



Comparative Effectiveness Review Disposition of Comments Report

Research Review Title: *Labor Dystocia*

Draft review available for public comment from September 5, 2018, through October 10, 2018.

Research Review Citation: Myers ER, Sanders GD, Coeytaux RR, McElligott KA, Moorman PG, Hicklin K, Grotegut C, Villers M, Goode A, Campbell H, Befus D, McBroom AJ, Davis JK, Lallinger K, Fortman R, Kosinski A. Labor Dystocia. Comparative Effectiveness Review No. 226. (Prepared by the Duke Evidence-based Practice Center under Contract No. 290-2015-00004-I.) AHRQ Publication No. 29-EHC007. Rockville, MD: Agency for Healthcare Research and Quality; May 2020. DOI: <https://doi.org/10.23970/AHRQEPCCER226>. Posted final reports are located on the Effective Health Care Program [search page](#).

Comments to Research Review

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Comments on draft reviews and the authors' responses to the comments are posted for public viewing on the Web site approximately 3 months after the final research review is published. Comments are not edited for spelling, grammar, or other content errors. Each comment is listed with the name and affiliation of the commentator, if this information is provided. Commentators are not required to provide their names or affiliations in order to submit suggestions or comments.

The tables below include the responses by the authors of the review to each comment that was submitted for this draft review. The responses to comments in this disposition report are those of the authors, who are responsible for its contents, and do not necessarily represent the views of the Agency for Healthcare Research and Quality.

Section	Commentator & Affiliation	Comment	Response
Quality of Report	TEP Reviewer #1	Superior	Thank you. No response needed
Quality of Report	TEP Reviewer #2	Good	Thank you. No response needed
Quality of Report	TEP Reviewer #3	Superior	Thank you. No response needed
Quality of Report	Peer Reviewer #1	Fair	Thank you. No response needed
Quality of Report	Peer Reviewer #2	Superior	Thank you. No response needed
Quality of Report	Peer Reviewer #3	Good	Thank you. No response needed
Quality of Report	Peer Reviewer #4	Good	Thank you. No response needed
General	TEP Reviewer #1	This is an excellent and comprehensive review. It is an important clinical topic. It has been well framed, especially as regards the potential benefits in reducing the cesarean delivery rate (I thought this section was particularly well written) and potentially improving perinatal outcome.	Thank you. No response needed
General	TEP Reviewer #1	This systematic review has taken a while to put together. I am concerned that no papers were included after January 2016 (page 6, line 15), and that readers may see this as a major deficiency.	The final report now includes evidence up to the search date of September 6, 2018.
General	TEP Reviewer #2	The report addresses a clinically meaningful topic and the key questions are well defined.	Thank you. No response needed

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Section	Commentator & Affiliation	Comment	Response
General	TEP Reviewer #3	<p>I thank the DUKE team for this very thoughtful and well organized comparative effectiveness review.</p> <p>Particular strengths include the introduction, background, conclusion, and directions for future research sections; taken as a whole, these portions of the document succinctly and effectively frame this concern and the need for a review on labor dystocia and provide 4.57 both meaningful interpretation of the state of the science and where future research is needed. As well the CER information by each outcome is clear and comprehensive, the figures and tables are logical and easy to interpret, and the appendices are well done. An additional and very important strength of this review are the summaries and key findings of each key question. The authors have accomplished exemplary work in precisely stating what is known and what is not known about each subject.</p> <p>While there are many specific portions of the text that are outstanding, I would like to offer specific praise for the lucidity and accuracy of one statement and two directions for future research:</p> <p>a) (ES-26): ‘In summary, evidence suggests that the specific criteria used to define “normal” labor, or a specific threshold for intervention, may affect cesarean delivery rates but not other maternal or neonatal outcomes in some settings. Yet there is no available evidence for the United States. Among women in the United States with spontaneous onset of labor and vaginal delivery, labor progression is slower for women having their first baby compared to women with prior deliveries, but the high proportion of women receiving oxytocin augmentation prevents drawing any inferences about the “normal” labor curve in women with spontaneous onset of labor, no interventions to augment labor, and no adverse maternal or neonatal outcomes.’</p> <p>b) (ES-26): ‘It would be extremely useful to have separate labor curves derived from contemporary U.S. data for women with spontaneous onset of labor, no augmentation with oxytocin or other pharmacologic agents, and vaginal delivery of healthy baby, stratified by parity, as well as for women with augmented labor. Such labor curves would provide a better understanding of the modern natural course of labor and may provide better information on when to initiate agents to augment labor and when to proceed with cesarean delivery.’</p> <p>c) (ES-27): ‘Given the importance of the labor process to patient preferences and their birthing experience and the lack of evidence about the impact of available interventions on these preferences, the development of tools for estimating patient preferences for both the process and maternal and neonatal outcomes of labor should be a priority. Discrete choice experiments would be one method appropriate for estimating preferences for these complex tradeoffs.’</p>	Thank you. No response needed

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General	TEP Reviewer #3	<p>Following are a few suggestions for strengthening this review:</p> <p>1) Page 13 (ES-1) lines 10 – 20: It may strengthen this section to add more specific information from the Preventing the First Cesarean ACOG/SMFM publication regarding how importantly the diagnosis of labor dystocia drives US cesarean delivery rates (up to 55% of the major indications for US cesarean- Spong et al 2013). While this is common knowledge to those of us working in these areas, the extent to which labor dystocia impacts US cesarean may not be known to stakeholders who may not have the time to thoroughly read the entire review.</p>	We have added the suggested text to the introductory paragraph.
General	TEP Reviewer #3	<p>) Page 13 (ES-1) lines 45-47: It would be more inclusive and thorough to also reference the efforts made by the American College of Nurse Midwives in safely reducing the US cesarean delivery rate: http://birthtools.org/Reducing-Primary-Cesareans-NEW</p>	We have made the suggested addition to the Evidence Summary and main text introduction
General	TEP Reviewer #3	<p>a) The excellent details about differences in the sample and population studied by Friedman vs in the CSL might importantly inform the naïve reader so that they can better appreciate the strengths and limitations of the earliest work on ‘normal’ vs ‘abnormal’ labor progress. This very nicely detailed on page 38 (ES-26)- ‘... the most striking difference between these two studies was the proportion of women who received oxytocin augmentation (14.6% in the NCPP cohort from 1959 to 1966 compared with 45.9% in the CSL cohort from 2002 to 2008).’</p> <p>i. Could a similar point to the details of page 38 be made on page 13?</p>	We have provided similar details within the introductory text to help clarify the limitations of normal vs abnormal labor research.
General	TEP Reviewer #3	<p>ii. The differences in rates of assisted delivery between the Friedman and the CSL samples is another important difference to point out for both the introduction and for the commentary on page 38</p>	We have added the following text to our introduction “although, as discussed below, there have been substantial changes in practice since these “traditional” definitions were developed which raise questions about their generalizability to modern populations.

