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### ARTICLE DETAILS

<table>
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<th>TITLE (PROVISIONAL)</th>
<th>Caesarean Sections and For-Profit Status of Hospitals: Systematic Review and Meta-analysis</th>
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<tbody>
<tr>
<td>AUTHORS</td>
<td>Hoxha, Ilir; Syrogiannouli, Lamprini; Luta, Xhyljeta; Tal, Kali; Goodman, David C; da Costa, Bruno; Jüni, Peter</td>
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### VERSION 1 - REVIEW

<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Paulo Fontoura Freitas</th>
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<tbody>
<tr>
<td>REVIEWER Location</td>
<td>Universidade Federal de Santa Catarina - UFSC - Brasil</td>
</tr>
<tr>
<td>REVIEW RETURNED</td>
<td>31-Aug-2016</td>
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</table>

### GENERAL COMMENTS

Reasons behind the almost worldwide increasing rates of caesarean sections remains a important and permanent debate. The presented meta-analysis was directed to a issue of extreme relevance and calls attention to the paradox of differences in cesarean rates in favor of the private system, among women with better social conditions, amongst which it would be expected a lower obstetric risk.

The authors performed an extensive review and statistical methods were properly applied.

Even taking into account this as “the first meta-analysis to address the association of CS rates with for-profit status of hospitals”, questions can be raised concerning specific points:

1) Authors conclusions pointing to financial incentives as a definitive "causal mechanism explaining the observed association: It has been shown that the method of compensation, by itself, is not a determining factor for the high prevalence of cesarean section in our midst, and that the whole situation is much more complex than this, including local and cultural factors influencing the degree of flexibility of medical judgment concerning clinical criteria for cesarean sections, difficult to be measured as part of a meta-analysis.

2) Implications for policy making focused mainly on financial issues: Reimbursement policies favoring normal delivery has already showed to cause only a temporary halting.

3) What this study actually adds in terms of the current knowledge concerning the issue of an association between private practice and higher rates of caesarean sections?: An extensive literature worldwide has showed consistence in this direction and, there seems to be an agreement about this association.

<table>
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<tr>
<th>REVIEWER</th>
<th>Mark Smith</th>
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<td>REVIEWER Location</td>
<td>Senior Director</td>
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<td>REVIEWER Location</td>
<td>Truven Health Analytics</td>
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<td>REVIEWER Location</td>
<td>USA</td>
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<td>REVIEW RETURNED</td>
<td>01-Nov-2016</td>
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</table>
This is a clearly written description of a meta-analysis on an interesting topic. I have a few requests for clarification and minor edits:

* Page 7, line 49: the phrase "the odds of receiving CS was 1.41 higher" is missing a word. Should it be "1.41 times higher"?

* Page 11, Implications for Policy Making: I think it's unfair, at least in the US context, to say that clinical guidelines are unclear. The American College of Obstetrics and Gynecology (ACOG) has published guidelines that offer as much clarity as the evidence base will allow (one example: http://www.acog.org/Resources-And-Publications/Committee-Opinions/Committee-on-Obstetric-Practice/Cesarean-Delivery-on-Maternal-Request). For the same reason, I would not state that the field wants for clear and evidence-based clinical guidelines. The evidence base is weak, as ACOG notes, but they draw what conclusions from it they can. The guidelines may allow for clinical judgment, but I would not label that a lack of clarity.

* Methods (pp. 4-5): I think it would be useful to explain how 'planned' and 'emergent' are defined. There should be little room for choice over birth method if the c-section is truly emergent. If so, then financial incentives should make no difference, and the observed difference in emergency CS rates would simply reflect different caseloads. The authors should give an argument about why emergency c-sections should be included in a study about financial incentives.

* In the Discussion section, please comment on the change observed over time in Figure 2. It looks like the OR is much lower in recent years than it was earlier. Why might that be? Is it consistent across countries?

* Do the authors know by how much (what proportion) the average payment for CS exceeds the average payment for vaginal birth in the 8 countries represented in Table 1? A difference of 100% should have a stronger impact than a difference of, say, 20%. It should be easy to find a rough approximation for the US, but maybe not for all of those countries.

