

# Vaginal assistance of breech delivery

## Introduction

During October in 2001, Mary Hannah et al.,<sup>1</sup> published in *The Lancet* their prospective, multicenter and aleatorised trial whose target was the comparison in the outcomes of planned cesarean section (CS) or vaginal delivery in term pregnancies with the fetus in complete breech presentation.<sup>1</sup> In 121 centers of 26 countries, 2088 patients with a single pregnancy and the fetus in complete breech presentation were aleatorised to:

- i. Vaginal assisted delivery.
- ii. Scheduled CS.

The patients belonging the group “breech assistance” (BA) were assisted by an obstetrician defined as experienced in breech deliveries. The follow up of mothers and newborns was done during the next 6 week's. The main outcomes were:

- i. Perinatal mortality,
- ii. Neonatal mortality or severe neonatal morbidity,
- iii. Maternal mortality,
- iv. Severe maternal morbidity.

There were analyzed the information of 2083 patients. Between the 1041 patients assigned to CS, 941 (90.4%) underwent to this treatment. From the 1042 patients assigned to vaginal delivery, 591 (56.7%) underwent to vaginal delivery (VD). The perinatal mortality (PM), neonatal mortality (NM) and severe neonatal morbidity (SNM) were significantly lower in the group of CS (17 of 1039, 1.6%) than in the group of VD (52 of 1039, 5%) (RR 0.33 CI 95% 0.19-0.56;  $p < 0.0001$ ). There were no differences neither in maternal mortality (MM) nor severe maternal morbidity (SMM) (41 of 1041-3.9% vs 33 of 1042-3.2%; RR 1.24-CI 95% 0.79-1.95;  $p = 0.35$ ). The authors concluded that a scheduled CS is better than VD for the term fetus in complete breech presentation. The maternal complications were similar for both groups. However, there are some outcomes from this study that not allow a clear explanation.

## Perinatal risks

Firstly the authors analyzed the next outcome: the reduction in perinatal risk by a CS compared with a VD was too higher in countries with a low perinatal mortality rate than in countries with a high PM. Hannah et al.,<sup>1</sup> Hypothesized that a possible explanation is the bias in registration of neonatal morbidity in countries with high PM. It mean, in this countries is less probable the search of evidence of trauma at birth, seizures, hypotony or a “normal level of consciousness”. At the same time, the mothers are less proclives to inform the physicians about the problems that she can see in the newborn and most of the newborns, especially if they were delivered by VD, so, they were discharged before this problems might be manifested. Besides, the newborns delivered in countries with high PM, could die before some of this morbidities appear. However, the authors add, the reduction of benefit of a CS in this countries could be real, at some point due to a more skill of physicians in the assistance of breech VD. Is noteworthy how an randomized trial wich should give us clear outcomes, allow to

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produce this kind of speculations that become null the statistic power of the study, and are a new proof of the pride with this authors think how medicine is exerted in developing countries. In spite of, they recognize us a high skill to assist VD in breechs once they concluded that VD in breechs should not occur.<sup>2</sup> Not less important are the conclusions of this outcome: in the countries with high PM, it should be performed 39 CS to prevent one perinatal death or an affected newborn while in countries with low PM the number of CS needed to prevent harm is seven.

It should be mentioned that the probability of present maternal complications associated to CS are too higher in developing countries and is too high the probability that this women will develop complications associated with deliveries of the next newborns, because the probability to become pregnant is higher than in developed countries because of several reasons (social and economical level, contraception, cultural facts, etc). Respect to this problem, Shennan<sup>3</sup> said that there is a cost in immediate maternal morbidity when a CS is scheduled. There is no study that had considered the long term outcomes in this case. The future morbidity had not been evaluated beyond the present pregnancy and is definitely a problem in pregnancies with a uterine scar. The long term effects in the Childs are unknown too. In some developing places, the risks associated with a CS could be higher than a VD and 97% of newborns will not be affected by a VD in breechs. The costs needed to perform more CS may be significative and until impossible to be afforded for some societies. And is necessary to keep some skills to assist VD in breechs because some patients are going to refuse to be operated.

## Included and excluded patients in the study

Hannah et al.,<sup>1</sup> describe some patients were excluded because of fetopelvic disproportion diagnosis. Is really complicated to analyze this conclusion because while could be situations in wich clearly a fetopelvic disproportion is suspected, generally the proportion of pelvic part does not ensure the proportion of the cephalic part, may be this is the main question for the obstetrician in front of a breech presentation. While the authors support that the outcomes have no change excluding nuliparas, is weird the way to define a physician experienced in breech assistance. In most of countries, since several years ago (at least 20 years before the study) the breech nulipara undergo to CS. For example, among the Argentinian specialists that have participated in the study, is impossible to find experts in breechs

nuliparas because the simple reason that breech nuliparas go to CS since 1978 after the WHO statement. So, it should be assumed that in our country, except a very few physicians, there are not experts in breechs nuliparas assistance. This information is important because a half of patients presented an additional difficulty related with parity and is that they were assisted for operators with relative training to overcome this difficulty. Any obstetrician who exert the speciality, know very well that is not the same to assist a breech fetus in nuliparas than in patients with previous VD.