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General	TEP Reviewer #3	b) The point could be made more explicitly it is clinician perspectives that labor is 'too long' that leads to the diagnosis of labor dystocia	We have added the following sentence" Variation between providers about definitions or perceptions of "abnormal" labor length may contribute to variations in rates of diagnosis" to the paragraph "Diagnosis" in the introduction
General	TEP Reviewer #3	<p>4) I appreciate that the expectation with this kind of review and in keeping with multiple methods guides and well-accepted standard methodologies is the heavy reliance on experimental studies. And for most of the outcomes compared, it is likely that RCTs are the optimal research design. For example, research comparing 2 vs 4 hour partograms (though I do appreciate that the Neal study was observational), spinal vs continual epidurals, or high vs low dose oxytocin- all of these comparisons are ideally examined when participants are randomized to comparison groups.</p> <p>However, the study of essentially normal physiologic processes, like labor, may ultimately require different scientific approaches to more confidently extrapolate findings to the broader population when other kinds of interventions are in question. Specific to this review, the study of certain labor interventions about which laboring women may have stronger preferences (e.g., water birth) or instincts (e.g., the positions one assumes during unmedicated labor) may create feasibility and ethical barriers to randomization. As well even women who are willing to be randomized may find it very difficult to persist during the trial, resulting in difficulty with recruitment and retention. High numbers of women declining or dropping out of a study raises different questions of bias than may be delineated for evaluating the quality of science for inclusion in a review. If it is of benefit, more details on this subject can be found here: http://bit.ly/causalinferenceseries</p> <p>It may strengthen the review to include one sentence acknowledging these limitations for the key questions involving outcomes that may be more difficult to study with RCTs and/or with one sentence in the 'Risk of Bias Assessment of Individual Studies' section on page 18 (ES-4).</p>	<p>We have added a statement to this page, along with a reference to a relevant systematic review of this issue:</p> <p>We acknowledge that one limitation to these standard criteria in settings where patients have strong <i>a priori</i> preferences for certain outcomes or aspects of the process (like labor) may affect willingness to be randomized, and may lead to issues related to generalizability or other biases, although available evidence suggests that there is little impact on internal validity.[King M, Nazareth I, Lampe F, et al. Impact of Participant and Physician Intervention Preferences on Randomized Trials: A Systematic Review. <i>JAMA</i>. 2005;293(9): 1089–1099. doi:10.1001/jama.293.9.1089]</p>

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General	TEP Reviewer #3	5) Page 30 (ES-18) Please define early vs. late epidural on this page.	We have indicated in a footnote that early epidural was defined as immediate initiation of EA at first request (< 4 cm), and late initiation consisted of delay of EA until the cervix was dilated to at least 4 cm.
General	TEP Reviewer #3	6) Page 36 (ES- 24) lines 37 – 48 Please consider adding more details regarding differences in intervention rates between the Friedman and CSL samples. In addition to clarifying the differences in practice patterns between the 1950s-70's and the early 2000's, by detailing how much all of these samples of laboring women received intervention, the point is even more explicitly made that we do not have US research about normal vs abnormal labor progress that includes samples with no to minimal intervention. Making this point in this portion of the document will further highlight the relevance of the first bulleted research recommendation of the review: ('It would be extremely useful to have separate labor curves derived from contemporary U.S. data for women with spontaneous onset of labor, no augmentation with oxytocin or other pharmacologic agents, and vaginal delivery of healthy baby, stratified by parity, as well as for women with augmented labor. Such labor curves would provide a better understanding of the modern natural course of labor and may provide better information on when to initiate agents to augment labor and when to proceed with cesarean delivery.)	We have expanded this future research need in the discussion and evidence summary.
General	TEP Reviewer #3	7) Page 36 (ES- 24) lines 49 - 55 Please consider adding this sentence to the end of this paragraph: 'Feasibility and ethical challenges with obtaining a large, contemporary sample of women laboring with minimal to no intervention limits our fundamental scientific understanding of normal labor, normal labor progress, and when durations of labor lead to worse maternal/child outcomes.'	We have added the suggested text in to the evidence summary and discussion section of the main report.