---

**REVIEWER**

Gillian Worthy
Kleijnen Systematic Reviews
UK

**REVIEW RETURNED**

22-Nov-2016

---

This is a clear and well-reported systematic review using appropriate methodologies. I only have a few minor comments.

1. The flow diagram of the review process currently in Appendix 1 would be better in the main figures.
2. In the searching section of the methods please specify that full details are given in appendix 10.
3. Why was your primary outcome the adjusted OR? Adjusted for what, anything or specific covariates? Please add more details. Does this mean that studies not reporting an adjusted analysis were excluded?
4. Was there a quality assessment? If not then add a reason.
5. in the analysis did you also consider I-squared values as a
measure of heterogeneity
6. In the methods it would help to list the categories used in the stratification e.g study design (RCT, cohort etc).
7. "among 11 studies reporting adjusted estimates the median number adjusted for was 8 (range 2 to 124). Is 124 correct that seems like a lot of factors to adjust for!? How big was the study?

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Comment 1: Authors conclusions pointing to financial incentives as a definitive “causal mechanism explaining the observed association: It has been shown that the method of compensation, by itself, is not a determining factor for the high prevalence of cesarean section in our midst, and that the whole situation is much more complex than this, including local and cultural factors influencing the degree of flexibility of medical judgment concerning clinical criteria for cesarean sections, difficult to be measured as part of a meta-analysis.
Response: We agree that the role of financial incentives may vary greatly by country. In the first submission, we attempted throughout the text to reflect that observed association is a “likely” indication of the role of financial incentives and not a definitive causal relationship. For example, in the abstract, we write: – “CS are more likely to be performed by for-profit hospitals… Since financial incentives are likely to play an important role,". We have changed the statement reading “Financial incentives are the most likely causal mechanism behind the observed association” to “Financial incentives are likely to contribute to the observed association” to address this reviewer's comment (Page 10, paragraph 2 of the manuscript)

Comment 2: Implications for policy making focused mainly on financial issues: Reimbursement policies favoring normal delivery has already showed to cause only a temporary halting.
Response: We revised the sentence “Changing reimbursement policies so that vaginal deliveries and CS are paid similarly could keep overall payments to physicians and hospitals approximately constant without encouraging unnecessary CS.” to “Changing reimbursement policies so that vaginal deliveries and CS are paid similarly could keep overall payments to physicians and hospitals approximately constant without encouraging unnecessary CS but won’t guarantee the elimination of overuse.” (Page 12, paragraph 1 of the manuscript)

Comment 3: What this study actually adds in terms of the current knowledge concerning the issue of an association between private practice and higher rates of caesarean sections?: An extensive literature worldwide has showed consistence in this direction and, there seems to be an agreement about this association.
Response: We have revised last sentence on first paragraph “Our estimates of a 41% increase in adjusted odds of CS associated with for-profit status of hospital has a similar or larger magnitude than the associations found for the characteristics above, and are therefore obviously relevant for both clinical and policy decision making.” to “Our estimate of a 41% increase in adjusted odds of CS associated with for-profit status of hospital has a similar or larger magnitude than the associations found for the characteristics above and therefore appears relevant for both clinical and policy decision making. Our systematic review indicates agreement across 17 studies performed in seven countries as to the direction of this association, even though the magnitude of the association shows some variability.” (Page 9, paragraph 2 of the manuscript)

Reviewer: 2
Comment 1: Page 7, line 49: the phrase “the odds of receiving CS was 1.41 higher” is missing a word. Should it be “1.41 times higher”?
Response: We revised this (Page 8, paragraph 2 of the manuscript).
Comment 2: Page 11, Implications for Policy Making: I think it's unfair, at least in the US context, to say that clinical guidelines are unclear. The American College of Obstetrics and Gynecology (ACOG) has published guidelines that offer as much clarity as the evidence base will allow (one example: http://www.acog.org/Resources-And-Publications/Committee-Opinions/Committee-on-Obstetric-Practice/Cesarean-Delivery-on-Maternal-Request). For the same reason, I would not state that the field wants for clear and evidence-based clinical guidelines. The evidence base is weak, as ACOG notes, but they draw what conclusions from it they can. The guidelines may allow for clinical judgment, but I would not label that a lack of clarity.
Response: We agree. We deleted this sentence. (Page 12, paragraph 1 of the manuscript)

