

Should we offer elective induction of labor to nulliparous women at 39 weeks?



TO THE EDITORS: We have read with great interest the metaanalysis of cohort studies by Grobman et al¹ that compared elective induction of labor at 39 weeks gestation among nulliparous women with expectant treatment. The authors illustrated that elective induction of labor at 39 weeks gestation was associated with a significantly lower risk of cesarean delivery, maternal peripartum infection, and perinatal adverse outcomes, which include respiratory morbidity, intensive care unit admission, and death. These results further confirm the conclusion of the A Randomized Trial of Induction Versus Expectant Management (ARRIVE) that was published in August 2018,² which led a revolution in the management of pregnancies beyond 39 weeks gestation in low-risk women, with many clinicians dismissing expectant management in favor of induction.

We should, however, carefully consider the limitations inherent in the work of Grobman et al before too hastily dismissing long-accepted practice. One of the shortcomings of this metaanalysis is that 4 of the 6 cohort studies involved a majority of subjects who were overweight or obese; 42% of women in the fifth study presented excessive weight gain.¹ A similar observation can be drawn from the ARRIVE trial, in which obesity was very prevalent (more than one-half in both study groups).² We question the generalizability of these results, because up to one-third of all pregnancy complications are thought to be attributable to overweight/obesity or excessive gestational weight gain.³ It is possible that they are applicable only in high-risk populations, notably regarding higher maternal body mass index.

In this context, we favor weighing up maternal and fetal indications over reflex interventions based on gestational age, while we acknowledge that cesarean delivery rates among selected populations may benefit from routine induction. ■

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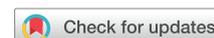
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REPLY



We thank the authors for their interest in our metaanalysis concerning labor induction at 39 weeks of gestation.¹ We could not agree more with their statement that this intervention should not be a “reflex” based on gestational age alone. Indeed, labor induction at 39 weeks of gestation without other indication should be initiated not as a reflex action but in the context of patient choice and a shared decision-making framework. Part of such a framework is the conveyance of accurate information based on available data. Although it is true that obesity was prevalent in these observational studies as well as in the A Randomized Trial of Induction Versus Expectant Management (ARRIVE) trial,² there is no indication that obesity per se was the characteristic wholly or in part responsible for the benefits that are associated with labor induction, which are quite consistent across studies with different populations. Indeed, in the ARRIVE trial, an a priori subgroup analysis demonstrated that cesarean delivery was reduced by a similar relative magnitude among women who were assigned randomly to the planned induction arm, regardless of body mass index strata (or age, cervical status, or self-reported race or ethnicity). Thus, at this point, there is no subgroup that has been identified for which the ARRIVE trial results (or the similar results from the observational studies in the meta-analysis) are not applicable. Until the time that such a subgroup is determined, we believe it is important to support patient choice based on data that do exist, which suggest that no particular subgroup of women at 39 weeks of gestation accrues less benefit from labor induction. ■

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Cystoscopy at the time of benign hysterectomy: a decision analysis



TO THE EDITORS: We were excited to read your timely and important publication “Cystoscopy at the time of benign hysterectomy: a decision analysis”¹ because safety in and quality of gynecologic surgery is a major focus. We wish to express several concerns regarding the methods of this study and possible implications for its interpretation and conclusions.

First, we suggest the outcomes used within the authors’ models are incomplete. In the paper, Cadish et al¹ account for only 90-day outcomes of diagnostic testing, treatment, and readmission for treatment without fully considering other documented infectious and systemic sequelae. Recently, Blackwell et al² demonstrated the profound and far-reaching consequences of a delayed ureteral injury, with includes increased odds of rehospitalization, kidney injury, sepsis, and death. A robust model of lower urinary tract injury at the time of hysterectomy should include these substantive complications to ensure appropriate cost estimates of delayed injury recognition.

Second, although we agree that the use of Medicare fee schedules represents an appropriate cost basis, our concern is with the use of these data. The reported costs appear to include only the professional component of each procedure.¹ In our experience, it is customary also to include facility, anesthesia, diagnostic interpretation, and hospitalization fees where applicable. Exclusion of these fees, which are published by the Centers for Medicare and Medicaid Services,^{3,4} systematically underestimates all costs that are used within the proposed model. For example, the authors used a cost of \$858.41 for laparoscopic hysterectomy (CPT 58571). When accounting for the facility fee of an ambulatory surgical center and the anesthesia fee for a presumed 2-hour case in a low-risk patient, we calculate a cost estimate of \$8198.66.³ Appropriate tabulation of these costs may change conclusions significantly regarding the marginal cost of cystoscopy and the reported threshold analysis findings.

Third, we question the assumptions used in the model of “selective” cystoscopy. The authors describe performing cystoscopy in “the group with above-average risk” but provide no further enumeration of the probability of cystoscopy or risk calculation/distribution.

Given our concerns regarding the assumptions and cost calculations in this analysis, we caution against the use of this study’s conclusions to drive decision-making regarding

cystoscopy at the time of hysterectomy and will continue to support its universal implementation at our institution while awaiting further research that will address this important topic. ■

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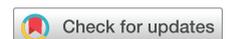
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REPLY



We agree that delayed ureteral injury causes significant morbidity after hysterectomy. We intentionally modeled hysterectomy modalities separately, accounting for increased thermal injury with laparoscopic or robotic approaches. Blackwell et al¹ published their study of delayed ureteral injury sequelae after our analysis was complete, but even so, delayed injury rarely is diagnosed beyond the 90-day postoperative period, even when