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General	TEP Reviewer #3	8) Page 37 (ES-25) lines 27 – 34 This information about the high numbers of RCTs that were conducted outside of the US, and the limitations this creates in generalizing findings to the US context, is incredibly vital information for appreciating this review. Could this information be shared in an additional area of the document? Could it be mentioned in the key messages?	We now include a key message that states “Much of the evidence on different interventions came from studies performed outside the US. Differences in patient, provider, health system and other characteristics may affect the applicability of these results to a US setting. ”
General	TEP Reviewer #3	Thank you again for the privilege of participating in the generating this comparative effectiveness review. Please don’t hesitate to contact me if I can clarify any suggestions or be of help in any other way.	Thank you. No response needed
General	Peer Reviewer #1	I am confused by the Key Messages on page ii. The first 3 bullets are not from this review.	The bullets correspond to our KQ1 which focuses on criteria for defining abnormal labor.
General	Peer Reviewer #1	My primary criticism is that there is a lack of internal consistency in the report of the purpose of the review stated on page ii (brief abstract), page vi (structured abstract), and on page 3 (Scope of the review). It further confused by the statement on ES-2 (lines 9-10) and page 3, "evaluates the comparative effectiveness of different strategies for treating labor dystocia in women with otherwise uncomplicated pregnancies." This is not what the review does since the majority of the comparisons were of interventions NOT done after a diagnosis of dystocia. Treatment traditionally follows diagnosis, which is a proverbial problem in maternity care, particularly since there are no clear evidence-based criteria for the diagnosis of dystocia. The majority of interventions evaluated in this review are used to promote physiologic labor (or to relieve women's pain) NOT to TREAT dystocia. This conundrum is why I cannot give the report any more than a fair evaluation. It simply does not do what the authors say it is going to do. It does not compare interventions for the treatment of dystocia. I also think that this review attempts to do too much and because of that the real meat is obscured; especially in relationship to the idea of the diagnosis of dystocia and treatment following diagnosis.	Many of these topics were suggested by the Technical Expert Panel. We have clarified in the introduction that the report addresses (1) issues related to diagnosis, including definitions and the use of partograms, (2) interventions or strategies used as part of labor management to either directly (artificial rupture of membranes) or indirectly (choice of pain management strategies or monitoring) prevent the diagnosis of dystocia, or (3) manage dystocia once diagnosed (timing and dosing of oxytocin).

Section	Commentator & Affiliation	Comment	Response
General	Peer Reviewer #2	The AHRQ Comparative Effectiveness Review titled “Labor Dystocia” is a 186 page review of the evidence on the definition of “normal” labor progression, and the comparative effectiveness of different strategies for treating labor dystocia in women with otherwise uncomplicated pregnancies. In general, the manuscript is well-written, with very few significant grammar or syntax corrections needed. This Reviewer would defer comments on statistical methodology to a professional Statistician. My specific comments and questions follow:	Thank you. No response needed
General	Peer Reviewer #2	Page 6 Line 15...Could the Authors further elaborate on why published studies prior to 2005 were not included, (i.e. why 2005 was selected as a cutoff), as many potentially relevant studies on intrapartum care are excluded from this analysis. Presumably there is a goal of assessing “contemporary” obstetrical practice, but the 2005 cutoff, as explained, seems arbitrary.	Through discussion with our technical expert panel we determined the start date of our search. Given changes in many aspects of practice (particularly threshold for C/S), there was concern that earlier studies may not be appropriate and therefore was limited to 2005 onwards. We clarify this now in the main report
General	Peer Reviewer #2	Page 16 Line 17...The figure does not fully define “uncomplicated pregnancies”, although later in the manuscript (page 54) exclusions are defined. Nevertheless, it remains unclear if medical complications (e.g. hypertension, diabetes, etc) or obstetrical complications (e.g. anomalies, IUGR) are excluded. After reading the manuscript, it would seem unclear whether a woman with various complications (e.g. full term with preeclampsia and IUGR) might be included in some of these studies, which would then call into question whether the various study populations were uniformly “uncomplicated”.	We have clarified that any study which explicitly included patients with medical or obstetrical complications were excluded, although the majority were not always explicit.
General	Peer Reviewer #2	Page 23 Line 46...The phrasing “...duration of duration...” is awkward and should be revised.	This phrasing has been revised.
General	Peer Reviewer #2	Page 23 Line 50...Was kneeling position a factor at delivery or during second stage or both? Was fetal position (i.e. occiput posterior) a factor?	The key points highlight the specific comparisons where the strength of evidence is greatest. Additional detail about other comparisons or outcomes are provided in greater detail in the main report.

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General	Peer Reviewer #2	Page 26 Line 53...Is the genus formally Anethum or Anethum, as references to both can be found in past publications?	We consistently use Anethum in this report following the lead of the included publications.
General	Peer Reviewer #2	Page 27 Line 36...Should read “perineum”, as opposed to “peritoneum”.	This has been revised.
General	Peer Reviewer #2	Page 27 Line 48...Why are some statistically significant (and for that matter insignificant) results reported in the tables (e.g. as percentages or odds ratios) and others not? Should the tables be standardized in this regard?	We have standardized the tables to not include specific odds ratios.
General	Peer Reviewer #2	Page 33 Line 49...The phrase “both each” seems redundant and might read better simply as “each”, here and elsewhere in the manuscript.	This revision has been made
General	Peer Reviewer #2	Page 151 Line 44...As a trade name, “Pitocin” should be capitalized, and probably be referred to as its’ generic “oxytocin” throughout the manuscript.	We have made the suggested edit.
General	Peer Reviewer #2	The key questions are very relevant and the report is meaningful to a large audience.	Thank you. No response needed
General	Peer Reviewer #2	Thank you very much for requesting my review of this interesting paper.	Thank you. No response needed

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General	Peer Reviewer #3	<p>Thank you for allowing me to review this comprehensive evidence based comparative effectiveness review on labor dystocia.</p> <ul style="list-style-type: none"> It is important to let the audience know that this is NOT a review about diagnosing labor dystocia but is specifically about what to do once labor dystocia is diagnosed. Perhaps the title should be changed to “Management of Labor Dystocia” to be more specific 	<p>Many of these topics were suggested by the Technical Expert Panel. We have clarified in the introduction that the report addresses (1) issues related to diagnosis, including definitions and the use of partograms, (2) interventions or strategies used as part of labor management to either directly (artificial rupture of membranes) or indirectly (choice of pain management strategies or monitoring) prevent the diagnosis of dystocia, or (3) manage dystocia once diagnosed (timing and dosing of oxytocin).</p>
General	Peer Reviewer #3	Pg. 2: Would recommend putting into “Purpose of Review” that this is for women in spontaneous labor and not women induced	We now state in the scope of the review section that we limit our review to women in spontaneous labor and exclude those who are undergoing induced labor
General	Peer Reviewer #8	Pg 2: In Key Messages: Why is bullet 2 included (about maternal age)? That is the only detail specific comment in there (as opposed to broad comments about general management strategies). Would recommend removing that statement	Because parity and maternal age are primary determinants of “normal progress”, we feel that this key message is important to retain.
General	Peer Reviewer #3	There is no mention of delayed vs. valsalva pushing in the “Key Messages” when it is mentioned as part of the “purpose of review” (or take out of the purpose of review)	The key messages focus on findings with low, moderate, or high strength of evidence. The evidence supporting delayed vs Valsalva pushing was insufficient.
General	Peer Reviewer #4	The report is clinically meaningful mainly because of its systematic and very well-defined approach. That is especially evident in the clear statement of the key questions and the approach to answering them.	Thank you. No response needed

