umbilical cord prolapse (malpresentations, multifetal pregnancy, polyhydramnios, etc.), a review by Wong, et al.\(^1\) does not contain a discussion of prevention of umbilical cord prolapse. Funic presentation in the third trimester of pregnancy is the major identifiable risk factor for prolapse of the umbilical cord.\(^3,4\)

Currently, there is no strategy to prevent umbilical cord prolapse in patients with funic presentation or other conditions putting patients at risk (eg, polyhydramnios). Recently, we presented our data on using third trimester cervical cerclage to prevent umbilical cord prolapse in patients with persistent funic presentation.\(^5\) None of our patients developed umbilical cord prolapse or experienced any complications from cerclage placement.\(^5\) Cervical cerclage provides the mechanical obstruction of the cervix, which will not allow the presenting umbilical cord to prolapse (Figure).

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REFERENCES


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**FIGURE**

Multiple loops of umbilical cord below the presenting part. Cervical cerclage prevents it from prolapsing.

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**Does simulation really increase gynecologic surgical skill?**

**TO THE EDITORS:** We read with interest the article by Orejuela et al.\(^1\) evaluating the effect of simulation training on learner operative skills in gynecologic surgeries. The authors concluded that simulation-based training could improve operative skills, and had a moderate quality of evidence. However, we would like to highlight the following methodological concerns of these results.

The authors pooled the results of randomized controlled trials (RCTs) and nonrandomized comparative studies (NRCs) together, which may introduce bias because of their methodological differences. We therefore performed a subgroup analysis on the basis of study design (Supplemental Figures 1–3) and found that there are differences in the effect estimates between NRCs and RCTs among 3 meta-analyses. In Supplemental Figures 1 and 3, the NRCs showed a considerable difference, whereas the RCTs did not. In Supplemental Figure 2, a markedly small effect of a decrease was found in the pooled result, irrespective of the study design. However, there was no significant difference in the subgroups of both NRCs and RCTs. Therefore, we believe that the NRCs and RCTs should not simply be pooled in a single metaanalysis.\(^2\) The conclusions on the effectiveness of simulation-based training could be considered misleading and might lack clinical relevance, as RCTs are more likely to provide unbiased information than NRCs.

In addition, the authors did not correctly use the Grading of Recommendations Assessment, Development, and Evaluation (GRADE) approach and did not report each factor for downgrading and upgrading the quality of evidence explicitly and transparently. First, according to the GRADE methodology,\(^3\) the quality of RCTs and NRCs should be assessed separately, and the final assessment result should be presented by considering the results of RCTs and NRCs comprehensively. Please note, we only present the results of the RCTs, because
the quality of evidence of RCTs is equal to or higher than NRCs in the current review (Supplemental Table). Second, the NRCs should initially be rated as “low” according to the GRADE handbook. Third, the quality of evidence for all the outcomes should also be downgraded by imprecision, because the sample sizes of the included studies were relatively small.

Researchers should decide when to include both RCTs and NRCs in evidence synthesis and should carefully explain the results from different study types, which will increase the certainty and comprehensiveness for a certain research question. We also recommend that reviewers should seek appropriate methodology training in conducting systematic reviews and do their best to guarantee the reliability and accuracy of results.

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Summarizing the evidence of the benefit of surgical simulation in gynecologic surgery

We appreciate the interest in our research on the important topic of evidence-based surgical training by Shen et al. Although we agree with the concern about pooling randomized and observational studies, we believe that in the setting of limited evidence, as is the case regarding simulation training for gynecologic surgeries, analyzing all studies together provides the clearest summary of the evidence. In contrast with the suggestion by Shen et al, the evidence does not suggest differences in findings between the randomized trials and nonrandomized comparative studies. It is erroneous to compare only the statistical significance of different groups of studies; this does not determine whether there are differences in findings but likely says more about power differences than statistical differences. Although we omitted the data from our manuscript, we conducted linear regression analyses to determine whether the trials and observational studies had significantly different findings. In brief, none did (high fidelity vs usual scores, P=.63 between study designs; high fidelity vs usual operating time, P=.81; low fidelity vs usual scores, P=.10).

Regarding the Grading of Recommendations, Assessment, Development, and Evaluation (GRADE) methodology, we used the same rigorous system in all studies to assess their methodologic limitations and downgraded the observational studies as per the ROBINS-I tool. This is consistent with guidance from the ROBINS-I and GRADE authors. Lastly, sample size is only 1 component to consider the precision of the quality of evidence. We focused on the confidence interval and statistical significance of the estimates in our determination of precision and concluded that the estimates were sufficiently precise for our conclusions.

We believe that our conclusions still hold as originally stated. We would like to again emphasize the need for more randomized trials with larger numbers of trainees that report patient–related outcomes.
### SUPPLEMENTAL FIGURE 1

High-fidelity simulators vs usual training

<table>
<thead>
<tr>
<th>Study or Subgroup</th>
<th>High Fidelity</th>
<th>Usual Training</th>
<th>Std. Mean Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Total</td>
</tr>
<tr>
<td>Larsen 2009</td>
<td>33.1</td>
<td>12.4</td>
<td>11</td>
</tr>
<tr>
<td>Jokinen 2019</td>
<td>16.4</td>
<td>5.3</td>
<td>10</td>
</tr>
<tr>
<td>Jokinen 2020</td>
<td>17.1</td>
<td>3.1</td>
<td>10</td>
</tr>
<tr>
<td>Subtotal (95% CI)</td>
<td>31</td>
<td>28</td>
<td>96.8%</td>
</tr>
</tbody>
</table>

