



BJOG Exchange

the author comments that ligation or occlusion of the iliac internal artery does not have a firm recommendation to reduce blood loss, so it is not clear why he used it. Additionally, he interrupted the blood supply in the cervico-vaginal junction, a fact that demonstrates the poor efficacy of major arterial ligation in abnormal invasive placentation. After appraise a video (supporting by the author), we cannot realise the advantages of this technique to solve the dissection of the vesicouterine plane because we cannot find any evidence of anterior abnormal placentation, such as bulging, myometrial thinning or newly formed vessels between the bladder, the uterus and the placenta. Unfortunately, there is no viewing of the invaded area before or after the surgery; for this reason, it is not possible to conclude if this case is a true placenta accreta or percreta. The author should be able to demonstrate that in his experience this technique reduces maternal morbidity (bleeding and bladder lesions), compared with traditional hysterectomy or previously published conservative techniques.⁴ We hope that obstetricians think deeply about this technique before using it, mainly to understand that is not an easy solution for abnormal placentation cases, and also that Dr Matsuzaki be recognised as the original author of this approach. ■

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Author's reply

Sir,

We read with great interest the letter from Drs Palacios-Jaraquemada and Cali, and wish to clarify the important points raised.

Not a new surgical technique

When trying to secure the lower uterine vasculature, others have used the 'Doyen reverse hysterectomy approach', advancing from the cul-de-sac and incrementally upwards alongside the cervix, without dissecting the bladder from the lower segment.¹ The main drawbacks of this approach are suboptimal exposure of the cul-de-sac and uterine arteries behind the enlarged uterus, and the necessity for posterior upward cervical traction as one advances. Cervical traction might cause haemorrhagic avulsion of placental vessels and ureteral injuries.²

In our technique, the posterior vaginal fornix at the pouch of Douglas is exposed by the placement of a sponge stick into the vagina. Lower uterine segment elevation simplifies the exposure and dissection of critical areas, allowing for optimal visualisation of the pouch of Douglas and uterine vessels behind the enlarged uterus.³

Bleeding control and improved bladder separation

Developing the vesicouterine space early in the operation using the

conventional caesarean hysterectomy provokes immediate haemorrhage. In our technique, complete surgical devascularisation of the uterus is performed before attempting the separation from the bladder. In the presence of bladder wall invasion, anterior bladder wall cystotomy is helpful for defining dissection planes and determining whether posterior bladder wall resection is required.

Aortic cross-clamping in almost all cases?

This study only included women operated on with this new technique. Aortic cross-clamping was performed prophylactically in cases of suspected placenta percreta.

Internal iliac artery ligation

Most authors consider internal iliac artery ligation to be a safe procedure if performed carefully and with knowledge of pelvic anatomy. What then occurs is the virtual abolition of arterial pressure and a concomitant lack of pulsation. Thrombosis in the vessels may remain *in situ*, allowing for the identification of remaining individual bleeding sites for ligation.⁴

Video uncertainty

A definitive diagnosis of placenta accreta, increta, and percreta cannot be made until after delivery. Most cases of placenta percreta involving the bladder are recognised only during delivery.

Video S1 shows our technique in a patient with placenta increta. Hence, there are no external signs of placental invasion.

Maternal morbidity

Analytical studies must be performed to confirm its effectiveness and safety.

Is this an easy solution?

Outcomes are improved when the delivery of women with placenta accreta is accomplished in centres with

multidisciplinary expertise and experience in the care of this pathology.

Original author of this approach

The authors seem to have misunderstood our new surgical technique. It goes beyond a simple retrograde hysterectomy, as it mimics the en bloc resection of a pelvic malignancy for cul-de-sac disease in one contiguous sample. We widely open the retroperitoneum, enabling the identification of noble structures to then mobilise ureters laterally. The exposure of the cul-de-sac and dissection of critical areas are facilitated by the placement of a sponge stick into the vagina and lower uterine segment elevation. After uterine devascularisation, the development of vesicouterine space allows the specimen to be drawn out of the pelvis and an assessment of the need for partial bladder resection is made. ■

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Re: A recent study by economists on the impact of home births on infant outcomes confuses the debate on home birth

Sir,

We respond to the letter by De Jonge et al.¹ claiming that our study confuses the home birth debate.² We argue the opposite.

True experiments on home birth safety are unfeasible. Observational studies tend to suffer from confounding factors. The case mix differs between low-risk women choosing home/hospital on countless measurable and unmeasurable characteristics. Multiple regressions cannot control for all differences, and nor does using planned birthplace instead of actual birthplace provide a full solution. Hence, it is unclear whether the absence of mortality differences between home and hospital births in De Jonge et al.³ reflects residual confounding, or really shows that home births are safe.

The next-best option to true experiments are ‘natural experiments’ in which perfectly similar women end up in different birthplaces as a result of some external, quasi-random factor. This is what we do in our study. De Jonge et al. apparently misunderstood this fact. We find that home births increase infant mortality for women with low incomes who are considered at low risk.

De Jonge et al. claim that our finding is solely the result of the inclusion of premature births.¹ This is not true.

- 1 We also conducted analyses using only births at term (available upon request), as well as separate analyses for deliveries before and after 40 weeks of gestation. In all cases, our results remained unchanged.
- 2 Importantly, our results are valid only for a subgroup of women: those who are affected by the external factor (the

‘instrumental variable’). Preterm (and post-term) deliveries are essentially unaffected by our instrumental variable, so our conclusions do not apply to (and are not influenced by) them.

De Jonge et al.¹ mention that our effect size is larger than the sample average mortality rate. It would be misleading to focus too strongly on point estimates when confidence intervals are wide. As we mention in our paper, our confidence interval starts at 2.1 deaths per 1000 births, which is significantly lower than the mortality rate in our sample. Moreover, as mentioned above, our results only refer to a subgroup of women for whom neonatal mortality rates may be higher.

Finally, De Jonge et al.¹ criticise our use of actual rather than planned birthplace. We use actual place of birth because the case mix differs across planned place of delivery as a result of self-selection.⁴ In addition, we find the same results in our paper when we reclassify women who were referred during delivery according to their planned place of delivery. And we conducted analyses using planned place of birth instead of actual place of birth (results available upon request). Each time, our results remained the same.

De Jonge et al. had all the information given above in writing and in greater detail before they submitted their letter to *BJOG*. Their similar letter to the journal where we published our article was rejected on the aforementioned grounds. This makes us wonder why De Jonge et al. still proceeded with writing this letter.

Though our results only show that home births are unsafe for certain groups of women at low risk, our results are closer to demonstrating causality than those described by De Jonge et al.,³ who claim that home births are safe for all women at low risk. We therefore strongly disagree with their statement that our study is what confuses the debate. ■