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- Comments from the reviewers and editors (email to author requesting revisions)
- Response from the author (cover letter submitted with revised manuscript)*

*The corresponding author has opted to make this information publicly available.

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Questions about these materials may be directed to the Obstetrics & Gynecology editorial office: obgyn@greenjournal.org.
RE: Manuscript Number ONG-19-1849

Social Determinants of Pregnancy-Related Mortality and Morbidity in the United States: A Systematic Review

Dear Dr. Wang:

Your manuscript has been reviewed by the Editorial Board and by special expert referees. Although it is judged not acceptable for publication in Obstetrics & Gynecology in its present form, we would be willing to give further consideration to a revised version.

If you wish to consider revising your manuscript, you will first need to study carefully the enclosed reports submitted by the referees and editors. Each point raised requires a response, by either revising your manuscript or making a clear and convincing argument as to why no revision is needed. To facilitate our review, we prefer that the cover letter include the comments made by the reviewers and the editor followed by your response. The revised manuscript should indicate the position of all changes made. We suggest that you use the "track changes" feature in your word processing software to do so (rather than strikethrough or underline formatting).

Your paper will be maintained in active status for 14 days from the date of this letter. If we have not heard from you by Nov 28, 2019, we will assume you wish to withdraw the manuscript from further consideration.

REVIEWER COMMENTS:

Reviewer #1: Wang et al performed a systematic review of the association between social determinants of health and pregnancy-related morbidity and mortality in the United States. This is a critically important question and very relevant for the Green Journal audience given that the US ranks among the worst for maternal mortality in relation to other high income countries. The authors synthesized the current literature on the impact of social determinants on pregnancy-related mortality and morbidity and used the WHO Conceptual Framework on SDH to identify potential areas of research, and clinical implications. Given the heterogeneity of the methodologies in studies, they are unable to perform a meta-analysis and instead present their results in a thoughtful, qualitative way.

Introduction:
- Would be helpful to provide specific estimates on severe maternal morbidity and mortality in the US compared to other countries as background

Methods:
- The authors appropriately followed the PRISMA statement and provide their search strategy as well as review strategy.

Results:
- Their search yielded 199 studies with a 96.3% agreement between reviewers.
- The authors categorized studies by outcomes and type of SDH studied including pregnancy-related death, SMM, direct maternal morbidity and emergent hospitalizations or readmissions. They appropriately excluded pregnancy-associated death that was not pregnancy related in order to focus on opportunities for public health and clinical intervention.
- The results section is well organized with a general overview of their findings and description of breakdown of section. The authors organized this section based on the WHO's Conceptual Framework, which provides a convenient and easy to follow flow for the reader.

Table:
- Providing some estimate on magnitude of differences in Table 2 would be helpful. The increasing or decreasing arrows seem oversimplified.

Discussion:
- The authors organize their discussion into identifying gaps in the literature, challenges and clinical implications. They provide a thought-provoking, insightful discussion of their findings and do a nice job of identifying future work that needs to be done.
- For first gap: are there any identified studies that do address the role of racism and / or residential segregation and the impact on SMM or mortality? As this is the biggest finding from the review, it would be helpful to summarize even smaller
Reviewer #2: This systematic review covers the breadth of literature drawing connections between the social determinants of health and maternal morbidity and mortality. As we strive to understand the drivers of maternal morbidity and mortality, and, importantly, the disparities in these outcomes, a full accounting of the impact of social determinants of health will be crucial.

I have the following comments and clarifications:

1) Abstract:
   a. Line 50: Based on my understanding of the study as described in the body of the manuscript, this is a descriptive analysis rather than truly qualitative. Systematic review using a qualitative approach conveys to this reader, at least, that the results would then include content analysis for themes, or might consist of something of a "meta-ethnography." Rather, this study represents a descriptive accounting of the available quantitative literature on the subject. Perhaps my concern can be chalking up to semantics, but for this qualitatively and quantitatively trained reviewer, it was a bit jarring to see "qualitative analysis" in this context. I would recommend altering this phrase throughout to "descriptive."
   b. Line 52-53: While I recognize the ultimately descriptive nature of this systematic review, the subjective phrase "found substantial evidence" does not provide the reader an understanding of the way in which the evidence is "substantial." I would recommend a more quantitative descriptive approach to this claim, perhaps with proportions of articles with positive findings in the various arenas. Lines 52-58 would benefit from putting the statements in numerical/quantitative context (e.g., of the ** SDH categories we assessed, only ** were present in the reviewed literature rather than "literature is limited in the diversity of SDH studied").