### Fetal weight

The calculation of fetal weight (FW) was done by clinical methods in 40.2% of patients in the group CS and 41% in VD. This is another critical point because the calculation of FW by ultrasound admit a variation of more or less 400g and of course, bigger should be the variation with clinical calculation. In 30.6% of patients in CS and 31.3% of patients scheduled to VD, the level of flexion or deflexion of the fetal head was estimated by clinical methods. This information is really amazing, the reader can not know if the authors were naive or they unknow the manouvers related with the right evaluation to be done when the obstetrician is going to assist a delivery in breech. We can say without be afraid to make a mistake that the lack of imagenological assistance in the calculation of deflexion of the fetal head in breechs is less than prehistoric and unacceptable equivocal. Is possible to think that this kind of bias does not have to do with the outcomes because it was similar for both groups, but is a simply analysis, because if it was a mistake in the FW calculation or the calculation of fetal head deflexión, it would have been only constituted a problem in the case of VD.

### Clinical amnionitis and fetal heart rate anomalies

Although is not a significative information, there were amnionitis in 3cases of CS and 11cases assigned to VD. It is an important disease because at the present time is one of the main causes of brain injury in the newborn.<sup>4</sup> It was not analyzed the outcomes of this cases in the study, but is possible suppose that in the 156cases assigned to VD with non reassuring fetal heart rates, could be included among this patients, it means that the non reassuring fetal heart rate had not to do with the fetal presentation. The incidence of no reassuring patterns of fetal heart rates happened in 13patients assigned to CS and in 156 assigned to VD ( $p < 0.0001$ ). When the perinatal outcomes are analyzed, to think that the mentioned before is a consequence or is attributable to breech presentation, imply not to rule out an evident confessional factor. Since the time that labor is present, there is no doubt that some cases are going to develop some pathological patterns in the fetal heart rate. These facts is not possible in the cases assigned to CS or not are in labor. In fact, between the patients assigned to VD, vaginal delivery was achieved in 591cases, but there are to add 376cases that ended in CS after to start labor. So, the non reassuring fetal heart rate patterns are only attributable to labor and not to fetal presentation. If we not consider that in this way and we assign patients with any kind of fetal presentation to VD or CS, obviously that the group assigned to labor will develop non reassuring fetal heart rate patterns, just because the presence of uterine contractions of intensity enough to become evident some pathological fetal status. The main subject is how the authors discriminated the fetuses with heart rate anomalies during labor from the fetuses that no at the time to analyze perinatal complications. However, the common sense suggest that is evident

that the fetuses with fetal heart anomalous patterns should have had an more torpid evolution than the fetuses without this patterns and this is not precisely attributable to a breech presentation, so if in this group there were affected fetuses, they should be excluded from the analysis.

### Labor induction and forceps utilization

In the study, the labor was induced in 4cases scheduled to CS and 83 cases scheduled to VD. Only those who exert obstetrics in active way and no behind the desk, know that induction of labor in breech presentation is little more than a imprudent decisión. Another time, nobody knows how this fact was taken in account when the outcomes were evaluated. There was 21cases assigned to CS that ended in VD assisted by forceps, and this situation happened in 123cases in the group assigned to VD. Once again, we are in front of another serious case of confusion by the time to analyze the outcomes. The indication to apply forceps in delivery assistance in breech presentation may occur in two situations:

- i. To solve the fetal pelvic distocia. In the practice, is neither wise nor prudent to continue with VD in front of such difficulty. Only should be justified in the case of absolute impossibility to perform a CS. Nobody into the sane can insist in continue with VD assistance under this circumstances in the ambit of a randomized controled trial. It seems logical that a fetal pelvic breech distocia in labor be considered an exclusion factor from the trial.
- ii. The second is in front of the situation of fetal head retention. This is an unpredictable fact. The skilled obstetrician should be prepared to solve it fastly and rightly by forceps application.

Any of both situations are serious bias in the perinatal outcomes consideration. If we are in front of 144 VD by forceps application, first is necessary to discriminate this cases by the time of evaluate outcomes, and second, reaffirming previous concepts, the operators were not so expert as expected, because a right evaluation of the cases and associated situations, not should result in such amount of cases needing forceps application to end breech delivery of the fetus.