Heterogeneity: Tau² = 0.60; Chi² = 6.87, df = 2 (P = 0.03); I² = 71%
Test for overall effect: Z = 1.66 (P = 0.10)

#### 1.1.2 NRCS

<table>
<thead>
<tr>
<th>Study or Subgroup</th>
<th>High Fidelity</th>
<th>Usual Training</th>
<th>Std. Mean Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Total</td>
</tr>
<tr>
<td>Akdemir 2014</td>
<td>17</td>
<td>4.5</td>
<td>20</td>
</tr>
<tr>
<td>Subtotal (95% CI)</td>
<td>20</td>
<td>20</td>
<td>30.4%</td>
</tr>
</tbody>
</table>

Heterogeneity: Not applicable
Test for overall effect: Z = 3.30 (P = 0.0010)

Total (95% CI) 51 | 48 | 100.0% | 0.94 [0.24, 1.64] |

Heterogeneity: Tau² = 0.30; Chi² = 7.50, df = 3 (P = 0.06); I² = 60%
Test for overall effect: Z = 2.63 (P = 0.008)

Test for subgroup differences: Chi² = 0.16, df = 1 (P = 0.69); I² = 0%

Objective structured assessment of technical skills/objective structured assessment of laparoscopic salpingectomy scores (revised).


### SUPPLEMENTAL FIGURE 2

High-fidelity simulators vs usual training

<table>
<thead>
<tr>
<th>Study or Subgroup</th>
<th>High Fidelity</th>
<th>Usual Training</th>
<th>Std. Mean Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Total</td>
</tr>
<tr>
<td>Larsen 2009</td>
<td>11</td>
<td>14.4</td>
<td>11</td>
</tr>
<tr>
<td>Jokinen 2019</td>
<td>14.6</td>
<td>12.6</td>
<td>10</td>
</tr>
<tr>
<td>Jokinen 2020</td>
<td>144</td>
<td>21</td>
<td>10</td>
</tr>
<tr>
<td>Subtotal (95% CI)</td>
<td>31</td>
<td>28</td>
<td>41.8%</td>
</tr>
</tbody>
</table>

Heterogeneity: Tau² = 0.02; Chi² = 2.22, df = 2 (P = 0.33); I² = 10%
Test for overall effect: Z = 1.27 (P = 0.20)

#### 1.2.2 NRCS

<table>
<thead>
<tr>
<th>Study or Subgroup</th>
<th>High Fidelity</th>
<th>Usual Training</th>
<th>Std. Mean Difference</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
<td>Total</td>
</tr>
<tr>
<td>Aalborg 2013</td>
<td>376</td>
<td>200</td>
<td>7</td>
</tr>
<tr>
<td>Akdemir 2014</td>
<td>335</td>
<td>157</td>
<td>20</td>
</tr>
<tr>
<td>Janissen 2015</td>
<td>75</td>
<td>62</td>
<td>15</td>
</tr>
<tr>
<td>Subtotal (95% CI)</td>
<td>42</td>
<td>41</td>
<td>58.2%</td>
</tr>
</tbody>
</table>

Heterogeneity: Tau² = 0.03; Chi² = 2.40, df = 2 (P = 0.30); I² = 17%
Test for overall effect: Z = 1.77 (P = 0.08)

Total (95% CI) 73 | 69 | 100.0% | -0.40 [-0.74, -0.06] |

Heterogeneity: Tau² = 0.00; Chi² = 4.66, df = 5 (P = 0.46); I² = 0%
Test for overall effect: Z = 2.33 (P = 0.02)

Test for subgroup differences: Chi² = 0.06, df = 1 (P = 0.81); I² = 0%

Total operation time (revised).

SUPPLEMENTAL FIGURE 3
Low-fidelity simulators vs usual training

<table>
<thead>
<tr>
<th>Study or Subgroup</th>
<th>Low Fidelity</th>
<th>Usual Training</th>
<th>Std. Mean Difference</th>
<th>(IV, Random, 95% CI) Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Coleman 2002</td>
<td>21.7</td>
<td>5.9</td>
<td>11</td>
<td>20.3</td>
</tr>
<tr>
<td>Banks 2007</td>
<td>64</td>
<td>5</td>
<td>10</td>
<td>45</td>
</tr>
<tr>
<td>Galia 2013</td>
<td>30</td>
<td>3</td>
<td>48</td>
<td>27.5</td>
</tr>
<tr>
<td>Subtotal (95% CI)</td>
<td>69</td>
<td>72</td>
<td>64.2%</td>
<td></td>
</tr>
</tbody>
</table>

Heterogeneity: Tau² = 0.44, Chi² = 7.23, df = 2 (P = 0.03); I² = 72%
Test for overall effect: Z = 1.95 (P = 0.05)

2.1.2 NRCS
Anotosh 2012
17 2.2 20 11.8 4.5 20 23.6% 1.44 [0.74, 2.14] 2014

Heterogeneity: Tau² = 0.00, Chi² = 0.83, df = 1 (P = 0.36); I² = 0%
Test for overall effect: Z = 4.04 (P < 0.0001)

Total (95% CI)
93 100.0% 0.98 [0.37, 1.58]

Heterogeneity: Tau² = 0.27, Chi² = 10.81, df = 4 (P = 0.03); I² = 63%
Test for overall effect: Z = 4.16 (P = 0.002)

Test for subdomains: Chi² = 0.53, df = 1 (P = 0.47), I² = 0%

Objective structured assessment of technical skills/ objective structured assessment of laparoscopic salpingectomy scores (revised).
C.I. confidence interval; N.W. mean difference; RCT, randomized controlled trial; SD, standard deviation.


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Letters to the Editors