2) Introduction
   a. Line 75 - not only are SDH "also ... drivers of maternal mortality," but they shape the risk for the more proximal drivers noted in lines 73-74. It would be worth drawing this connection in the prose as you point to the figure which so nicely lays it out.
   b. Relatedly, a reference to Geoffrey Rose's "Sick individuals and sick populations," the seminal public health publication in 1985 that drove public health professionals to ask "What puts people at risk of risks (e.g., what makes certain individuals more likely than others to have hypertension, or obesity)" and sparked wider focus on the social determinants of health, and to Marmot and Wilkinson's seminal 1998 work "Social Determinants of Health" (published by the WHO) (or some of Marmot's earlier work 1991-1995), might be appropriate as the authors are describing/defining social determinants of health. If space for references is at issue, the single reference to the WHO Commission publications will suffice, but I do feel it is reasonable to bring these key publications to the attention of a clinical audience.
   c. The urgency and importance of the review is clearly laid out in the Introduction, with a clear purpose statement.

3) Methods:
   a. Excellent that the study is registered in PROSPERO! I have a clarification, however, as the registered PROSPERO protocol explicitly states that qualitative studies will be included. In contradiction, the body of the Methods section (line 112-113) and Appendix 2 (inclusion/exclusion criteria) state that qualitative studies were excluded. Why the change? Please address why qualitative studies were ultimately excluded. This is especially intriguing given that no additional quantitative analyses on data extracted from the studies were completed.
   b. Line 108 - Librarian consultant - excellent.
   c. Figure 2 - Because this figure provides data on included/excluded studies and reasons for exclusion, it should be the first portion of the Results section, rather than methods. In addition, the phrasing of the box "Records after duplicates removed" makes it sound like that is the number remaining after duplicates are removed which becomes clear is not the case when the box records screened has an n of 9835. I would recommend simplifying the descriptor of that box to "Duplicate titles removed," or removing that box and modifying the Records screened to "Records remaining after duplicates removed."
   d. Line 120-123: Excellent use of multiple reviewers.
   e. Line 125 - 129: Does this paragraph imply that studies rated poor by the NIH Tool were not included? If not, how is this represented in Figure 2? For those not familiar with the NIH tool, a description of the questions used in this tool (in Appendix 3 or otherwise) would be very useful.
   f. In this reviewer's opinion, since the search strategy provided in Appendix 1 is also provided in the PROSPERO link, Appendix 1 is somewhat redundant/unnecessary. It might be more helpful if among the appendices there were a table providing the details of the NIH Quality Assessment Tool (answers to each of the questions for each of the studies). This would be invaluable to understanding the overall quality of the literature available (see, for example: https://www.mdpi.com/2072-6643/10/5/555/s1). This is the observational/cohort study equivalent of the detailed assessment of bias recommended by the PRISMA guidelines for randomized intervention trials. The Cochrane Collaboration suggests that a single summary score of poor/fair/good is insufficient to judge the bias or quality of a study (e.g.,

- Would refrain from "first" statements unless can be supported by literature search.
- I applaud the authors for calling awareness to the fact that clinicians themselves may be part of a system that might sustain health inequities through differential care. While this is a hard notion to recognize, it is very important and further work to better characterize this is desperately needed.

-Appraise the authors for calling awareness to the fact that clinicians themselves may be part of a system that might sustain health inequities through differential care. While this is a hard notion to recognize, it is very important and further work to better characterize this is desperately needed.
https://guides.library.cornell.edu/systematic_reviews/bias). Frankly, I am skeptical that no studies were rated "poor" among the 85 that met the specified inclusion criteria. Therefore, it is imperative to understand how the reviewers rated each study on the individual questions of the quality assessment tool. The table or Appendix should be included or referenced in the Results.

g. One surprising omission from the organizational structure is the impact of substance use disorder. Did the authors find any articles related to this? In most classification schemes, substance use disorder morbidities are considered "pregnancy-associated," however, substance use disorder, as with the frequent co-morbid mental health disorders, are frequently exacerbated by pregnancy. Substance use disorder, recovery, and addiction are also strongly tied to structural social determinants of health (the authors note this in relationship to mental health, suicide, and homicide, but again substance use is left out, on Line 449). Many maternal deaths are tied to substance use disorder and mental health disorders (see, for example, Smid et al 2019 - which is out of the time scope of this review, I believe), and it would be important to include this as a category even if there are no studies in the authors' time frame. It is an important gap in the literature as the opioid epidemic expands.

4) Results
a. Figure 2 should be referenced with Lines 133-134.
b. The section "Organization and definitions" seems as though it belongs in the Methods section rather than Results. I would remove this, tighten it, and place it in a similarly-titled section in Methods.
c. Line 145-146: I recognize that the Callaghan 2012 reference is the peer-reviewed publication of the CDC's criteria for severe maternal morbidity. However, given that many now know these criteria as the CDC's rather than as "Callaghan's" it may be worth noting somewhere that this definition is the one utilized by the CDC.
d. Lines 282-284: It is unclear to me what is meant by "statistical significance depended on the sample." Do the authors mean wider or narrower confidence intervals (e.g. precision of the estimate), or a higher or lower p value (which is less useful) or a larger or smaller odds ratio? Or do the authors mean some studies had statistically significant findings, where others did not? It would be helpful to be more precise here to provide an understanding of the literature.

e. Line 454: Results are "descriptive" in nature.