### Neonatal morbidity and mortality

To analyze the perinatal outcomes of the study, we are going to take as a reference the opinión of Jos van Roosmalen in his article.<sup>5</sup> There were 13 perinatal deaths in the VD group:

- i. One was a fetus died before the incorporation of a twin pregnancy to the study (the trial just included single pregnancies). This death is clearly not associated with the delivery mode.
- ii. The second death was intrauterine and happened in a 3650g fetus that was in cephalic. This case neither is related with delivery mode.
- iii. The third was a late neonatal death in a newborn who weighted 2000g (low birth weight, neither was taken in account the contribution of the low weight in the death). The newborn was discharged in good condition and suffered an unexpected death. There is no reason to attribute this death to a delivery mode.
- iv. The fourth death was a neonatal death as well in a newborn who weighed 2500g and was discharged in good health and died after present a gastric and intestinal symptoms. Once again nothing to do with delivery way.

- v. The deaths five and six also were neonatal in newborns who weighed 2500 and 2700g because of respiratory diseases without any mention about a difficulties during delivery.
- vi. The death seventh, eighth and ninth, all presented fetal heart alterations. In two cases, the fetal heart rate disappear before to perform a CS. In the third case, it seems to be a delay in the performance of a CS because a fetal acute suffering. The fetal heart alterations are attributable to labor and not to fetal presentation.
- vii. Finally, this outcomes leave us only 4 deaths attributable to difficult delivery. So, only 4 deaths and not 13 are the deaths attributable to a VD of a fetus in breech presentation.

In the group that underwent to CS, there were 14cases of neonatal morbidity and 39 in the group of fetuses delivered by VD. But, the pediatricians who evaluated morbidity known the way of delivery, so the study was not double blinded introducing the observator bias. For example, the neonatal hypotony was one of the morbidity factors. It happened twice in the cases of CS and 18cases in the VD group. The hypotony disappear two hours after delivery in 7 out 18cases and also y debatable the meaning of hypotony in de 11cases left. Another factor of neonatal morbidity was “the abnormal level of consciousness” that included: hyper alert, dizziness and lethargic. So, related to neonatal morbidity, the information is still less convincent and generate more doubts than answers.

### Roosmalen ask

Is possible to defy the outcomes of an aleatorized trial? The author answer is: yes. The reasons are:

- i. The outcomes could have been casual.
- ii. The outcomes could be subjectives and under the observator bias (neonatal morbidity) and other outcomes could have been not included (long term maternal and neonatal morbidity ).
- iii. In a multicenter study, are included a variety of patients with different risks, in this case neither evaluated nor considered.
- iv. Besides, it was analyzed that the study present problems with the definition used in qualify an obstetrician experienced in VD assistance in breech delivery. One requisite to be considered with experience enough was to finished the residency, wich one no way is enough to be considered with experience enough from a practical point of view to exert with capability all the obstetrical maneuvers with a huge complexity.

### Level of complexity of assistencial centers

Another confusional factor was that only 35.2% of patients assigned to CS and 35.4% assigned to VD were assisted in high complexity centers. Two thirds of patients of each group were assisted in centers in which ones does not have the minimal conditions to assist an obstetrical emergency. Once again, is not possible to think that these proportions became the sample equivalent. It means: two thirds of patients whose delivery was vaginal were assisted in centers where:

- i. It was not posible to perform an emergency CS, nobody was on duty to assist a depressed newborn and there was not machines to ventilatory assistance for more than 24hours.
- ii. Probably, if this information had been communicated to the patients before to sign the consent, is expectable that much of

patients would have not signed it knowing the risks under the trial would be done and the unethical conditions to offer this assistance.

When Hannah said that after a follow up of two years of the newborns under this protocol, the risk of adverse perinatal outcome was lower in the group delivered by CS before labor, she should mentioned the conditions previously alluded.

### What to do in developing countries?

In her comment in *The Lancet*,<sup>2</sup> Dr. Lumley said that the findings in the Dr. Hannah trial are difficult to be applied in developing countries in coincidence with our affirmation at the begining of the manuscript. The physicians in this countries seems to be more experienced about the manouvers related with breech assistance because the benefits of a CS were lower. If the subject of obstetrical skills is true, then the findings of the trial are not applicable to developing countries. In this countries, the serious neonatal morbidity would not been detected totally or this morbidity finally was a neonatal death. If it is true, also there have doubts about the applicability of this policies in developing countries. The number of patients needed to be operated to avoid one death was 14 in developed countries and 39 in developing countries.

After this analysis, is there some option for the breech assistance?

The proposal of Dr. Lumley and Shennan<sup>3</sup> is:

- i. The patient should be informed about the outcomes of the Dr. Hannah trial.
- ii. Also, the patient is informed about the criticism that the study deserve.
- iii. After an evaluation of obstetrical situation (level of flexion of the fetal head, fetal size and estimated fetal weight) the patient should be counceled to undergo to a CS or to wait the spontaneous start of labor in the case of think that the possibility of need an emergency CS is low.
- iv. If the patient does not wish a trial of VD, the CS should be performed.

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### Conflict of interest

The author declares no conflict of interest.

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