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Introduction	TEP Reviewer #1	Background (1) Should a clear definition of labor dystocia be included up front or is this too variable?	We have expanded our discussion of the definition of labor dystocia in the introduction.
Introduction	TEP Reviewer #1	(2) Should it be made clear up front that this refers to progress in labor (i.e., assumes spontaneous labor and not induction of labor)?	We now state in the scope of the review section that we limit our review to women in spontaneous labor and exclude those who are undergoing induced labor
Introduction	TEP Reviewer #1	(3) Should it be clarified up front whether this is limited to labor at term or does it include analysis of preterm labor?	The exclusion of women in preterm labor is included in our exclusion criteria for the studies.
Introduction	TEP Reviewer #1	(4) Is there any reason to include in the background section a discussion about changes in the rate of labor dystocia over time? Or differences in labor dystocia throughout the country / the world? Or differences in labor dystocia in women of different ethnic/racial groups?	We have added to the introduction a reference to changes over time, which is discussed in more detail in KQ1.
Introduction	TEP Reviewer #1	(5) There is much discussion in the introduction section about shoulder dystocia. I think this should all be removed. This review is about labor dystocia and not shoulder dystocia. I found this confusing.	We have removed this discussion
Introduction	TEP Reviewer #1	(6) I personally distinguish between primary arrest (where the patient never got on their labor curve) vs secondary arrest (patient was on labor curve and then fell off). I find it useful when thinking about the causes of labor dystocia: the 3 P's (powers, passenger, passage). Secondary arrest is much more likely to be CPD due to latter two cases; primary arrest more likely inadequate contractions. I'm not sure this has been studied.	In discussions with the TEP, this stratification was not identified as a criteria for classifying studies, and, as the reviewer notes, we did not identify any included studies which used the classification.
Introduction	TEP Reviewer #2	The introduction highlights the purpose well.	Thank you. No response needed
Introduction	TEP Reviewer #3	The introduction is excellent. Please see general comments for several suggestions.	Thank you. No response needed
Introduction	TEP Reviewer #3	The report is clinically meaningful for comparisons that have adequate evidence to support reaching a conclusion. The report may ultimately be more meaningful in identifying important gaps in the science on this subject and, hopefully, will drive future research and shape future research funding.	We agree. No response needed.

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Introduction	TEP Reviewer #3	The target population and audience are well defined	Thank you. No response needed
Introduction	TEP Reviewer #3	The key questions are appropriate and explicitly stated	Thank you. No response needed
Introduction	Peer Reviewer #1	There are unreferenced sentences in the introduction (page 1, lines 22-27 and 29-31). Although I do not disagree with these statements, in a report such as this, these statements require referencing.	We have inserted in references to these specific sentences.
Introduction	Peer Reviewer #1	Why are the WHO statements on cesarean delivery rates ignored?	Although our primary focus is on the US, we have added in reference to the WHO statements on cesarean delivery
Introduction	Peer Reviewer #1	Page 2, Treatment strategies - this is where the problem of diagnosis preceding treatment becomes very apparent. See your statement in line 28-29. Virtually none of the studies reviewed to answer the KQ were interventions applied AFTER the diagnosis of dystocia. You must deal with this chicken & egg issue for your report to have any validity of interpretation.	As previously noted, we have clarified that the review addresses (1) diagnosis, (2) the possible effects of other common labor interventions on the diagnosis of prolonged labor, and (3) strategies (including oxytocin) for treating prolonged labor.
Introduction	Peer Reviewer #2	The Introduction does set the stage for subsequent analysis.	Thank you. No response needed
Introduction	Peer Reviewer #2	Page 48 Line 40...The aforementioned consensus is not universal (Clark SL, Garite TJ, Hamilton EF, Belfort MA, Hankins GD. "Doing something" about the cesarean delivery rate. Am J Obstet Gynecol 2018 Sep;219(3):267-271. PMID: 29733840), as discussed in this very recent opinion piece.	We have added reference to the suggested discussion of this topic.
Introduction	Peer Reviewer #2	Page 49 Line 8...Might this be an appropriate place to encourage development and use of "core outcome sets" in this type of clinical research?	We have inserted discussion of this need to the future research section of the report.
Introduction	Peer Reviewer #3	Abstract (pg. 6) <ul style="list-style-type: none"> Would reiterate some of my comments above into the abstract: this is management of labor dystocia and not defining it; this is for women in spontaneous labor and not women induced; comment on pushing if it was part of the purpose of the review 	The abstract has been updated to reflect the changes throughout the report and revised evidence.