5) Discussion
a. Line 330: Again, the use of "substantial evidence" is somewhat ambiguous. Is the weight of this evidence created simply by the number of papers? The effect size or precision? Some clarity here would be useful.
b. Overall, the discussion is well structured and the gaps are clearly delineated. As mentioned in the Methods section, I would like to see some commentary on the contribution of SDH and substance use disorder.
c. Line 401- 410: The limited study designs gap is an important one. However, the authors steal their own thunder by hinting at the types of complex study designs that are required for these studies at the ends of the two prior sections - for instance, hierarchical multilevel models, policy-related regression discontinuity approaches, life-course approaches (longitudinal latent class models or growth models), structural equation modeling, etc. etc. I agree that there is a definite need to improve the complexity of these studies to account for the complexity of data. However, it would be helpful for the authors to lay out more of a roadmap with some specific methodological approaches.
d. One element missing from this discussion are the often low odds ratios that are achieved when studying the distal/structural social determinants of health. Though there has been much debate in the observational and cohort study literature about "pseudo-epidemics" and false results associated with low odds ratios, it is worth noting that these multi-level, complex risk factors frequently have low odds ratios because the effects are indirect (with multiple other steps in the pathway) and the data are messy. In this reviewer's opinion, the low odds ratios are less an indicator of no effect, but a call for replication. The complexity of the data (even when perfectly collected) will remain, and therefore replication will lend robustness to the literature. Multiple studies showing similar effect sizes and direction but low odds ratios lend credibly to the associations between large structural risk factors and outcomes. It would be helpful for the authors to draw attention to this, as skeptics of SDH work frequently cite low odds ratios among the reasons to discount the importance of SDHs compared to more proximal factors.
e. Line 454: Results are "descriptive" in nature.

Reviewer #3: The authors present a systematic review evaluating social determinants of morbidity and mortality in pregnancy in the US. The following items should be addressed:

1. Results - it would be useful for the reader to know what the sources of the data were for the studies you evaluated. Were these data obtained from birth certificates or other such large databases? Single institution data? Later on the authors refer to state-level data or otherwise, but it would be helpful to have this information presented sooner and in one cohesive paragraph.

2. Line 225 - it would be helpful to list here the number of women involved in the comparison, since it would provide a more useful context for the reader to know if the quantities of women in those studies with no difference were approximately the same as those with a difference.

3. Discussion line 330 - the way the sentence is written indicates that Hispanic ethnicity is associated with higher rates of maternal mortality and morbidity, however the authors elsewhere showed some evidence for lower rates for Hispanic women (line 196-198, reference 32, 33). Please revise.
4. Discussion/Clinical Implications - the authors correctly state in line 433 that more SDH and interventions should be studied, however the authors should temper statements regarding interventions or changes in practice as a result of this study (line 429). This study design does not lend itself to recommendations for provision of clinical care; the subsection should perhaps be re-titled.

5. Figure 1 - the arrow leaving Health Behaviors on the left side ends without a conclusion. This is also the case for the arrow leaving stress/psychosocial factors on the right.

STATISTICAL EDITOR COMMENTS:
The Statistical Editor makes the following points that need to be addressed:

lines 133-134 and Fig 2: It is not clear how the studies met inclusion (n = 199) fits within the counts cited on lines for agreements and conflicts. Need a more complete flow diagram. How many of the n = 86 (line 50) vs n = 85 (Fig 2) included in the final analysis were initially agreed upon vs how many were initially disputed by the two primary researchers?

Results (General): The studies are summarized by outcomes and by SEP, Insurance, Education etc. But it seems that quantitative results are not specifically cited. It might be useful to either enlarge those summaries (? actual Tables of results or ranges) or limit the presentation to more qualitative research, citing themes or areas that require more research. The present format seems neither qualitative nor quantitative in approach.

EDITOR COMMENTS:
1. Thank you for your submission to Obstetrics & Gynecology. In addition to the comments from the reviewers above, you are being sent a notated PDF that contains the Editor’s specific comments. Please review and consider the comments in this file prior to submitting your revised manuscript. These comments should be included in your point-by-point response cover letter.

***The notated PDF is uploaded to this submission's record in Editorial Manager. If you cannot locate the file, contact Randi Zung and she will send it by email - rzung@greenjournal.org.***

- We no longer require that authors adhere to the Green Journal format with the first submission of their papers. However, any revisions must do so. I strongly encourage you to read the instructions for authors (the general bits as well as those specific to the feature-type you are submitting). The instructions provide guidance regarding formatting, word and reference limits, authorship issues, and other things. Adherence to these requirements with your revision will avoid delays during the revision process, as well as avoid re-revisions on your part in order to comply with the formatting. The Precis should be no more than 25 words.

- Isn’t your study to look at whether SDH actually do influence rates of pregnancy complications, etc? If so, then line 85 should read ..."may experience......"

