

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	A systematic review and meta-analysis to examine intrapartum interventions, and maternal and neonatal outcomes following immersion in water during labour and waterbirth
AUTHORS	Burns, Ethel; Feeley, Claire; Hall, Priscilla J.; Vanderlaan, Jennifer

VERSION 1 – REVIEW

REVIEWER	Ellwood, David Griffith University, School of Medicine
REVIEW RETURNED	25-Sep-2021

GENERAL COMMENTS	<p>Thanks for allowing me to review your very interesting manuscript, and congratulations on performing such a comprehensive systematic review and meta-analysis.</p> <p>I have only one comment on the data and this may simply require some clarification. I realise that the studies presented are a mixture of those in which water immersion was only in labour, some in labour and for birth, and some for birth only. Yet, for some of the outcomes (e.g. caesarean section) the fact that birth in water occurred would completely preclude the outcome from taking place. Have you defined this outcome in terms of intention to give birth in water or does this outcome only get considered in studies in water immersion for labour only?</p> <p>I note the use of the term delivery to describe birth in several places, and I would encourage you to change this language use so that the birth of a baby is described as a birth.</p> <p>The figures of the Forrest plots do not appear to be labelled in any way so it is difficult to work out to what each one relates. Can this be corrected?</p>
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REVIEWER	Scarf, Vanessa University of Technology Sydney, Centre for Midwifery, Child and Family Health
REVIEW RETURNED	29-Sep-2021

GENERAL COMMENTS	<p>Thank you for the opportunity to review this manuscript. The authors have comprehensively and systematically reviewed studies on the interventions and outcomes for women and newborns following water immersion during labour and birth. The review is well written and the methods are clear. Below are a few comments regarding language and clarity of the content:</p>
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	<p>Introduction: The authors use the term 'delivery' to describe the process of giving birth- it is preferred to use the term 'birth' in most cases. For example, page 4, line 35 "...who use water immersion for labour and birth experience different care practices than women who have standard delivery (birth) care." P4, line 33: women, not woman. P5, line 29: "for labour and/or birth</p> <p>Results: Study description- please be consistent with the numerical results in the brackets. eg. (k=XX, n=XX) Table 1: Please replace delivery with birth in the title. The list of interventions and outcomes in the key stop at 14 and there appears to be 24.</p> <p>Table 4: It is unclear exactly what table 4 indicate. It would be clarified if the title was changed to "Results of subgroup analysis of interventions in an obstetric setting on outcomes of water immersion for Labour and Birth compared to standard care."</p> <p>The results are compelling, however it is difficult to match the text with the figures as the figures do not have labels in this submission. It will be more obvious when the figures are placed within the text (if that occurs), however there is no way at present to determine what variable the forest plots are displaying, except by careful counting of figures and included papers. Also, many of the numerical results in the text do not quite match the numerical results in the forest plots. Eg: Amniotomy OR 0.72 in text, 0.71 in FP. Many of the CIs don't match and the total number of participants also do not match the numbers in the FPs of many of the analyses.</p> <p>The discussion and conclusion/implications for practice highlight important points relating to generalisability and research methods and reporting. This is a valuable piece of work which will contribute necessary evidence to support the use of water immersion for labour and birth.</p>
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REVIEWER	Wand, Handan The Kirby Institute for Infection and Immunity in Society, University of New South Wales
REVIEW RETURNED	22-Dec-2021

GENERAL COMMENTS	<p>The study is well conducted, thorough, and rigorous. The writing is clear and concise.</p> <p>My main comment is regarding the way that the heterogeneity handled and the authors' decision using "random effect model" versus "fixed effect model". The authors report the I² statistic as a measure of heterogeneity; the reported values range considerably, and yet for each one, the authors state that when the heterogeneity is >50% they used the mixed effect model to handle large heterogeneities. However, their mixed models still produced very large I-squared values. The authors made their conclusions using the results with very high I-squared statistics: 84% for Amniotomy, 89% for Augmentation, 93% for opioid use and 91% for pain etc. I think the choice of a fixed-effect vs. random effect model should be based on 2 important factors: whether the included studies are functionally identical, meaning they include similar or nearly identical populations, interventions, and methods, and whether the goal of synthesis of results across studies is to</p>
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	<p>compute a common effect size that is applicable to populations similar or identical to those included but not generalizable to other populations. If these 2 conditions are not met, a random-effects model is appropriate. Please comment.</p> <p>Minor comments:</p> <ul style="list-style-type: none"> • Page 25 line 47: % is missing in I-squared statistics • Page 27, line 14: something is wrong with the p-value? Is it supposed to be 0.325 rather than 325. Please check and correct • Something is wrong with the X-axis scales in all the figures. Why it is extended all the way to 100? I think it is labelled incorrectly; the largest upper bound is only 11.55
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VERSION 1 – AUTHOR RESPONSE

