

<b>Article details: 2018-0033</b>	
Title	Effect of an innovative care model, The MONARCH Centre, on decreasing postpartum length of stay: an interrupted time series study
Authors	Ghislain Hardy MD, Jo Ann Colas Msc, Deborah Weiss PhD, David Millar MD, Alan Forster MD, Mark Walker MD, Daniel J. Corsi PhD
<b>Reviewer 1</b>	Innie Chen
Institution	Department of Obstetrics and Gynaecology, University of Ottawa, Ottawa, Ont.
General comments (author response in bold)	<p>I enjoyed reading this interesting article studying the effect of an innovative care model on decreasing postpartum length of stay in hospital. Article seems to address majority of the specific questions asked by the editors of the journal. However, I request authors to address the following before the manuscript is accepted for publication:</p> <p><b>We thank the reviewer for these helpful comments. Please see below our point-by-point response to each of the reviewer comments.</b></p> <ol style="list-style-type: none"> <li>Supplemental tables A and B, shows a statistically significant increase in readmission of babies while comparing pre- and post-implementation periods, among both vaginal and Cesarean deliveries. Authors have failed to show the statistical significance testing for the difference in tables/results, and also ignored this issue in discussion; while highlighting "no difference in readmission" based on original trial (instead of data of this study). I suggest to perform suitable statistical analysis, adequately report the findings in results section and then handle the issue appropriately in discussion. <b>We have included the appropriate statistical analyses to describe the trends in readmission rates for mothers and babies. The findings indicate an increase in readmission among babies from 0.6% to 1.5% (p=0.018) in cesarean deliveries and from 1.4% to 2.1% in vaginal (p=0.016) and we have highlighted this finding in the abstract and results (pages 8-9) and discussion (pages 11-12, 13) instead of the previous data on readmission.</b></li> <li>Table 1 may be replaced with merged supplemental table A and B (with some tests of significance), which are much more informative and are in line with study objectives <b>We have replaced table 1 with a modified and merged version of supplemental table A and B as suggested.</b></li> <li>The LOS change observed in pre- and post- implementation is primarily observed in data presented in table 5, and has been generalized in discussion giving an impression as it has been observed across all type of analyses. Though authors have strong support from data for success of MONARCH for (vaginal/caesarean deliveries for typical cases; decrease in percentage of postpartum LOS greater than 48 hours; and effect of MONARCH at 1.5 years against predicted model without MONARCH) but the discussion and conclusion tend to generalize the finding without highlighting/refuting the significance of findings which do not conform to generalized intent. I suggest to give these non-conforming findings due weightage in discussion and conclusion. <b>We have revised the discussion to give equal weighting to all analyses and to draw a balanced conclusion.</b></li> <li>Page 14, line 42: Evidence of safety and high level of satisfaction is based on an older trial data and not from this study and may please be excluded from conclusion; as this study provide some evidence of increased re-admission among babies. <b>We agree and have removed the data on level of satisfaction which was obtained in the previous study. We have discussed the issue of readmission among mothers and babies (see pages xx-xx)</b></li> <li>There is some confusion about the implementation period and post implementation follow-up period as written in manuscript <ol style="list-style-type: none"> <li>Page 8, line 24: pre-implementation period ends on December 31, 2014</li> <li>Page 7, line 29: Implementation period starts on January 1, 2014</li> <li>Page 7, line 29: Implementation period ends on July 31, 2015</li> <li>Page 7 line 35: Post implementation follow-up starts on June 1, 2015</li> <li>Page 8 line 26: Post implementation follow-up starts on August 1, 2015</li> </ol> <p>Authors should update the dates, without confusing overlapping periods resulting from typos <b>We made corrections to the dates above as indicated.</b></p> </li> <li>Page 12, line 17: "not associated" is repeated <b>Corrected.</b></li> </ol>
<b>Reviewer 2</b>	Erin Graves
Institution	Institute for Clinical Evaluative Sciences, Toronto
General comments (author response in bold)	<p>Comments to the Author</p> <p>Overall, I think this is a strong and important study.</p> <p>I have a few small changes to suggest:</p> <p>(1) Page 6 - the OR for client satisfaction that you report is not within the CI reported. Please double check. <b>Corrected.</b></p> <p>(2) Page 6 - you report the percent differences that are almost identical in magnitude (4 vs 6%) but interpret them completely differently. Please provide the p-values if this is why these are being interpreted differently, or else explain more fully. <b>We have edited and removed this statement from the revised introduction.</b></p> <p>More broadly:</p> <p>(3) One of your primary conclusions was that this program had high client satisfaction rates during your pilot work, yet details were not provided on this within the paper besides a brief reference to another paper and a citation. To include this as a primary conclusion, I would prefer more discussion of this survey and the results, potentially tying it into the discussion beyond simply the conclusions. <b>We agree and have removed these statements from the primary conclusions as they refer to previous work, the present study did not have data on the client satisfaction rates. This has been revised in the discussion (see page 13).</b></p> <p>(4) I found the tables provided in the supplementary appendix (stratified by time frame) to be much more informative than the current Table 1, as the allow the reader to understand the data and periods that were used. I would recommend replacing Table 1 with Table A from the appendix. <b>We agree and have made this change as requested. The new table 1 incorporates tables A and B as suggested by the previous reviewer.</b></p>