- line 88: define social autopsy

- line 126: Do you mean all studies that met the inclusion criteria?

- Line 133: for Clarity, please put the first sentence here which reports the final number of included articles AFTER the second sentence which reports the total #, etc

- Line 143: Some authors have argued that suicide might be a considered a pregnancy associated death given the risks of suicide associated with post partum depression. Perhaps worth a note of this in your discussion. SDH's have certainly also been credited with risk for PP Depression.

- Line 138: please give a definition of direct maternal morbidity

- Line 167: you mentioned above that you only included those w/ Good or Fair ratings so not necessary to repeat here.

- Line 177-180: For the sections on Race/Ethnicity and Socioeconomic, Political and Cultural context, you
described how the factors were associated with maternal death or SMM. In this section on Socioeconomic position, you just tell us that these were the most commonly studied, but don't give us any information about the results of the studies. I’m not sure how your paper unfolds from here, but these paragraphs should have parallel construction, including results of the studies.

- line 190: "Higher excess" seems redundant. Are they higher risks or excess risks?

- Line 204: Please tell us in the methods section how you arrived at these Odds Ratios. Please also include confidence intervals.

- Line 273: Assault during delivery? I've never read a paper to my recollection on assault during delivery. This doesn't seem like SDH as much as a trauma event.

- Line 316: Do you mean hospitals with the highest quartile of proportions of minorities? I'm thrown by the top 25% with highest proportion--that seems to be redundant.

2. The Editors of Obstetrics & Gynecology are seeking to increase transparency around its peer-review process, in line with efforts to do so in international biomedical peer review publishing. If your article is accepted, we will be posting this revision letter as supplemental digital content to the published article online. Additionally, unless you choose to opt out, we will also be including your point-by-point response to the revision letter. If you opt out of including your response, only the revision letter will be posted. Please reply to this letter with one of two responses:
   A. OPT-IN: Yes, please publish my point-by-point response letter.
   B. OPT-OUT: No, please do not publish my point-by-point response letter.

3. As of December 17, 2018, Obstetrics & Gynecology has implemented an "electronic Copyright Transfer Agreement" (eCTA) and will no longer be collecting author agreement forms. When you are ready to revise your manuscript, you will be prompted in Editorial Manager (EM) to click on "Revise Submission." Doing so will launch the resubmission process, and you will be walked through the various questions that comprise the eCTA. Each of your coauthors will receive an email from the system requesting that they review and electronically sign the eCTA.

Please check with your coauthors to confirm that the disclosures listed in their eCTA forms are correctly disclosed on the manuscript's title page.

4. Figure 1: Has this figure been previously published elsewhere? If yes, please provide a copy of the original source.

Tables, figures, and supplemental digital content should be original. The use of borrowed material (eg, lengthy direct quotations, tables, figures, or videos) is discouraged, but should it be considered essential, written permission of the copyright holder must be obtained. Permission is also required for material that has been adapted or modified from another source.

Both print and electronic (online) rights must be obtained from the holder of the copyright (often the publisher, not the author), and credit to the original source must be included in your manuscript. Many publishers now have online systems for submitting permissions request; please consult the publisher directly for more information.

When you submit your revised manuscript, please upload 1) the permissions license and 2) a copy of the original source from which the material was reprinted, adapted, or modified (eg, scan of book page(s), PDF of journal article, etc.).

5. As of January 1, 2020, authors of systematic reviews must prospectively register their study in PROSPERO (https://www.crd.york.ac.uk/PROSPERO/), an international database of prospectively registered systematic reviews. Please refer to the PROSPERO registration number in your submitted cover letter and include it at the end of the abstract.

6. Standard obstetric and gynecology data definitions have been developed through the revITALize initiative, which was convened by the American College of Obstetricians and Gynecologists and the members of the Women's Health Registry Alliance. Obstetrics & Gynecology has adopted the use of the revITALize definitions. Please access the obstetric and gynecology data definitions at https://www.acog.org/About-ACOG/ACOG-Departments/Patient-Safety-and-Quality-Improvement/revITALize. If use of the revITALize definitions is problematic, please discuss this in your point-by-point response to this letter.

7. Because of space limitations, it is important that your revised manuscript adhere to the following length restrictions by manuscript type: Review articles should not exceed 25 typed, double-spaced pages (6,250 words). Stated page limits include all numbered pages in a manuscript (i.e., title page, précis, abstract, text, references, tables, boxes, figure legends, and print appendixes) but exclude references.

8. Specific rules govern the use of acknowledgments in the journal. Please note the following guidelines:
   * All financial support of the study must be acknowledged.
   * Any and all manuscript preparation assistance, including but not limited to topic development, data collection, analysis, writing, or editorial assistance, must be disclosed in the acknowledgments. Such acknowledgments must identify the
entities that provided and paid for this assistance, whether directly or indirectly.