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Introduction	Peer Reviewer #3	<p>Background</p> <ul style="list-style-type: none"> Pg 13: How is spontaneous labor defined? The authors define latent and active labor in this paragraph but do not define spontaneous labor. Does this include women admitted in early labor for augmentation? Does this include women with premature rupture of membranes? Is this based on contraction patten and cervical changes? A definition that was used must be included. Understanding that many studies may have defined it differently, there should be some guidance to the readers on the population of women this pertains to (perhaps it might be easier to include how non-spontaneous labor was excluded vs. how “spontaneous labor” was included. For ex, if all women undergoing an induction were excluded, then the authors may find it easier to define that and say that all other women were included). This is a critical element of interpreting and applying these data that must be included in the revisions. 	<p>We have inserted the following sentence to the introduction: “We also limit our review to women in spontaneous labor, with definitions varying somewhat between studies but generally including the onset of spontaneous uterine contractions, and explicitly exclude studies of women undergoing induction or women with premature rupture of membranes at term.”</p>
Introduction	Peer Reviewer #4	<p>The aims are very clearly stated and the approaches to answer them are well and logically defined.</p>	<p>Thank you. No response needed</p>
Methods	TEP Reviewer #1	<p>As regards the section entitled 'Scope of Review' (e.g., pages 14 and 50):</p> <p>In addition to stating what is 'in scope', I think it would be useful to clarify up front what is 'out of scope.' That is, what variables may be in play for clinicians when deciding whether to not to proceed with cesarean, but that are not considered in this systematic review. For example:</p> <ol style="list-style-type: none"> (1) multiple pregnancies (2) prior cesarean/VBAC (3) use of episiotomy (4) fetal macrosomia (size of the fetus) (5) contraindications to any of these procedures (e.g. not performing early amniotomy in patients with HIV or HCV) (6) assessment of clinical pelvimetry (7) whether magnesium sulfate (given for example to patients with preeclampsia) effects the labor curve and increases the cesarean delivery rate 	<p>We have added the sentences to the Scope of the Review section: “We explicitly excluded studies which included women with other potential indications for cesarean section (e.g., multiple pregnancies, prior cesarean) or other conditions which might affect either the likelihood of diagnosis of dystocia (e.g., use of magnesium sulfate in preeclampsia) or lead to contraindications to some interventions (e.g., HIV and amniotomy). We also did not include interventions such as estimation of fetal size or clinical pelvimetry which might affect physician perception of the risk of labor dystocia.”</p>

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Methods	TEP Reviewer #2	Inclusion and exclusion are justified. Definitions are appropriate.	Thank you. No response needed
Methods	TEP Reviewer #3	Inclusion and exclusion criteria are well justified and accurate	Thank you. No response needed
Methods	TEP Reviewer #3	The search strategies are clear, logical, and represent a thorough search.	Thank you. No response needed
Methods	TEP Reviewer #3	Definitions/diagnostic criteria are appropriate	Thank you. No response needed
Methods	TEP Reviewer #3	Statistical methods were used appropriately	Thank you. No response needed

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Methods	Peer Reviewer #1	<p>Why was sample size, e.g. power analyses for various outcomes, not discussed? This seems to be a vary obvious issue in much of the research, especially when low-incidence outcomes were reported. I encourage the investigators to consider whether the fact that a study was conducted outside of the US (particularly when it was conducted in another western country) to be such a major limitation. This is not discussed up front, yet in the results it is clear that unless a study was conducted in the U.S., the quality of the evidence was not valued.</p>	<p>Sample size is implicitly included in the rating of strength of evidence—measures of association for rare outcomes in smaller studies will by definition have wide confidence intervals and lead to low precision, which is one of the key elements in the SOE evaluation. We agree this is an issue, which is discussed in the Discussion section.</p> <p>The primary issue for non-US studies is that labor/delivery is in many ways a “complex intervention” that is affected by a number of factors that may well be unique to a given setting. A well-done RCT in a non-US setting definitely has value, but the question of generalizability—given differences in patients, providers, health systems, payers, etc., between the US and other countries, are the results generalizable to the US?</p>
Methods	Peer Reviewer #2	Inclusion & exclusion criteria seem reasonable (but require further elaboration as mentioned in my list of issues to address.	Thank you. No response needed
Methods	Peer Reviewer #2	Page 55 Line 28...It seems unusual that studies of nitrous oxide for intrapartum analgesia do not garner more mention, relative to some of the other interventions cited with low strength of evidence.	To be included in our review, studies needed to meet our inclusion/exclusion criteria as defined in the methods.
Methods	Peer Reviewer #3	Combined in response below	Thank you. No response needed

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Section	Commentator & Affiliation	Comment	Response
Methods	Peer Reviewer #4	It is not clear why the search has been conducted from January 1, 2005, to various dates in January 2016. Was this time period chosen arbitrarily or was there a rationale behind it.	Through discussion with our technical expert panel we determined the start date of our search. Given changes in many aspects of practice (particularly threshold for C/S), there was concern that earlier studies may not be appropriate and therefore was limited to 2005 onwards. The search has been updated and now includes literature through February 2019.
Results	TEP Reviewer #1	Findings are well summarized based on the available data. SOE is well defined and appropriately assigned.	Thank you. No response needed
Results	TEP Reviewer #1	A few specific comments: (1) I particularly like the inclusion of forest plots where possible.	Agree. No response needed
Results	TEP Reviewer #1	(2) I believe emotional support in labor has also been shown to decrease the need for pain medication in labor (page 23, line 36). I think this is a worthwhile endpoint to include.	The endpoints included in our analyses were determined based on feedback from our technical expert panel in terms of endpoints most critical to decision making. Although we agree the need for pain medication it is important endpoint, it is not directly in causal path related to other endpoints
Results	TEP Reviewer #1	(3) I would clarify in the text describing the Analytic Framework that the neonatal outcomes included are short-term only; we have not included an analysis of the long-term outcomes from any of these interventions.	The reviewer is correct that the neonatal outcomes of interest emphasize short term outcomes which are explicitly stated in the PICOTS framework.
Results	TEP Reviewer #2	The amount of detail is helpful and study characteristics are described sufficiently. The forest plots are very helpful.	Thank you. No response needed

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Results	TEP Reviewer #2	I think the some of the literature on delayed pushing is missing, including the largest trial to date (Fraser W et al, NEJM). There is a bulleted statement at the top of page 166 that states there are no RCTs on the timing of initiation of pushing but that is not correct.	The revised report is now updated to include the most recent evidence including studies related to timing or initiation of pushing.
Results	TEP Reviewer #3	Please see general comments or attached file for comments.	Thank you. No response needed
Results	TEP Reviewer #3	I am not aware of any studies that the investigators failed to include	Thank you. No response needed
Results	Peer Reviewer #1	The results by each KQ are inconsistently reported. Different details of the included studies for a KQ are reported; funding for included studies is sometimes addressed and other times not. I highly recommend that the authors adopt a systematic approach to their review for each KQ and edit the report accordingly with parallel information reported by each KQ.	In the revised report we have attempted to consistently report findings from the included studies.
Results	Peer Reviewer #1	I find it difficult to separate conclusions drawn from a group of single studies vs. systematic or meta-analytic reviews. The authors did not consistently state whether any of the single studies were included in any of the SR/MA for each specific comparison. Nor did they state the years of study publication for the studies included in each SR/MA. I consider this a significant limitation.	We have inserted in additional detail regarding the use of the systematic reviews in our SOE ratings and also the overlap of these SRs with our included individual studies.
Results	Peer Reviewer #1	KQ1 is not answered in the review. A 2-hr versus 4-hr action line on an undefined partogram does not answer the question of diagnosis of dystocia without consideration of the metrics, e.g. expectations of progress, that were built into the partograms evaluated.	In the discussion of SOE, we have added "In general, the SOE was reduced for outcomes because of inconsistencies between studies (which may be related to variation in the definitions used to build the partograms), and because the evidence was largely based on studies from non-U.S. settings (and several focused on low-resource settings)"

