Author's response to reviews

Title: Drivers and Deterrents of Facility Delivery in Sub-Saharan Africa: A Systematic Review

Authors:

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Reviewer's report
Title: Drivers and Deterrents of Facility Delivery in Sub-Saharan Africa: A Systematic Review
Version: 1 Date: 27 April 2013

Reviewer #1: MARTEY
Comment #1: Discussion, third paragraph, last sentence: "And while socioeconomic status is generally seen as universally important to facility-based delivery rates, why does it appear to be less important in countries with higher literacy rates?" This sentence does not read well.
Response: Thank you. The sentence was changed to read:

“And while socioeconomic status is seen as inextricably linked to facility-based delivery rates, why does SES appear to be less important in countries with higher female literacy rates?”

Comment #2: Discussion, paragraph 5, line 8: "For example, in this review 11 studies relying upon household data found that antenatal care use, frequency, and perceived quality is associated with a greater likelihood of facility-based delivery”. The 'is' in this sentence should be replaced with 'are'.
Response: Thank you. This was corrected.

Reviewer #2: Dako-Gyeke
Comment #1: One would expect the word “Factors” to have been included in the key search terms. Unfortunately, it is not part of the search terms. This might have led to exclusion of some studies that focused on factors that influenced FBD. Although it is mentioned in passing under “Study selection and data extraction” it is not included in the search terms. If it is an oversight, author must include it or state specifically why use of this word (“factors”) was avoided.
Response: This is a good point. Unfortunately, this was an oversight and the term ‘factors’ was not included in the search strategy. However, the search strategy was sufficiently comprehensive that it included blanket searches of maternal health services / utilization in developing countries, thus the addition of the term ‘factors’ would not have expanded the number of articles retrieved but
instead would have limited them. In subsequent searches, the term ‘factors’ would have been a good one to combine with determinants and predictors.

Comment #2: It is of concern that qualitative studies were totally excluded from this systematic review:

Comment 2a: First, the second aim of this paper is to document research designs and data collection methodology used to explore factors associated with FBD. In this case exclusion of qualitative studies does not allow the study to make conclusive comments on diverse methodological approaches used.

Response: There was much discussion about whether to include or exclude qualitative studies in this review. It has been argued that true “associations” can only be determined statistically, and thus this review should be limited to empirical (read: quantitative) data and statistically assessed associations. However, we agree with this reviewer’s point of view and have expanded mention of qualitative research in the discussion.

Comment 2b: Second, the results clearly indicate that exclusion of qualitative studies led to total disregard of cultural influences that mediate and shape FBD in Africa. This is because cultural factors are adequately studied through use of qualitative approaches. It is recommended that author comments on some of the cultural influences on FBD under the Discussion by referring to few qualitative studies such as Bazzano et. al (2008) that used ethnographic data.

Response: See response to 2a above.

Comment #3: Usually, systematic reviews have limitations that the author must acknowledge. Author must include a paragraph that highlights limitations such as use of articles published in English and only within a limited time period. The author must acknowledge that excluding articles that do not satisfy these criteria might have influenced type of articles pulled, and findings.

Response: Thank you for this. The following paragraph was added.

Despite its strengths, this review has several limitations worthy of note. First, the review was limited to articles published between 1995 and 2011. It is possible that having broadened the years of publication, the results may have been slightly different. Second, the review was limited to articles published in English and available via English-language search engines. This is an important limitation, given the number of Francophone countries in Africa and the likelihood that research coming from those countries may tell a very different story than those coming from English-speaking nations. By design, this review also focused upon quantitative studies that could provide statistical assessments of associations. The results may have been different – albeit perhaps more difficult to compare – if qualitative studies were also included in the assessment. This review was conducted by a small team of researchers, which may have affected the interpretation. The author was assisted in creating and implementing the search strategy by a master’s trained global health librarian, and the quality of the articles was judged by the author and a master’s level research associate. This small team was efficient, but it is possible that a larger team may have interpreted the literature slightly differently. Finally, this review consciously excluded qualitative manuscripts in order to focus on empirically testable
quantitative data. It is possible that including qualitative manuscripts would change the overall conclusions drawn in this review.