* All persons who contributed to the work reported in the manuscript, but not sufficiently to be authors, must be
acknowledged. Written permission must be obtained from all individuals named in the acknowledgments, as readers may
infer their endorsement of the data and conclusions. Please note that your response in the journal's electronic author form
verifies that permission has been obtained from all named persons.
* If all or part of the paper was presented at the Annual Clinical and Scientific Meeting of the American College of
Obstetricians and Gynecologists or at any other organizational meeting, that presentation should be noted (include the
exact dates and location of the meeting).

9. The most common deficiency in revised manuscripts involves the abstract. Be sure there are no inconsistencies between
the Abstract and the manuscript, and that the Abstract has a clear conclusion statement based on the results found in the
paper. Make sure that the abstract does not contain information that does not appear in the body text. If you submit a
revision, please check the abstract carefully.

In addition, the abstract length should follow journal guidelines. The word limits for different article types are as follows:
Reviews, 300 words. Please provide a word count.

10. Only standard abbreviations and acronyms are allowed. A selected list is available online at http://edmgr.ovid.com
/ong/accounts/abbreviations.pdf. Abbreviations and acronyms cannot be used in the title or précis. Abbreviations and
acronyms must be spelled out the first time they are used in the abstract and again in the body of the manuscript.

12. The journal does not use the virgule symbol (/) in sentences with words. Please rephrase your text to avoid using
"and/or," or similar constructions throughout the text. You may retain this symbol if you are using it to express data or a
measurement.

13. In your Abstract, manuscript Results sections, and tables, the preferred citation should be in terms of an effect size,
such as odds ratio or relative risk or the mean difference of a variable between two groups, expressed with appropriate
confidence intervals. When such syntax is used, the P value has only secondary importance and often can be omitted or
noted as footnotes in a Table format. Putting the results in the form of an effect size makes the result of the statistical test
more clinically relevant and gives better context than citing P values alone.

If appropriate, please include number needed to treat for benefits (NNTb) or harm (NNTh). When comparing two
procedures, please express the outcome of the comparison in U.S. dollar amounts.

Please standardize the presentation of your data throughout the manuscript submission. For P values, do not exceed three
decimal places (for example, "P = .001"). For percentages, do not exceed one decimal place (for example, 11.1%)

14. Line 457: We discourage claims of first reports since they are often difficult to prove. How do you know this is the first
report? If this is based on a systematic search of the literature, that search should be described in the text (search engine,
search terms, date range of search, and languages encompassed by the search). If on the other hand, it is not based on a
systematic search but only on your level of awareness, it is not a claim we permit.

15. Please review the journal's Table Checklist to make sure that your tables conform to journal style. The Table Checklist

16. The American College of Obstetricians and Gynecologists' (ACOG) documents are frequently updated. These
documents may be withdrawn and replaced with newer, revised versions. If you cite ACOG documents in your manuscript,
be sure the reference you are citing is still current and available. If the reference you are citing has been updated (ie,
replaced by a newer version), please ensure that the new version supports whatever statement you are making in your
manuscript and then update your reference list accordingly (exceptions could include manuscripts that address items of
historical interest). If the reference you are citing has been withdrawn with no clear replacement, please contact the
editorial office for assistance (obgyn@greenjournal.org). In most cases, if an ACOG document has been withdrawn, it
should not be referenced in your manuscript (exceptions could include manuscripts that address items of historical
interest). All ACOG documents (eg, Committee Opinions and Practice Bulletins) may be found via the Clinical Guidance &

17. Figures

Figure 1: Is this original to the manuscript?

Figure 2: Should n=3,162 be the number of duplicates removed, rather than the number remaining?

18. Authors whose manuscripts have been accepted for publication have the option to pay an article processing charge and
publish open access. With this choice, articles are made freely available online immediately upon publication. An
information sheet is available at http://links.lww.com/LWW-ES/A48. The cost for publishing an article as open access can

Please note that if your article is accepted, you will receive an email from the editorial office asking you to choose a
publication route (traditional or open access). Please keep an eye out for that future email and be sure to respond to it promptly.

19. If you choose to revise your manuscript, please submit your revision through Editorial Manager at http://ong.editorialmanager.com. Your manuscript should be uploaded in a word processing format such as Microsoft Word. Your revision’s cover letter should include the following:

* A confirmation that you have read the Instructions for Authors (http://edmgr.ovid.com/ong/accounts/authors.pdf),

and

* A point-by-point response to each of the received comments in this letter.

If you submit a revision, we will assume that it has been developed in consultation with your co-authors and that each author has given approval to the final form of the revision.

Again, your paper will be maintained in active status for 14 days from the date of this letter. If we have not heard from you by Nov 28, 2019, we will assume you wish to withdraw the manuscript from further consideration.