<p>Reviewer 1</p> <p>Prof. David Ellwood, Griffith</p> <p>Cardiff Uni</p>	<p>Thanks for allowing me to review your very interesting manuscript, and congratulations on performing such a comprehensive systematic review and meta-analysis.</p> <p>I have only one comment on the data and this may simply require some clarification. I realise that the studies presented are a mixture of those in which water immersion was only in labour, some in labour and for birth, and some for birth only. Yet, for some of the outcomes (e.g. caesarean section) the fact that birth in water occurred would completely preclude the outcome from taking place. Have you defined this outcome in terms of intention to give birth in water or does this outcome only get considered in studies in water immersion for labour only?</p> <p>I note the use of the term delivery to describe birth in several places, and I would encourage you to change this language use so that the birth of a baby is described as a birth.</p> <p>The figures of the Forrest plots do not appear to be labelled in any way so it is difficult to work out to what each one relates. Can this be corrected?</p>	<p>Thank you.</p> <p>General comment - we could only be certain about the 'planned' water for labour only v waterbirth for the RCTs and to an extent for the retrospective observational studies, which prevented certainty about intended unless it was clearly stated in the paper.</p> <p>We've added a line in the results for caesarean to indicate that all studies reporting analysed either labour only or intended water birth (page 14).</p> <p>Thank you – this has been amended in the script and tables.</p>
<p>Reviewer 2</p>	<p>Thank you for the opportunity to review this manuscript. The authors have</p>	<p>Thank you</p>

<p>Ms. Vanessa Scarf, University of Technology Sydney</p>	<p>comprehensively and systematically reviewed studies on the interventions and outcomes for women and newborns following water immersion during labour and birth. The review is well written and the methods are clear. Below are a few comments regarding language and clarity of the content:</p> <p>Introduction: The authors use the term 'delivery' to describe the process of giving birth- it is preferred to use the term 'birth' in most cases. For example, page 4, line 35 "...who use water immersion for labour and birth experience different care practices than women who have standard <u>delivery</u> (birth) care." P4, line 33: women, not woman. P5, line 29: "for labour and/or birth</p> <p>Results: Study description- please be consistent with the numerical results in the brackets. eg. (k=XX, n=XX) Table 1: Please replace delivery with birth in the title. The list of interventions and outcomes in the key stop at 14 and there appears to be 24.</p> <p>Table 4: It is unclear exactly what table 4 indicate. It would be clarified if the title was changed to "Results of subgroup analysis of interventions in an obstetric setting on outcomes of water immersion for Labour and Birth compared to standard care."</p> <p>The results are compelling, however it is difficult to match the text with the figures as the figures do not have labels in this submission. It will be more obvious when the figures are placed within the text (if that occurs), however there is no way at present to determine what variable the forest plots are displaying, except by careful counting of figures and included papers. Also, many of the numerical results in the text do not quite match the numerical results in the forest plots. Eg: Amniotomy OR 0.72 in text, 0.71 in FP. Many of the CIs don't match and the total number of participants also do not match the numbers in the FPs of many of the analyses.</p> <p>The discussion and conclusion/implications for practice</p>	<p>Corrected</p> <p>Thank you, this has been corrected.</p> <p>Thank you. This has been corrected.</p>
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	<p>highlight important points relating to generalisability and research methods and reporting. This is a valuable piece of work which will contribute necessary evidence to support the use of water immersion for labour and birth.</p>	
<p>Reviewer: 3 Dr. Handan Wand, The Kirby Institute for Infection and Immunity in Society</p>	<p>The study is well conducted, thorough, and rigorous. The writing is clear and concise.</p> <p>My main comment is regarding the way that the heterogeneity handled and the authors' decision using "random effect model" versus "fixed effect model". The authors report the I² statistic as a measure of heterogeneity; the reported values range considerably, and yet for each one, the authors state that when the heterogeneity is >50% they used the mixed effect model to handle large heterogeneities. However, their mixed models still produced very large I-squared values.</p> <p>The authors made their conclusions using the results with very high I-squared statistics: 84% for Amniotomy, 89% for Augmentation, 93% for opioid use and 91% for pain etc. I think the choice of a fixed-effect vs. random effect model should be based on 2 important factors: whether the included studies are functionally identical, meaning they include similar or nearly identical populations, interventions, and methods, and whether the goal of synthesis of results across studies is to compute a common effect size that is applicable to populations similar or identical to those included but not generalizable to other populations. If these 2 conditions are not met, a random-effects model is appropriate. Please comment.</p> <p>Minor comments:</p> <ul style="list-style-type: none"> • Page 25 line 47: % is missing in I-squared statistics • Page 27, line 14: something is wrong with the p-value? Is it supposed to be 0.325 rather than 325. Please check and correct • Something is wrong with the X-axis scales in all the figures. Why it is extended all the way to 100? I think it is 	<p>Thank you. The reason for this decision has been clarified in the methods on page 10.</p> <p>Thank you, these have been corrected.</p> <p>Thank you for this comment. We kept the scale the same for each analyses to allow comparison of the magnitude between studies and analyses. Though this scale is large, it is not adequate to encompass all outcomes as Figure 6 has one study that is not fully captured in the visible scale.</p>

	labelled incorrectly; the largest upper bound is only 11.55.	
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VERSION 2 – REVIEW

REVIEWER	Scarf, Vanessa University of Technology Sydney, Centre for Midwifery, Child and Family Health
REVIEW RETURNED	21-Feb-2022

GENERAL COMMENTS	These revisions have clarified and strengthened the paper.
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REVIEWER	Wand, Handan The Kirby Institute for Infection and Immunity in Society, University of New South Wales
REVIEW RETURNED	28-Feb-2022

GENERAL COMMENTS	My queries/questions are
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