Comment 3: Methods (pp. 4-5): I think it would be useful to explain how 'planned' and 'emergent' are defined. There should be little room for choice over birth method if the c-section is truly emergent. If so, then financial incentives should make no difference, and the observed difference in emergency CS rates would simply reflect different caseloads. The authors should give an argument about why emergency c-sections should be included in a study about financial incentives.
Response: Thank you for addressing this point. When re-discussing the single study initially classified as having emergency CS as an outcome (Korst et al 2005), we found our classification misleading. We revised it to distinguish between 'Indication for CS established before labour (i.e. planned CS); Indication for CS established during labour; and Any CS irrespective of indication. Korst et al 2005 is now classified as Indication for CS established during labour.

Comment 4: In the Discussion section, please comment on the change observed over time in Figure 2. It looks like the OR is much lower in recent years than it was earlier. Why might that be? Is it consistent across countries?
Response: We agree the OR is lower in recent years, but the test for trend across pre-specified periods was not significant and therefore we were careful in not over-interpreting this finding.
To address this reviewers comment, we included the following statement in the discussion section: “Even though, a test for trend across periods of data collection was negative, we found the association between for-profit status of hospitals and odds of CS less pronounced in recent years. In view of the negative test for trend, this could be a chance finding. Alternatively, this reflects attempts of care providers and policy makers to attenuate raising CS rates over time.” (Page 9, paragraph 1 of the manuscript)
In response to the reviewer’s question if trends are consistent across countries, we would like to highlight that for adjusted outcomes, which were the primary outcome measure, only US and France had studies in at least two periods of data collection. As shown in Figure 1, in the US, Braveman et al. reported an adjusted OR of 1.89 (private for-profit vs public non-profit) and an adjusted OR of 1.41 (private for-profit vs private non-profit) for the year 1991. Korst et al reported an adjusted OR of .35 (private for-profit vs public non-profit) for the year 1995 and Coonrod et al reported an adjusted OR of 1.13 (private for-profit vs private non-profit) for the year 2005. Huesch et al. reported an adjusted OR of 1.21 (private for-profit vs public non-profit) for the year 2010. Based on these numbers, there is no clear secular trend in the US. In France, Naidich et al reported an adjusted OR of 1.35 (private for-profit vs private non-profit) for the year 1991. Carayol et al A reported an adjusted OR of 1.30 (private for-profit vs public non-profit) for the period between 1995 to 2004. Carayol et al B reported an adjusted OR of 1.38 (private for-profit vs public non-profit) for the years 2001-2002. Finally, Coulm et al reported an adjusted OR of 1.35 for the year 2010. Again, there is no secular trend to be observed within France.

Comment 5: Do the authors know by how much (what proportion) the average payment for CS exceeds the average payment for vaginal birth in the 8 countries represented in Table 1? A difference of 100% should have a stronger impact than a difference of, say, 20%. It should be easy to find a rough approximation for the US, but maybe not for all of those countries.
We agree that this information would be important and may add value to interpretation of results. We
have found data only for the US, see http://transform.childbirthconnection.org/resources/datacenter/chargeschart/statecharges/.
For example, for 2011, the average charges in the US for non-complicated vaginal births performed in hospital were 10,657 USD while charges for non-complicated CS were 17,859. This is 67.6% difference.

Unfortunately, data on the comparative cost of CS and vaginal deliveries are incomplete or unavailable, see http://www.who.int/healthsystems/topics/financing/healthreport/30C-sectioncosts.pdf. In particular, the cost of vaginal deliveries appears unavailable for all countries, except the US.
To address this reviewer’s comment, we have added the following sentence in the discussion: “Health insurers can encourage overprovision of CS as they tend to reimburse hospitals and physicians better for CS than for vaginal delivery (11, 43, 44).” (Page 11, paragraph 1 of the manuscript)

Reviewer: 3
Comment 1: The flow diagram of the review process currently in Appendix 1 would be better in the main figures.
Response: We made this change and reflected changes of sequence in the text of manuscript as well as numbering of files.