Sincerely,

Nancy C. Chescheir, MD
Editor-in-Chief

2018 IMPACT FACTOR: 4.965
2018 IMPACT FACTOR RANKING: 7th out of 83 ob/gyn journals

In compliance with data protection regulations, you may request that we remove your personal registration details at any time. (Use the following URL: https://www.editorialmanager.com/ong/login.asp?a=r). Please contact the publication office if you have any questions.
Dear Dr. Chescheir,

Thank you for the very helpful review of our manuscript, “Social Determinants of Pregnancy-Related Mortality and Morbidity in the United States: A Systematic Review” (PROSPERO registration number CRD42018102415). We are pleased to submit this revised manuscript in light of your review and noted specific responses to reviewer comments below.

**Reviewer #1:**

*Introduction: Would be helpful to provide specific estimates on severe maternal morbidity and mortality in the US compared to other countries as background*

We included example comparisons of MMR on the revised manuscript lines 76-79: “The rate of maternal mortality has been steadily increasing in the US within the past 30 years and is one of the highest among high-income countries. For example, the US maternal mortality ratio (MMR) in 2017 was 19 maternal deaths per 100,000 live births, compared to 10 in Canada and 7 in the United Kingdom.” However, specific comparisons of SMM are harder to make across countries due to heterogeneity in SMM definitions, monitoring and measurements. (See Geller, S.E., Koch, A.R., Garland, C.E. et al. A global view of severe maternal morbidity: moving beyond maternal mortality. Reprod Health 15, 98 (2018) doi:10.1186/s12978-018-0527-2)

*Table: Providing some estimate on magnitude of differences in Table 2 would be helpful. The increasing or decreasing arrows seem oversimplified.*

Thank you for your suggestion. We added the effect estimates to Appendix 3 so that Table 2 is easier to read. However, we defer to the editor for final decision regarding placement of these numbers.

*Discussion: For first gap: are there any identified studies that do address the role of racism and/or residential segregation and the impact on SMM or mortality? As this is the biggest finding from the review, it would be helpful to summarize even smaller studies that might address this question*

Our search did not identify any studies that explicitly studied the role of racism or residential segregation in SMM or mortality. However, some studies on race and SMM/mortality have posited this mechanism within their discussion. We noted this on lines 444-450: “We did not find any studies in this review that explicitly studied measures of individual and structural racism (e.g. perceived everyday racism and residential segregation) in association with SMM or mortality, though this association has been suggested in studies of low birthweight and preterm birth. Future research might
benefit from grounding in a theoretical perspective such as Critical Race Theory and incorporating measures to better clarify the role of race and ethnicity in maternal outcomes, as has already been done in birth outcomes research.”

*Would refrain from "first" statements unless can be supported by literature search.*

We correspondingly changed line 570 to “this systematic review is one of the first” which does not carry the same claim as “this systematic review is the first.”

**Reviewer #2:**

1) **Abstract:**

   a. Line 50: Based on my understanding of the study as described in the body of the manuscript, this is a descriptive analysis rather than truly qualitative. Systematic review using a qualitative approach conveys to this reader, at least, that the results would then include content analysis for themes, or might consist of something of a "meta-ethnography." Rather, this study represents a descriptive accounting of the available quantitative literature on the subject. Perhaps my concern can be chalked up to semantics, but for this qualitatively and quantitatively trained reviewer, it was a bit jarring to see "qualitative analysis" in this context. I would recommend altering this phrase throughout to "descriptive."

   Line 454: Results are "descriptive" in nature.

   We agree and correspondingly changed the wording from “qualitative” to “descriptive” where noted.

   b. Line 52-53: While I recognize the ultimately descriptive nature of this systematic review, the subjective phrase "found substantial evidence" does not provide the reader an understanding of the way in which the evidence is "substantial." I would recommend a more quantitative descriptive approach to this claim, perhaps with proportions of articles with positive findings in the various arenas. Lines 52-58 would benefit from putting the statements in numerical/quantitative context (e.g., of the ** SDH categories we assessed, only ** were present in the reviewed literature rather than "literature is limited in the diversity of SDH studied").

   Thank you for this suggestion. We now quantify the number of articles with positive findings by SDH (see Table 2). Per your suggestion, we therefore adjusted our abstract as follows:

   “A total of 83 studies met inclusion criteria and were analyzed. 78 out of 83 studies examined socioeconomic position and/or individual factors as predictors, demonstrating evidence of associations between minority race and ethnicity (58/67 studies with positive findings), public or no insurance coverage (21/30), and lower education levels (8/13) and increased incidence of maternal death and SMM. Only 2/83 studies investigated associations between these outcomes and socioeconomic, political and cultural context (e.g. public policy); and 20/83 studies investigated material and physical circumstances (e.g. neighborhood
environment, segregation), limiting the diversity of SDH studied as well as evaluation of such evidence."

In the result section (lines 172-179), we also add more details from Table 1 on how many studies covered each category of SDH:

“Table 1 lists the frequency by which each predictor and outcome was studied in the literature. Of note, the majority of studies examined “socioeconomic position and individual factors” as predictors, most of which focused on race or ethnicity (67/83 studies) and insurance (30/83). Many fewer studied “material and physical circumstances” (20 studies) and “socioeconomic, political and cultural context” (2 studies on state policy).”

