Reviewer’s report

Title: Trend and socio-demographic differentials of Caesarean section rate in Addis Ababa, Ethiopia: Analysis based on Ethiopia demographic and health surveys data

Version: 2
Date: 4 February 2014

Reviewer: Fernando Volpe

Reviewer’s report:

The author has made a considerable effort to improve the manuscript, and that should be acknowledged. Describing the trends of obstetric procedures, and analyzing their determinants, is indeed an important scientific contribution.

However, this is mostly a descriptive study, and this is how it should be valued. Inferences and projections don’t fit properly in this case, considering the rationale and the limitations of this study.

I agree with the editor’s recommendation that the analyses of the risks of early breastfeeding should be suppressed. I suggest the author removes all related to this topic from this paper, and prepare another short communication to present those results separately. Again, this is a descriptive study, not an outcome study. Insisting in retaining this outcome analysis would rise a different line of questioning, regarding the choice of this and not other outcomes, or even its very validity as an indicator of quality of care. So it is clear to this reviewer that this topic should be addressed separately and more thoroughly in a separate paper.

The main arguments presented attain to the notion that CS rates over 15% are “unjustifiable”, in “excess”, not “acceptable”, not “tolerable”. I have purposely stressed these expressions as they are really spread all over the paper. However, the author himself states that “the appropriate range for the CS rate is debatable”. If it is debatable, then why using so many judgmental expressions in a scientific paper? It expresses the authors beliefs and wishes, rather than the actual state of knowledge on this so debated subject. This is mostly what I called “biased interpretation” in my previous review. So, the challenge posed to the author is to rewrite every judgmental statement, using instead a neutral attitude and language style.

And, regarding misinterpretation of references, I am quite sure I did not “availed empirical evidences on the importance of CS for the reduction of maternal and neonatal mortality especially in countries with high mortality levels”, neither my findings showed that high CS rates “could cause negative maternal and child health consequences”. Also, it is inappropriate to cite “7. Althabe F, Belizán JM: Caesarean section: the paradox. Lancet 2006,368:1472-1473.” when stating that “the World Health Organization (WHO), considers 5-15% as the optimum range”. Again, when citing Villar et al. (2006), the author misinterpreted their findings –
they are not associated to “unjustifiably high CS rates”, since main findings relate to (and I quote the original paper) “c) elective, if decision to do the operation was made before onset of labour and the woman was referred either from an antenatal clinic or a high-risk ward (if the timing of the decision was unclear, we did not identify as elective those caesareans done in women whose labour had been induced or those done in women who received anaesthetic during a spontaneously initiated labour).” – also, Villar et al. findings do not directly test the 15% argued threshold, on the contrary, they treated this variable as continuous. In the same line, the author cites Lauer et al. (2010) as the source for the statement that “According to the WHO, the CS rate in any population should lie within the range of 5-15% and rate exceeding 15% is medically unjustifiable” – again, this is just not there! The original 10-15% range was determined by a consensus at the Joint Interregional Conference on Appropriate Technology for Birth, held in Fortaleza, Brazil, 1985, and was not a result of an evidence based formal study – this document should be the correct reference. So I believe the appropriate recommendations for improving this manuscript would be: correct all biased and misinterpreted citations, don’t rely blindly on (and repeat) authoritative opinions, and question every number with science.

Now, back to the merit of methods and findings:
There is an important issue on the methodology of data gathering, which is only the last birth in 5 years was considered. There could be a selection bias, since parity could be different among subgroups and influence CS rates. That should be controlled for, or at least acknowledged as a limitation.

I can’t see a reason not to perform multivariate analyses of the determinants of mode of delivery. This should be added to the paper and the main results be rewritten according to the new findings. Attention should be paid to multicollinearity, however, and the correlation matrix should be presented or described as well. This addition would improve substantially the relevance of the results and the scientific merit of the paper. This is THE major issue, those are THE major findings, they surely deserve more refinement.

The author states that “The available sample size might be inadequate to determine the CS rate for each of the data points included in the study.” But in the methods he says “Post hoc power calculation via StataSE 11 showed that the sample is adequate to do the analysis with 99% confidence level and 90% power.” This is confusing to the reader. Is the sample representative or not? Perhaps if the author shows that post-hoc power tests for subgroup analyses are appropriate, this question would be dismissed. Anyway, please clarify.

In methods, “The three datasets were downloaded from Measure DHS website and analyzed using SPSS 20.0 for windows. “ is an expression difficult to follow. Perhaps simply removing it would be more adequate.

In results, please correct the notation of small P-Values (P<0.001 instead of P=0.000).

The 1.6% CS rate annual increase reported was the figure estimated by the
model? If so, please clarify that in results, since the actual variation was approximately 1.3%/year.

The projection “If the current progression is sustained, by 2025 the rate might get as high as 50%.” is bold, and not evidence based. There are not grounds to predict the evolution of the rates over time, since other models (not linear) could also fit the data and were not formally compared. Please remove this phrase.

In Discussion, the author states that private institutions perform more CS in order to maximize their revenues. Despite this could be true for some cases, this statement is pejorative and could be even aggressive. Other factors, not financial, could also be involved, like the satisfaction of their clients as a natural consequence of the clients demands. Culture and women empowerment are possibly also involved in this decision making process. The author could explore this point a little more and enrich the discussion.

When discussing about medical costs, there is an issue that deserves consideration: being costly is not necessarily a bad thing. Maybe sometimes it is costly because it is better. In the author’s statement, there is an underlying (and reference based) notion that this cost could be prevented, since those CS are “excessive”. But, in Addis-Ababa, this cost was attributed to the private sector, so, ultimately, it was a consequence of free initiative and market laws. Should that private cost be prevented either?

I missed a discussion on how may the low CS rates in the less privileged population is a problem in Ethiopia. Frankly, I feel this should be the main concern, not the opposite. I tend to be less interested on what rich families of Addis-Ababa are willingly doing with their money, than what the poorer are not receiving when they should be. But, this is my personal (not scientific) opinion, so feel free to disregard this point.

Regarding language style, many colloquial expressions persist: increased dramatically; of enormous significance; was extremely high; etc. Please rephrase, being very careful with scientific style.

In the Conclusion, the author classifies the CS rates as epidemic. The term is pejorative, as it is originally associated with disease. Why not just conclude that rates have been rising? The last paragraph shows recommendations that are not based on results. Rewrite the conclusion as to simply summarize the descriptive information.

I hope the comments have been helpful not only to improve the manuscript, but also to provoke some philosophical and scientific reflections.

I must now modify my statement of no conflict of interest, since I have some ideological positions that may contradict the ones presented in this paper, and have myself published on these grounds.

Level of interest: An article whose findings are important to those with closely
related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

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