Reviewer #3: Nikiema

Comment #1: It is recommended to involve a minimum of two researchers at all stages of a systematic review in order to minimize bias and errors. Please describe the review team and indicate how disagreements between raters have been solved.

Response: This manuscript was developed with the assistance of a global health librarian for the systematic search strategy, as well as a research associate who was not initially included in the authorship list. However, upon going back and instituting article-by-article quality scores and needing to talk through the aspects of the revisions required, the research associate was added to the authorship list.

The following text was added in the methods section to reflect this partnership:

Each research study was coded independently by each author based on a modified version of the STROBE statement. The STROBE statement was used to develop a scale ranging from 0-34 points that covered such areas as study background, objectives, design, setting, participants, data collection methods, variables assessed, analysis methods, reporting of results, discussion of relevance to other literature, discussion of limitations, and inclusion of implications. Each item was scored on a 0-2 scale (0=not included/addressed, 1=somewhat included/addressed, 2=clearly included/addressed). Total scores for each paper were compared across authors and averaged. Discrepancies of more than 4 points were discussed and consensus reached. Final averaged scores were divided into tertiles to determine the strength of the evidence, with those scoring in the lowest tertile providing ‘weak’ evidence, those scoring in the middle tertile providing ‘moderate’ evidence, and those in the top tertile providing ‘strong’ evidence.

Comment #2: One characteristic of a systematic review is that it focuses on all high quality research evidence on the review question. Please explain how the quality of the selected studies have been assessed and how it impacted the decision-making on the accuracy of the presented evidence.

Response: The quality of the selected studies was assessed as described above. Note that only those studies which were judged to be moderate or strong were included in Table 5.

Comment #3: “Original research” is one of the inclusion criteria. Therefore the rational for including review articles is thus unclear. Please further clarify your decision to include them instead of (or in addition to) working with the original articles listed in these review papers.

Response: Thank you for this. In revising the manuscript, review articles were removed.

Comment #4: The review was restricted to articles published in English language. This language restriction may lead to “language bias” and should be
addressed in the discussion.
Response: Thank you for this. This was included in a paragraph of limitations at the end of the discussion section.

Comment #5: Selected studies should be described, ideally in a table, with authors, sample size, design & method, and, if available, assigned rates after quality assessment.
Response: With 65 studies included in this review, a comprehensive table is approximately 10 pages long. It was not included in the first submission due to concerns about space constraints. However, it is included in the revision as Appendix A.

Comment #6: The reason for not performing meta-analysis is not convincing. The author could define more rigorous inclusion criteria (including methodological criteria) and restrict the meta-analysis to a sub-sample of studies meeting those criteria.
Response: The purpose of this review was to provide a comprehensive examination of the factors associated with facility-based delivery in sub-Saharan Africa. Where meta-analyses are often the most useful is in trying to determine the cumulative impact of an intervention, or to determine the outcomes associated with different treatment arms. In such cases, the studies to be combined need to be similar enough in both assessment methods and in population characteristics to make valid comparisons. In this review, however, the studies in question are extremely heterogeneous. They run the gamut from facility-based to population-based to samples that were purposely selected. In addition, data collection ranges from cross-sectional chart reviews to facility-based surveys to periodic health and demographic surveillance surveys. So not only is there no effect size to measure if a meta-analysis were to be performed on these data, but the data are so disparate as to raise questions about the validity of combining them into a meta-analysis.

That said, the STROBE Statement (Strengthening the Reporting of Observational studies in Epidemiology, available at http://www.strobe-statement.org/index.php?id=strobe-home) was consulted, as was the MOOSE checklist for reporting of meta-analyses of observational studies (Stroup et al., 2000), to ensure that quality of studies was being assessed appropriately. And pursuant to this comment and the one from Reviewer #4 below, a quality checklist was implemented and each included manuscript was subjected to quality review by two researchers who discussed each article and assigned it a final score. The text of the methods was amended on Pages 6 and 7.