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Results	Peer Reviewer #1	There is inconsistency in the statement of various KQ and the order in which results are reported. See the KQ 3 Supportive Care. In the question the order is "ambulation, nutrition, hydration, and emotional support; yet the results are reported as emotional support, perineal compresses or massage (not in the KQ), massage (not in the KQ), water birth (not in the KQ), acupressure (not in the KQ), acupuncture (not in the KQ), aromatherapy (not in the KQ), anthem graveolens (not in the KQ), ambulation and positioning, nutritional/oral/parenteral. This is very confusing for the reader and is a "kitchen sink" section.	The supportive care techniques listed in the key question are not designed to be complete, but rather some examples that will be covered. We have however revised the key question to be consistent with the order in which these specific interventions are discussed.
Results	Peer Reviewer #1	On page 75, for KQ4, it appears the question is only about EA, yet look at the 10 comparison questions on pages 75-76. Again considerable inconsistency. The diagnosis of prolonged labor was not the outcome evaluated as specified in the KQ; rather it was mean length of labor stages and cesarean birth - please carefully evaluate what your review is about. This slippage of terminology and outcome measurement is problematic.	The key question provides an overview of the question to be addressed in that section. The more detailed inclusion/exclusion table and text of the section provides the additional detail about included interventions and comparators.
Results	Peer Reviewer #1	The forest plots are sometimes insufficiently labeled, particularly a lack of identification of the unit of measurement, e.g. a mean of what?	We have added additional details regarding the unit of comparison to the forest plot titles.
Results	Peer Reviewer #2	Page 25 Line 20...The odds ratio indicates an increased (as opposed to decreased) risk in perineal trauma, assuming intervention to be the study group.	This has been corrected.
Results	Peer Reviewer #2	There is substantial detail in the report which most Readers will not fully read, but is nevertheless, essential to the report.	Thank you. No response needed
Results	Peer Reviewer #3	Key Question 1 <ul style="list-style-type: none"> Would recommend changing the wording of this question to: "Evaluating differences in outcomes with abnormal labor" or something to that effect. When stating this question is about "criteria used to define abnormal labor," I was expecting a review on how definitions of abnormal labor were chosen (which is well written in the main report) and the evidence (or lack of) behind different time lines (as suggested in the NICHD preventing the first cesarean) being chosen to define abnormal labor. 	Although we clarify in the text following the key question the evaluation of differences in outcomes with abnormal labor we have not modified the specific key question wording.
Results	Peer Reviewer #3	I would consider adding to the recommendations/areas for future research that studies begin to evaluate the impact of utilizing new definitions for failed labor/arrest of labor (as recommended by NICHD) on maternal/neonatal outcomes	We have expanded the discussion of the definition of labor dystocia to the discussion section.

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Results	Peer Reviewer #3	Key Question 2; <ul style="list-style-type: none"> Evidence summary: Would suggest the authors comment about the effect of amniotomy on duration of labor among multiparous women – even if it does NOT shorten labor and/or if there are not enough trials to examine this question, would explicitly state that then. The authors discuss it in the main report but it is not included in the evidence summary. 	The key points for each section have been revised to reflect the updated evidence. The evidence highlighted in the key points and evidence summary reflect those findings with the greatest evidence.
Results	Peer Reviewer #3	How is “early amniotomy” defined? This is not included in the main report either.	We now include in the main report a definition for early amniotomy of receiving amniotomy at 4-5 cm cervical dilation appears
Results	Peer Reviewer #3	Was the outcome of “prolapsed cord” included in these studies and if so, I would report that since that is a clinical concern with amniotomy	Although this was not an outcome included—presumably, prolapsed cord would lead to increased c-section rates, one of the primary outcomes
Results	Peer Reviewer #3	Key Question 3 <ul style="list-style-type: none"> Would recommend the authors give examples of “supportive care measures” in the opening sentence of this question within the evidence summary. 	We now state that supportive care measures included interventions such as continuous emotional support, perineal massage, water birth, acupuncture, ambulation and positioning strategies.
Results	Peer Reviewer #3	What is the rationale for including supportive care in this review? These measures are not typically thought of as interventions/management for labor dystocia or things that might contribute to labor dystocia. I really enjoyed this portion of the review but started to think that perhaps this review is not really just about “labor dystocia” as the title states. Instead it is more about interventions/management strategies that may impact the risk of cesarean delivery among women in spontaneous labor. Not sure if it is worth changing the title or clarifying earlier on. Otherwise, if we want to purely restrict this to labor dystocia, I think this section needs to be limited to the articles that truly evaluated supportive care as a component of labor dystocia and/or the impact on labor dystocia.	As with our response to Reviewer 1, we have clarified that the report covers diagnosis, direct and indirect prevention (i.e., does choice of pain management strategy affect risk of diagnosis of dystocia), and management.