	<p>(5) While I appreciate the value of the control analyses run, I'm not sure they added much value to the interpretation of the project. I wouldn't remove them, but I think a stronger control (or additional) analysis would be one focused on safety. For example, readmissions were mentioned as a key metric of the 'safety' of this program and running the same ITS methodology to look at maternal readmissions across this time period would strengthen the conclusion that this program is safe while simultaneously working as a control. There may be other indicators of safety that are important to look at instead of readmissions as well.</p> <p><b>We have undertaken additional analyses of the readmission rates for mothers and babies. Due to the small sample sizes and low rates of readmission (&lt;2%) it was not sufficient data to run an ITS analysis however we have looked at statistical analyses of the readmission rates and observed a small increase in rates in babies which we have given discussion to (page 8-9, 11-12).</b></p> <p>(6) In the limitations you mention that this was hard to assess because of the varying complexity of obstetric cases. However, given that you used a regression model for the ITS, there should be a way to build in a complexity covariate that count account for this. I would suggest considering this addition to further strengthen your analysis and conclusions.</p> <p><b>This is a well-taken comment. However, within the dataset the level of complexity of the cases was not available and a validated rule to assign level of complexity to the cases was beyond the scope of this study. We have tried to address this limitation by stratification by typical cases which removed some cases which we determined may be more complex due to transfers, longer admission, or other complications and we have discussed this in the revised manuscript (see page 12).</b></p>
<b>Reviewer 3</b>	Anick Berard
<b>Institution</b>	Centre de recherche du CHU Sainte-Justine, Montréal, Que.
<b>General comments (author response in bold)</b>	<p>Comments to the Author Comment Very clearly written manuscript.</p> <p>In this manuscript, the interrupted time series regression models were used to assess changes in postpartum length of stay (LOS) between 1/1/2012 and 31/12/2016, covering 16,023 deliveries. At 1.5 years following the introduction of the community-based postpartum clinic, the average LOS following a cesarean birth was reduced by 20 hours (27% reduction, 95% CI: 9.5 to 30.4); for vaginal deliveries LOS was reduced by 6 hours overall (18% reduction, 95% CI: 5.2 to 31.1). Within health care expenses, hospital and in-patient care represents the largest proportion and giving birth is the most frequent reason for inpatient hospitalization. This study may have the potential for considerable cost savings in total health expenditure.</p> <p>Text: 2917 words, 5 tables, 3 figures. CMAJ Open word limit: 2500 Specific comments ABSTRACT Interpretation: Lines 42-44: "A strong association was found between declines in postpartum LOS during and following the introduction", your data did show "A association was found between declines in postpartum LOS and the introduction", remove the "during and" <b>We have revised this accordingly.</b></p> <p>Lines 47-50: no data from the current study show that "This alternative model of postpartum care is safe, has high patient satisfaction", which should be removed from the abstract. <b>This has been removed.</b></p> <p>Similarly in the Concluding remarks page 13 lines 36-49: remove "during and" and remove "safe, has high patient satisfaction". <b>These remarks have been removed.</b></p> <p>INTRODUCTION The introduction is too long (4 paragraphs, 680 words). <b>We have reduced the introduction in the revised manuscript to 2 paragraphs, xx words.</b></p> <p>METHODS The timeline: Pre-implementation period: January 1, 2012 to December 31, 2013 Implementation period: January 1, 2014 to July 31, 2015 Post-implementation period: August 1, 2015 to December 31, 2016. <b>We have corrected inconsistencies in the time periods, see page 5, page 6.</b></p> <p>Page 5 line 19 "following the introduction of the MONARCH centre in July 2014", while the implementation period should be January 1, 2014 to July 31, 2015. <b>This has been corrected.</b></p> <p>Page 6 lines "post-implementation follow-up between June 1, 2015 and December 31, 2016....", while the post-implementation period should be August 1, 2015 to December 31, 2016. <b>Corrected</b></p> <p>Page 7 lines 24 "in the pre-implementation (January 1, 2012 – December 31, 2014) segment.....", while the pre-implementation period should be January 1, 2012 to December 31, 2013. <b>Corrected</b></p> <p>However, the study period Page 2 line 22: "over a five-year period..." Page 6 line 19: "cover a five-year period..." Page 12 line 17: "over a four-year period..." Should be over a five-year period, right <b>These were corrected.</b></p> <p>Outcome Page 6 lines 38-49:</p>

	<p>The primary outcome was postpartum LOS in hours. Record-level data was aggregated to four week intervals (which accounts for differing number of days in months) for time-series analyses (over a total study duration of 48 months). As a secondary outcome, we examined the proportion of mothers having a postpartum LOS of greater than 48 hours.</p> <p>However, in your RESULTS</p> <p>Page 8 lines 27-28 "Rates of 30-day readmission were 2.0% and 1.1% for mothers and 1.0% and 1.8% for babies among cesarean and vaginal births, respectively. and lines 45-49 "Rates of readmission varied over the study period and in the post-intervention period; these were 2.4% and 1.1% for mothers and 1.5% and 2.2% for babies among cesarean and vaginal births, respectively." they were not included in the primary or secondary outcome, may be should be removed.</p> <p><b>We have decided to retain the readmission outcomes as these were noted as important by the previous reviewers and have adjusted the methods to include these as secondary outcomes related to safety.</b></p> <p>Page 9 lines 35-54 were described the result of Figure 2. Please add Figure 2 in somewhere of this paragraph.</p> <p><b>This has been added.</b></p> <p>Page 10 line 44 should be Figure 3, not Figure 4, there were only 3 Figures in this manuscript.</p> <p><b>This has been corrected.</b></p> <p>DISCUSSION</p> <p>Page 11 line 17, there were two "was associated".</p> <p>The comment on Concluding remarks has been listed at the beginning of the comment under the subheading of the ABSTRACT, specifically, Interpretation.</p> <p><b>This has been corrected.</b></p>
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