

Decreased brachial plexus palsy after institution of shoulder dystocia protocol

TO THE EDITORS: In the recent study evaluating outcomes after instituting a shoulder dystocia protocol that included 5 components, the authors describe a “reduced frequency of brachial plexus palsy when a shoulder dystocia occurred.”¹ I have several questions for the authors:

1. After institution of the protocol, did the institutional cesarean rate change?
2. Did the protocol include the use of downward traction?
3. Given the stable shoulder dystocia rate for the 3 study periods, to what do the authors ascribe the decreased brachial plexus palsy rates after institution of the protocol?

If the brachial plexus injuries were related to the shoulder dystocia event and not traction by the provider, would the authors have expected the brachial plexus palsy rate to have remained stable? ■

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The author reports no conflict of interest.

REFERENCE

1. Grobman WA, Miller D, Burke C, Hornbogen A, Tam K, Costello R. Outcomes associated with the introduction of a shoulder dystocia protocol. *Am J Obstet Gynecol* 2011;205:513-7.

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REPLY

We would like to thank Dr Jelsema for his interest in our article¹ as well as the questions he has asked. During time periods A, B, and C of the study, the cesarean rates were 27.1%, 28.2%, and 29%, respectively. This change in the point estimate was not statistically significant ($P = .12$). Even if statistical significance had been reached, we do not believe this would explain a decrease in the frequency of brachial plexus palsy documented

given the denominator for the frequency was “per shoulder dystocia,” and that the frequency of shoulder dystocia per delivery did not change over time. The protocol itself was focused upon the team response to the shoulder dystocia and did not convey specific maneuvers, or order of maneuvers, that should be employed in the management of the shoulder dystocia. Thus, providers’ judgment was relied upon to determine what maneuvers and traction were employed.

The last 2 questions are more difficult to answer, as they are speculative as opposed to factual. We do not believe we can know, from the study itself, what exact factor is responsible for the reduction in brachial plexus palsy. However, we do not believe it is due to providers changing their approach to traction, as that change was not part of the protocol. Indeed, we believe this study highlights that it is unlikely that there is one “provider” who is responsible for each and every obstetric outcome, particularly in a setting as emergent as shoulder dystocia and for an outcome with as many potential etiologies as brachial plexus palsy. Instead, the importance of a team approach, which includes contributions from physicians, midwives, nurses, patients, and their families, is emphasized as a potential factor in ameliorating the consequences of this obstetric emergency. ■

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Lymphadenectomy in endometrial cancer: what’s the right question?

TO THE EDITORS: We read, with great interest, the article by Sharma et al,¹ in which, in a large retrospective study on data from the National Cancer Institute’s Surveillance, Epidemiology, and End Results database, the authors analyzed women with stages I-II endometrioid adenocarcinomas of the uterine corpus treated between 1988 and 2006. Findings suggest that,

especially among women with high- to intermediate-risk tumors, patients who undergo lymphadenectomy are less likely to receive external-beam radiation. According to the authors, data support lymphadenectomy for the majority of women with endometrial carcinoma, thus sparing radiation-related morbidity and costs.

The debate concerning the proper indications for additional surgical procedures like lymphadenectomy, as well as postoperative treatments, is longstanding.

Both prospective randomized trials evaluating hysterectomy, with or without lymph node dissection, showed pelvic lymphadenectomy does not influence survival, with findings subsequently confirmed by a metaanalysis.²

Likewise, in spite of variegated retrospective analyses of selected groups of patients, aortic lymphadenectomy failed to demonstrate survival benefit in any prospective trials.³

As to adjuvant therapies, we agree with the authors that an optimal treatment is still unknown. Many trials demonstrated that radiation may improve local control without any impact on overall survival for patients affected by uterine-confined disease.⁴

In our opinion, the question should not be whether lymphadenectomy alters adjuvant radiation rate but rather whether nodal resection could have an impact on the natural history of endometrial carcinoma.