2) **Introduction**

a. **Line 75** - not only are SDH "also ... drivers of maternal mortality," but they shape the risk for the more proximal drivers noted in lines 73-74. It would be worth drawing this connection in the prose as you point to the figure which so nicely lays it out.

   We incorporated your suggestion, and that line (88-89) now reads SDH “shape the risk of these more proximal factors and thus are also substantial drivers of maternal mortality and morbidity.”

b. Relatedly, a reference to Geoffrey Rose's "Sick individuals and sick populations," the seminal public health publication in 1985 that drove public health professionals to ask "What puts people at risk of risks (e.g., what makes certain individuals more likely than others to have hypertension, or obesity)?" and sparked wider focus on the social determinants of health, and to Marmot and Wilkinson's seminal 1998 work "Social Determinants of Health" (published by the WHO) (or some of Marmot's earlier work 1991-1995), might be appropriate as the authors are describing/defining social determinants of health. If space for references is at issue, the single reference to the WHO Commission publications will suffice, but I do feel it is reasonable to bring these key publications to the attention of a clinical audience.

   Thank you for these references. We agree these are seminal works pertaining to our conception of SDH, and we incorporated them accordingly (lines 90-94): “According to the definition from the WHO Commission on the Social Determinants of Health (CSDH), built off the work of epidemiologists Michael Marmot and Richard Wilkinson, SDH consist of the material and social environmental conditions in which people are born, live, work, and age that may affect the health of an individual.”

3) **Methods:**

a. **Excellent that the study is registered in PROSPERO! I have a clarification, however, as the registered PROSPERO protocol explicitly states that qualitative studies will be included. In contradiction, the body of the Methods section (line 112-113) and Appendix 2 (inclusion/exclusion criteria) state that qualitative studies were excluded. Why the change?**
Please address why qualitative studies were ultimately excluded. This is especially intriguing given that no additional quantitative analyses on data extracted from the studies were completed.

Thank you for this great catch – we updated the PROSPERO protocol accordingly. We decided to exclude qualitative studies because we otherwise would have too much diversity and breadth in research methodologies for one systematic review, and synthesis of both qualitative and quantitative data would become complicated. We thought limiting to quantitative studies would lend more structure to the review, making the project more feasible, manageable and clear. That being said, we do believe that qualitative research is an important part of exploring this issue, and this type of methodology should be used in the future to expose and refine underlying mechanisms.

c. Figure 2 - Because this figure provides data on included/excluded studies and reasons for exclusion, it should be the first portion of the Results section, rather than methods. In addition, the phrasing of the box "Records after duplicates removed" makes it sound like that is the number remaining after duplicates are removed which becomes clear is not the case when the box records screened has an n of 9835. I would recommend simplifying the descriptor of that box to "Duplicate titles removed," or removing that box and modifying the Records screened to "Records remaining after duplicates removed."

Thank you for catching this error. We adjusted the PRISMA diagram to better reflect the duplicates removed and remaining records.

e. Line 125 - 129: Does this paragraph imply that studies rated poor by the NIH Tool were not included? If not, how is this represented in Figure 2? For those not familiar with the NIH tool, a description of the questions used in this tool (in Appendix 3 or otherwise) would be very useful.

Yes, we reworded that line to say, “Studies rated as poor were excluded from the review.” We included a brief description the NIH tool in the Methods section (142-147):

“One researcher assessed study quality using the NIH Quality Assessment Tool for Observational Cohort and Cross-Sectional Studies or for Case-Control studies, which is a series of guiding questions to appraise individual study quality and risk of bias. A “good” study has the least risk of bias, a “fair” study is susceptible to some bias but deemed not sufficient to invalidate its results, and a "poor" rating indicates significant risk of bias. A detailed list of these questions can be found in Appendix 4.”

A description of the questions used in this tool can be found in Appendix 4, along with detailed information about the quality of literature. Figure 2 is based on the PRISMA Flow Diagram and does not include exclusions by quality; in any case, of the studies that made it to the full-text review, no study was rated as “poor.”

f. In this reviewer’s opinion, since the search strategy provided in Appendix 1 is also provided in the PROSPERO link, Appendix 1 is somewhat redundant/unnecessary. It might be more helpful if among the appendices there were a table providing the details of the NIH Quality
Assessment Tool (answers to each of the questions for each of the studies). This would be invaluable to understanding the overall quality of the literature available (see, for example: https://www.mdpi.com/2072-6643/10/5/555/s1). This is the observational/cohort study equivalent of the detailed assessment of bias recommended by the PRISMA guidelines for randomized intervention trials. The Cochrane Collaboration suggests that a single summary score of poor/fair/good is insufficient to judge the bias or quality of a study (e.g., https://guides.library.cornell.edu/systematic_reviews/bias). Frankly, I am skeptical that no studies were rated "poor" among the 85 that met the specified inclusion criteria.

