recovery followed. The late Dr Young details a case of this description, which is thus related in a manuscript copy of his lectures which I possess:—"The woman had been two hours brought to bed, and all the different methods had been made use of to extract the placenta. The cord was broke. I put the woman on her side, and introduced my hand, but could not get hold of the placenta. I could get my hand up to it, but no further, the uterus having formed a sort of pouch for it; so that at last I was obliged to trust the matter entirely to nature, and, *what was very uncommon, no fetid stuff came away, nor anything like the placenta*, and yet the woman recovered, and continued in good health." He also notices, what must have occurred to others, as it has to myself, that frequently in cases of abortion, where the embryo has escaped, no membranes or placenta ever appeared, and, in fact, where no trace of these was ever afterwards seen—yet no bad consequences followed. The catamenia in due time returned, and the females became pregnant as usual. There are also cases reported where the placenta has been retained for many days in the uterus, and then expelled with little signs of putrefaction. Denman mentions one which did not pass off for fifteen days, and without any ill consequences. I might bring forward other cases of a similar nature; but these may be sufficient to show, that a large portion of placenta may be left behind with *perfect safety*. This being the case, it is, in my opinion, attended with much less risk to the mother, in cases of morbidly adhering placenta, to leave the portion attached, rather than, by striving to bring it away, to risk the infliction of a positive injury upon the uterus, which will almost certainly terminate in death.

MURRAY AND GIBB, PRINTERS, EDINBURGH.