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Results	Peer Reviewer #3	What does “mixed parity” refer to? Is this the same as multiparous? Would clarify that as this is not a standard obstetrical term.	We now clarify that many studies evaluated interventions in nulliparous (women who have not previously given birth) and multiparous women (women who have previously borne more than one child) separately. Other studies did not indicate the women’s parity and were considered to be mixed parity or unspecified parity defined as potentially including both nulliparous and multiparous women.
Results	Peer Reviewer #3	Typo on pg. 108, line 20 – “tow” should be “two”	The typo has been corrected.
Results	Peer Reviewer #3	Key Question 4 <ul style="list-style-type: none"> • Would consider broadening this section to “analgesia” in general and including IV pain medication and potentially moving the acupuncture from the supportive care section to this section. 	We have considered these changes in scope and location of findings but have kept discussion of the acupuncture evidence to the supportive care section of the report.
Results	Peer Reviewer #3	Key Question 5 <ul style="list-style-type: none"> • Loved the idea of including this and think that it should also be listed in the section for future research that should include the utility of frequent/timed cervical exams on diagnosing labor dystocia/intervening and the effects on pregnancy outcomes. 	Thank you, we have added to future research
Results	Peer Reviewer #3	Key Question 7: <ul style="list-style-type: none"> • Would recommend giving the reader information regarding the general definitions of “high” vs. “low” dose oxytocin protocols 	The definition for high and low dose oxytocin protocols varies amongst the included studies. We now list the specific interventions when discussing the included studies.

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Results	Peer Reviewer #3	Would the authors consider changing this question to oxytocin, in general, and then also including things like oxytocin rest or oxytocin “wash out” in labor?	Although we allowed various definitions of high and low dose oxytocin we have not changed the scope of the review to include the other suggested protocols.
Results	Peer Reviewer #3	Why not include nipple stimulation separate from low dose oxytocin?	Nipple stimulation was considered an “endogenous” form of low dose oxytocin, based on suggestion from the TEP
Results	Peer Reviewer #4	The results are presented clearly with appropriate and well-organized details. One exception is the absence of reports of heterogeneity of the studies included in the meta-analyses, which is critical for their interpretation.	Thank you. No response needed
Discussion/ Conclusion	TEP Reviewer #1	Well written.	Thank you. No response needed
Discussion/ Conclusion	TEP Reviewer #1	Two specific comments: (1) The following phrase under Future Research is unclear to me: “parental preference as surrogate for infants” (page 39, lines 19-20). I would consider deleting it.	We have clarified, with references. The key point is that, in terms of evaluating quality of life, parents often provide surrogate values for infants/children, which are inevitably influenced by the role of parent
Discussion/ Conclusion	TEP Reviewer #1	(2) The only section that makes me nervous is the analysis around high- vs low-dose oxytocin. It may be useful to clarify the dosing itself upfront (e.g., page 32, line 40). I am particularly concerned about the conclusion that high-dose oxytocin is associated with a lower cesarean delivery rate than low-dose oxytocin. I see this as a potential flashpoint. Are you confident in the SOE? I would be interested in hearing what other clinicians think of this conclusion.	Incorporating evidence from the search update, the SOE for high dose oxytocin lowering the cesarean delivery rate has been lowered to a low SOE.
Discussion/ Conclusion	TEP Reviewer #2	The findings are appropriately discussed, particularly related to augmentation and what is not known.	Thank you. No response needed

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Discussion/ Conclusion	TEP Reviewer #2	The future research section is clear. However, i am not sure that 'normal' non-augmented labor must be studied among those that have an SVD with a normal outcome. Because cesarean represents human behavior (for the most part), it is unclear to me why women who undergo cesarean for non-labor diagnoses with normal outcomes should not be included. Because I am not sure that the assumption that they are not making normal labor progress is correct.	We agree this is a complex issue. We have added clarification that a primary issue is that the threshold for non-labor indications for c-section may be influenced by duration of labor. .
Discussion/ Conclusion	TEP Reviewer #3	Please see general comments or attached file for comments.	Thank you. No response needed
Discussion/ Conclusion	TEP Reviewer #3	The future research section is outstanding.	Thank you. No response needed
Discussion/ Conclusion	Peer Reviewer #1	The discussion/conclusion is much too broad without an inadequate consideration of the many design limitations of conducting research of the type considered in this review with women during labor. Many of the interventions considered are by their very nature impossible to implement in a blinded fashion or difficult to study via an RCT due to women's preferences.	We have included a discussion of the difficulties of randomization in settings where patients have strong preferences.
Discussion/ Conclusion	Peer Reviewer #1	The interventions evaluated also were NOT to treat dystocia and this critical element is not discussed. We confuse fostering the progress of labor and supporting women's experiences during labor with diagnosis of dystocia. Just because a mean length of labor differs does not = a diagnosis of dystocia.	As previously noted, we have clarified that the review addresses (1) diagnosis, (2) the possible effects of other common labor interventions on the diagnosis of prolonged labor, and (3) strategies (including oxytocin) for treating prolonged labor.
Discussion/ Conclusion	Peer Reviewer #2	I am not aware of exclusion of important studies during the stated study interval.	Thank you. No response needed

Section	Commentator & Affiliation	Comment	Response
Discussion/ Conclusion	Peer Reviewer #2	Page 36 Line 23...In the Discussion it would be helpful to remind the Reader that studies were limited to the years 2005 and 2015, as many Readers will know that related, often quoted, studies have been published before this timeframe.	Through discussion with our technical expert panel we determined the start date of our search. Given changes in many aspects of practice (particularly threshold for C/S), there was concern that earlier studies may not be appropriate and therefore was limited to 2005 onwards. The search has been updated and now includes literature through February 2019. We now include this information in the main report.
Discussion/ Conclusion	Peer Reviewer #2	Page 36 Line 49...To this Reviewer, it would appear that a definition of contemporary “normal labor” is first, dependent upon whether one is describing the statistical boundaries of time in labor, versus maternal-fetal clinical outcomes, or both. If just describing statistically-defined confidence intervals, this would be almost impossible to define, unless a large cohort of women, stratified by independent variables (e.g. age, weight, medical morbidities) could be observed to delivery without any interventions (e.g. oxytocin, AROM, neuraxial anesthesia, etc), which is not likely to be studied due to methodologic and ethical considerations. Every study examining a particular intervention will be affected by a multitude of practices (e.g. oxytocin protocols, intrapartum anesthesia techniques, time-limitations, etc) which will vary across other studies with the same primary outcome.	We have added the following to clarify this: “Ideally, the definition would be derived based on data from a large group of women who were followed without intervention and had optimal maternal and neonatal outcomes, but there are obvious practical and ethical barriers to this.”
Discussion/ Conclusion	Peer Reviewer #2	Page 169 Line 35...A brief explanation of Applicability Ratings here would be helpful to the Reader.	We clarify that applicability ratings provide information as to whether the population, interventions, comparators, outcomes, or settings evaluated in the included studies are applicable to clinical practice and specific key questions of interest.