Comment #7: In the seventh paragraph under the discussion section the author states that “this systematic review of the literature builds upon the previous reviews in several important ways”. However, only one way (the first) was described.
Response: This paragraph was expanded as follows:
This systematic review of the literature builds upon the previous reviews in
several important ways. First, it focuses entirely on sub-Saharan Africa, explicitly including African journals. This is a departure from previous reviews. Thaddeus and Maine’s 1994 review, while generally focused on maternal mortality in Africa, included articles from Central and South America and across Asia and the Middle East. (Thaddeus and Maine, 1994) Similarly Say and Rayne’s 2007 review included only 8 articles from Africa, and Gabrysch and Campbell’s 2009 review – which was based upon Thaddeus and Maine’s and Say and Rayne’s reviews – included studies across Latin America, Asia, and the Middle East. While such inclusivity might have been helpful at a time when there was comparatively little written about barriers to facility delivery, it is not nearly as useful today in planning interventions that speak to the local context. The review presented here focuses exclusively on the issues pursuant to the sub-Saharan African context, something that has been sorely missing in the published literature. In addition, this review sought to include original research from the African sub-continent that was not published in mainstream western literature. This has complicated the search strategy for this review, and admittedly, it has increased the variability of the quality of studies reviewed. However, many of the articles retrieved from the African journals included in this search have shed valuable light on the phenomenon of facility-based delivery that might have otherwise gone unnoticed.

Reviewer #4: Hanson

Comment #1: Assessment of the quality of care of the studies, and if possible a meta analysis for selected high quality studies (e.g. the 43 using multivariate analysis?) for common predictors such as age, education, parity...

Response: Thank you for this. Please see response to Reviewer #3, Comment 6.

Comment #2: The introduction should develop more clearly what this review wants to add compared to previously published reviews.

Response: Given space limitations, the introduction was kept short. However, the following text was added:

Three previous reviews of the literature have addressed facility-based delivery[1-3], yet none were systematic, comprehensive, and focused on sub-Saharan Africa. The first review was not systematic, was conducted nearly 20 years ago, and the bulk of its references come from the mid 1980s.[1] This review addressed the factors that influenced the delay in deciding to seek care, the delay in getting to a health facility, and the delay in obtaining adequate care. The authors suggest that distance, cost, and quality of care are not sufficient to predict service utilization – other factors such as illness severity and socioeconomic status influence service use. This review resulted in what has come to be known as the Three Delays Model, perhaps one of the most commonly utilized conceptual frameworks in the maternal mortality literature. The second review focused on quantitative assessments of the impact of maternal health interventions on utilization.[2] Included in the review were a total of 30 quantitative studies from around the world, only 8 of which included data from Sub-Saharan Africa. Say and Raine concluded that there is enormous variability in maternal health service utilization, and that utilization appears to be extremely dependent upon contextual factors.[2] The third review centered its assessment
on references identified in the previous two reviews.[3] The authors used the literature to categorize determinants of facility-based delivery into four main themes: sociocultural factors, perceived benefit or need of skilled attendance, economic accessibility, and physical accessibility.[3] The authors conclude from their review that most research downplays perceived need and physical accessibility as significant barriers. Note that this review was not limited to any geographic region or any specific year range.

Given inherent differences between sub-Saharan Africa and much of the rest of the developing world, a review that explicitly focuses on sub-Saharan Africa is critical. In addition, a reconsideration of the domains of influence is also overdue. Thaddeus and Maine[1] see delays in care seeking as the crux of the issue around facility delivery. Say and Raine [2] do not posit a framework for understanding delivery location. Gabrysch and Campbell[3] see accessibility factors (including perceived need) and sociocultural factors as the most important drivers of decision making. This review attempted to explore the research literature in Africa to revisit the potential domains of influence over delivery location in sub-Saharan Africa.

Comment #3: A summary of the search strategy should be presented in the main body of the paper.
See Page 5.

Comment #4: The omission of articles published in French is a limitation
Response: Thank you for this. This was added to a paragraph describing study limitations at the end of the discussion.

Comment #5: A paragraph on assessment of the quality of the articles need to be included as well as a flow chart depicting the exclusion of articles step by step
Response: A description of the assessment of the quality of the articles was included on Pages 6 and 7. Figure 1 illustrates the exclusion of articles step by step.