Comment 2: In the searching section of the methods please specify that full details are given in appendix 10.
Response: We made this change to take into account reviewers comment and added a sentence that reads “Full details are given in Web Appendix 4.” (Page 5, paragraph 1 of the manuscript)

Comment 3: Why was your primary outcome the adjusted OR? Adjusted for what, anything or specific covariates? Please add more details. Does this mean that studies not reporting an adjusted analysis were excluded?
Response: We clarified this in the methods section and revised sentence “The pre-specified primary outcome was the adjusted OR of births delivered by CS in private for-profit hospitals as compared with public or private non-profit hospitals.” to “The pre-specified primary outcome was the OR of births delivered by CS in private for-profit hospitals as compared with public or private non-profit hospitals adjusted for confounding factors as specified by individual investigators.” (Page 5, paragraph 2 of the manuscript)

Web Appendix 4 presents the characteristics that estimates were adjusted for. We clarified study inclusion as follows. “Studies were included if they reported data on either primary or secondary outcome.” (Page 5, paragraph 2 of the manuscript)

Comment 4: Was there a quality assessment? If not then add a reason.
Response: Since included studies neither were randomised trials of interventions, nor non-randomised studies of interventions, we refrained from performing any formal quality assessment over and above the classification of study design (cross sectional versus retrospective cohort study) used in Figure 2 and an extraction of the types of data used in individual studies as reported in Web Appendix 3. To our knowledge there is no agreed quality assessment tool for the types of studies included in this systematic review.

Comment 5: in the analysis did you also consider I-squared values as a measure of heterogeneity.
Response: We pre-specified the quantification of heterogeneity through tau squared since I-squared estimates would be uniformly high because of the high precision of included studies. In this situation, the use of tau-squared is more informative. (See: Rucker G, Schwarzer G, Carpenter JR, Schumacher M. Undue reliance on I(2) in assessing heterogeneity may mislead. BMC Med Res Methodol. 2008;8:79.)

Comment 6: In the methods it would help to list the categories used in the stratification .e.g study
design (RCT, cohort etc).
Response: We made the changes to provide the list for each stratification category and the revised sentence reads “We conducted analyses stratified by study design (cross sectional versus retrospective cohort study), national CS rates (moderate, high, very high), period of data collection (up to 1994, between 1995 to 2004, 2005 and later), parity (primi and multiparae combined versus primiparae only), history of previous CS, and type of CS analysed (indication for CS established before labour (i.e. planned CS), indication for CS established during labour, any CS irrespective of indication) to investigate potential reasons for between-study heterogeneity and used chi-square tests to calculate p-values for interaction, or tests for linear trend in case of more than two ordered strata.” (Page 6, paragraph 2 of the manuscript)

Comment 7: “among 11 studies reporting adjusted estimates the median number adjusted for was 8 (range 2 to 124). Is 124 correct that seems like a lot of factors to adjust for!? How big was the study?
Response: It is correct. (See Supplementary Table 4, Model XIII of the included article: Huesch MD, Currid-Halkett E, Doctor JN. Measurement and risk adjustment of prelabor cesarean rates in a large sample of California hospitals. Am J Obstet Gynecol. 2014;210(5):443 e1-17.) The study included 408355 cases/deliveries.

### VERSION 2 – REVIEW

| REVIEWER                      | Paulo Fontoura Freitas  
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<td>13-Jan-2017</td>
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| GENERAL COMMENTS | In my opinion, after the corrections made by the authors, the article should be accepted for publication |

| REVIEWER                      | Mark Smith  
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<tr>
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| GENERAL COMMENTS | The authors have addressed my questions. I have no other comments. |
Caesarean sections and for-profit status of hospitals: systematic review and meta-analysis
Ilir Hoxha, Lamprini Syrogiannouli, Xhyljeta Luta, Kali Tal, David C Goodman, Bruno R da Costa and Peter Jüni

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