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Discussion/ Conclusion	Peer Reviewer #3	Discussion <ul style="list-style-type: none"> Findings in relation to what is already known: Pg. 37 – lines 7-9: I disagree with the authors' comment that "our findings... are consistent with current guidelines...allowing longer durations [of labor]." This review does not speak to the length of labor and/or current recommendations/guidelines regarding the utility in differing lengths of labor before performing a cesarean. 	In general, our findings that "normal" labor in modern settings is generally longer than earlier guidance are consistent with current guidelines {American College of Obstetricians & Gynecologists, 2014 #9044}, which are largely informed by the CSL data and encourage allowing longer durations for both first and second stages of labor before intervening with cesarean delivery. However, as noted, over half of women in the "normal" group received augmentation in the CSL data, and the data are not informative about optimal timing of augmentation.:
Discussion/ Conclusion	Peer Reviewer #3	Applicability: Such an important point to highlight – the different locations of the population but also the different demographic makeup of the populations being studied	Thank you
Discussion/ Conclusion	Peer Reviewer #4	The limitations of the studies included in the analysis need to be explored in more detail and their interpretations substantially more cautious. This is mainly driven by the significant limitation of the data available.	Thank you. We have updated the search to reflect additional data and have modified strength of evidence ratings as needed.
Clarity/ Usability	TEP Reviewer #1	The key points (executive summary) is clear and concise. Overall the document is well written. The conclusions do not extend beyond the limits of the data.	Thank you. No response needed
Clarity/ Usability	TEP Reviewer #1	I believe that clinicians will find this useful (e.g., perhaps it will lead to more early amniotomy, which I strongly encourage; I hope it will help to reassure providers and patients that epidural analgesia does not delay delivery or increase cesarean delivery rates).	Thank you. No response needed
Clarity/ Usability	TEP Reviewer #1	The question about high- vs low-dose oxytocin on the cesarean delivery rate is the one area I am concerned about (see comments above).	We have responded to individual concerns listed.

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Clarity/ Usability	TEP Reviewer #2	The report is well-organized and findings clearly stated.	Thank you. No response needed
Clarity/ Usability	TEP Reviewer #2	I am unsure what data exploring defining 'abnormal labor' based on outcomes (and not the CIs around time) were not explored. Or if thought not to be high enough quality, perhaps an important consideration for the discussion?	We did not identify any studies that defined abnormal labor based on outcomes—as noted in KQ1 and the discussion, this is a major limitation of the literature
Clarity/ Usability	TEP Reviewer #3	The reports is well organized.	Thank you. No response needed
Clarity/ Usability	TEP Reviewer #3	The main points are clearly presented	Thank you. No response needed
Clarity/ Usability	TEP Reviewer #3	The conclusions may be more relevant to future research; however, this report has strong potential to highlight for clinicians, policy makers, and funders the current state of labor dystocia science and the to better understand what constitutes normal and abnormal labor so that cesarean section can be reserved for labors with higher risk to lead to suboptimal maternal/child outcomes	Thank you. No response needed
Clarity/ Usability	TEP Reviewer #3	This report importantly synthesizes and compares the labor dystocia literature, providing new understanding of interventions that may help decrease labor dystocia as well as the lack of strong science about many interventions that may or may not decrease labor dystocia.	Thank you. No response needed
Clarity/Usability	Peer Reviewer #1	Although the report is generally well-structured, it has limitations as noted previously, particularly in consistency of the purpose/questions and reporting of results.	Thank you – we believe that the consistency and clarity of the revised report have been improved.
Clarity/Usability	Peer Reviewer #2	Yes to all questions, however there is not much new added to existing systematic reviews, but rather the range of related topics are nicely condensed into one report..	Thank you. No response needed
Clarity/Usability	Peer Reviewer #3	Well structured	Thank you. No response needed
Clarity/Usability	Peer Reviewer #4	The report is very well thought through, constructed and presented. Its conclusions are relevant to both the policy and practice decisions. However, the conclusions need to be stated and interpreted markedly more cautiously due to the substantial limitations of the studies included.	Thank you. We have responded to specific concerns individually below.

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Clarity/Usability	Peer Reviewer #4	1. A large number of conclusions is based on a single RCT. There is a substantial body of experimental and theoretical evidence showing that a large number of RCTs findings cannot be reproduced. Perhaps as many as 30 to 50%. (Ioannidis J. 2005 and 2008)	We agree that findings based on one RCT often have uncertainty and our strength of evidence ratings in these cases is reduced to reflect that limitation.
Clarity/Usability	Peer Reviewer #4	2. Frequently even when multiple RCTs are included in the analysis they are greatly underpowered to evaluate even large effects. For example, 3 RCTs of the effect of early amniotomy on the prevalence of operative vaginal delivery enrolled 611 patients. The study population has only 54% power to detect such large effect as doubling of the rate of operative vaginal deliveries from 4 to 8%.	We have added a discussion about power; power issues are also implicitly addressed in the SOE ratings, which include "precision" as a criterion.
Clarity/Usability	Peer Reviewer #4	3. many meta-analysis combine studies with such high heterogeneity that it prevents their interpretation. For example, Cochrane Database of Systematic Reviews on "Amniotomy for shortening spontaneous labour" shows I2 statistics of the inconsistency of the studies in the meta-analysis of 96% for the shortening of the 1st stage of labor and 71% for the rate of dysfunctional labor. The latter is highly significant but this finding is difficult to interpret in light of such a prodigious heterogeneity.	We have added a statement regarding heterogeneity in the discussion of applicability, since many of the considerations discussed there may well contribute to the heterogeneity.

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