Comment #5: Criteria for inclusion are mentioned in a vague manner. In result the statement in the results section “upon closer inspection, another 97 were removed” is difficult to follow.
Response: This was reworded as follows:
1,168 citations were identified, of which 123 were retrieved for full-text review. Most of the 1,045 eliminated were excluded due to western setting, lack of original data, or a primary outcome measure other than place of delivery. Of the 123 articles retrieved for full-text review, an additional 43 studies were identified by searching the references, most of which were published in non-indexed, regional journals. Thus a total of 166 articles were identified for full text review. Upon reviewing the full text, another 93 were removed for such reasons as being conducted outside sub-Saharan Africa, place of delivery not being the primary outcome, not including original data, using primarily qualitative methods, not being peer-reviewed, or full text not being available. This left 65 published
studies that met all inclusion criteria and for which data were extracted. (See Figure 1.)

Comment #6: Use of check-list as recommended when presenting a systematic review might help:
http://www.cochrane.org/about-us/evidence-based-health-care/webliography/books/reporting#m
Thank you. A modified checklist was created as described on Page 6 and 7.

Comment #7: No information is given on number of participants, setting, ...
Results:
Response: We are assuming you are referring to the detailed information about each of the 65 studies included? This is included in Appendix A.

Comment #8: The description “regional health and demographic surveillance data” is unclear and the source needs to be better explained and referenced
Response: This was reworded as follows:
…and 6 that relied upon regional Health and Demographic Surveillance Site data, which are collected at least twice per year from small research outposts responsible for tracking the health and demographics of a surrounding population catchment area.

Comment #9: Table 1 needs to be referenced. Also in the text references are missing to identify the respective studies.
Response: Table 1 is referenced on Page 8. All named studies have been referenced in the text.

Comment #10: Table 2: More detailed information is needed, eg. Adjustment for parity typically increases the effect of maternal age on uptake. Mixing results from univariate and multivariate analysis misses important findings.
Response: This review attempted to illustrate the breadth of evidence, including both univariate and multivariate analyses. In some cases univariate analysis was the best evidence available. In other cases both univariate and bivariate analyses were available. We believe that including both is important in that it allows the reader to judge for themselves what may be important factors for future study, as well as what has already been examined in multiple ways.

Comment #11: Direction of influence should not be left blank
Response: Thank you. The cells in question were populated.

Comment #12: The categorisation of individual, social, ANC and facility factors would need to be introduced in the introduction and method section (conceptual framework).
Response: Thank you. Text was added on Pages 5 and 7 developing the need for a reconceptualization of the categories from previous reviews.

Comment #13: Table 5: it is unclear what table 5 presents.
Response: The following text was added:

Table 5 illustrates those studies in which multi-variate models explored some or all of those factors, indicating the consistency of the findings across studies and across models. (See Table 5.)

Comment #14: The discussion fails to summarise the main findings of the study in a concise manner, but is mainly concerned with the limitations of the studies presented. Although the discussion is interesting and well written, it helps little to reflect the findings of the review as such.
Response: Thank you. We have expanded the discussion per this comment.

Comment #15: The issue of who factors might be categorised is taken up in the discussion, but as mentioned before, should be presented earlier in the method section to describe the rational.
Response: Thank you. See Comment #12 above.

Comment #16: The conclusion that e.g education and parity is most strongly and consistently associated with facility delivery cannot be concluded from this review as the meta analysis or at least a description of finding of each study is missing. This is what the reader would like to know, and where the systematic review would add to knowledge very much needed.
Response: A table summarizing each study was added per this comment and others. We believe that after reviewing 65 studies in detail and exploring all multivariate models presented in those studies, we can safely say that in the studies published to date, maternal education, parity / birth order, rural / urban residence, household wealth / socioeconomic status, distance to the nearest facility, and number of antenatal care visits are the factors most consistently associated with facility-based delivery. However, we are very careful to point out that this may be a result of researchers' heavy reliance on household surveys, cross-sectional data, and self-reported results from individual women. Results may be very different if a broader lens was used to examine antecedents to delivery location.