Data seem to suggest nodal status may represent a collateral marker of biological disease aggressiveness, without providing definitive indications on cancer spread.³

To date, there is no reliable marker to identify high-risk patients, needing strict follow-up, with or without adjuvant treatments.

In a randomized trial of women with apparent stages I-IIA endometrial cancer (Post-Operative Radiation Therapy for Endometrial Carcinoma-2), vaginal brachytherapy resulted not inferior to pelvic radiation in preventing locoregional relapse.⁴

So, are we going down the wrong road?

It appears illogical to avoid a treatment jeopardized by considerable morbidity and costs without survival benefit (ie, radiotherapy) by choosing another procedure (ie, lymphadenectomy), this too blighted by costs and morbidity, with no proven survival benefit.

Until we have reliable biomolecular markers, brachytherapy seems a reasonable choice when aiming for locoregional control, especially for nonsexually active women, or else chemotherapy treatment could be used to control a suspected systemic spread of disease.

In these cases, without pelvic radiotherapy, salvage lymphadenectomy could be safely performed in the case of nodal relapse. ■

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REFERENCES

1. Sharma C, Deutsch I, Lewin SN, et al. Lymphadenectomy influences the utilization of adjuvant radiation treatment for endometrial cancer. *Am J Obstet Gynecol* 2011;205:562.e1-9.
2. May K, Bryant A, Dickinson HO, et al. Lymphadenectomy for the management of endometrial cancer. *Cochrane Database Syst Rev* 2010; CD007585.
3. Benedetti Panici P, Basile S, Maneschi F, et al. Systematic pelvic lymphadenectomy vs. no lymphadenectomy in early-stage endometrial carcinoma: randomized clinical trial. *J Natl Cancer Inst* 2008;100:1707-16.
4. Nout RA, Smit VT, Putter H, et al. Vaginal brachytherapy versus pelvic external beam radiotherapy for patients with endometrial cancer of high-intermediate risk (PORTEC-2): an open-label, non-inferiority, randomised trial. *Lancet* 2010;375:816-23.

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REPLY

We appreciate the interest of Dr Basile and his colleagues in our work. Utilizing a large population-based database of patients with endometrial cancer treated from 1988 to 2006, we noted that those women who underwent lymphadenectomy were less likely to receive adjuvant external beam radiotherapy than those who did not undergo nodal evaluation. The association between lymphadenectomy and avoidance of radiation was strongest for women with intermediate risk tumors (Fédération Internationale de Gynécologie et d'Obstétrique 1988 stage IB grades 2 and 3 and stage IC grades 1 and 2).

Basile et al suggest that the proper question to ask is not whether lymphadenectomy influences the use of radiation but rather whether lymphadenectomy influences the natural history and survival of patients with endometrial cancer? In theory, we certainly agree with this point. Despite methodologic issues that have been well discussed, 2 large randomized trials failed to show a survival benefit for lymphadenectomy.^{1,2} Likewise, the Post-Operative Radiation Therapy for Endometrial Carcinoma (PORTEC)-2 investigators noted that vaginal brachytherapy was not inferior to pelvic radiation for women with apparent early-stage endometrial cancer.³

However, in clinical practice the issue is not as straightforward and clear-cut as to whether lymphadenectomy influences survival. Decisions regarding adjuvant therapy must be made and a fair question is whether lymphadenectomy helps to guide clinicians in these decisions. Much of the difficulty with these decisions arises from the lack of clear data defining optimal adjuvant therapy for endometrial cancer. The appropriate treatment for high-risk disease confined to the uterus as well as for patients with isolated nodal disease remains a subject of active debate. In the United States, patients with stage IIIC endometrial cancer are frequently treated with multimodality therapy including both chemotherapy and radiation.⁴ The purpose of the radiation is to sterilize nodal disease. Is chemoradiation superior to chemotherapy alone? If so, should we omit pelvic radiation in a patient with a grade 2 tumor invading 90% of the endometrium who did not undergo lymphadenectomy but has a nearly 20% risk of nodal disease?⁵ Although it is