Therefore, it is imperative to understand how the reviewers rated each study on the individual questions of the quality assessment tool. The table or Appendix should be included or referenced in the Results.

We agree with your suggestion and therefore added a table similar to the one you suggested (Appendix 4). It includes a description of the NIH tool questions, as well as answers to each question for each of the studies, followed by an overall rating. While we mostly kept the same quality rating for each study, we understand that this single summary score is an overall gestalt and may be subjective. Therefore, we hope that a detailed assessment of each study will help readers to better make a judgment for themselves as well.

g. One surprising omission from the organizational structure is the impact of substance use disorder. Did the authors find any articles related to this? In most classification schemes, substance use disorder morbidities are considered "pregnancy-associated," however, substance use disorder, as with the frequent co-morbid mental health disorders, are frequently exacerbated by pregnancy. Substance use disorder, recovery, and addiction are also strongly tied to structural social determinants of health (the authors note this in relationship to mental health, suicide, and homicide, but again substance use is left out, on Line 449). Many maternal deaths are tied to substance use disorder and mental health disorders (see, for example, Smid et al 2019 - which is out of the time scope of this review, I believe), and it would be important to include this as a category even if there are no studies in the authors' time frame. It is an important gap in the literature as the opioid epidemic expands.

We absolutely agree that substance use is strongly tied to both social determinants of health as well as maternal mortality and morbidity. However, we felt its place in the conceptual model was not as a fundamental cause, but as a pathway, as are other health behaviors and comorbidities. For example, if were we to include substance abuse, we logically should then include other health behaviors and mental health diagnoses, and this would then expand the review beyond a feasible scope. Ultimately more research on this is needed, and therefore we added text to clarify these are important pathways from SDH that should be studied in the context of the fundamental causes of disease framework:

“Second, we excluded some measures of ‘pregnancy-associated’ mortality and morbidity such as those related to substance use, mental health, suicide or homicide. These are beyond the scope of this review but are no less important in
contributing to maternal deaths, particularly as SDH may strongly influence these indicators, pregnancy may exacerbate these conditions, and the opioid epidemic continues to grow. Ultimately, more research is needed on these outcomes, alongside more traditional measures of maternal health.” (Lines 558-564)

4) Results

a. **Figure 2 should be referenced with Lines 133-134.**

   We moved the reference to Figure 2 from Methods to Results.

b. **The section "Organization and definitions" seems as though it belongs in the Methods section rather than Results. I would remove this, tighten it, and place it in a similarly-titled section in Methods.**

   We agree, and we moved that section to Methods.

c. **Line 145-146: I recognize that the Callaghan 2012 reference is the peer-reviewed publication of the CDC's criteria for severe maternal morbidity. However, given that many now know these criteria as the CDC's rather than as "Callaghan's" it may be worth noting somewhere that this definition is the one utilized by the CDC.**

   We now specified that these criteria are ones used by the CDC (line 163-164): “We defined SMM based on a series of obstetric outcomes used by the Centers for Disease Control and Prevention (CDC).”

d. **Lines 282-284: It is unclear to me what is meant by "statistical significance depended on the sample." Do the authors mean wider or narrower confidence intervals (e.g. precision of the estimate), or a higher or lower p value (which is less useful) or a larger or smaller odds ratio? Or do the authors mean some studies had statistically significant findings, where others did not? It would be helpful to be more precise here to provide an understanding of the literature.**

   We apologize for the misunderstanding. That specific case-control study did not show significance by income (p=0.18), but the study was done with a smaller sample limited to one hospital. The other two studies that did show significant differences were larger studies of New York City and California. Therefore, we adjusted the language to say that “two large studies in New York City and California found increased pregnancy-related mortality in lower-income versus higher-income communities, although this difference was not significant in a smaller, single-hospital sample.” (Lines 364-368)

5) Discussion

a. **Line 330: Again, the use of "substantial evidence" is somewhat ambiguous. Is the weight of this evidence created simply by the number of papers? The effect size or precision? Some clarity here would be useful.**

   We intend this to mean that many studies have researched these associations and of those, the majority found significant differences compared to non-significant differences. This is reflected in the proportion of studies with “positive findings” in Table 2. The exact
effect size, however, for each study can be found in Appendix 3. We changed the language in the discussion to clarify this: “A large number of studies studied individual-level indicators of SEP in relation to maternal outcomes; of those, the majority suggest that black race and Hispanic ethnicity, lack of insurance, and lower education are significantly associated with higher risk of maternal mortality and morbidity.” (Lines 417-420)

b. Overall, the discussion is well structured and the gaps are clearly delineated. As mentioned in the Methods section, I would like to see some commentary on the contribution of SDH and substance use disorder.

We included more commentary in the limitations section (see response to above [3g]).

c. Line 401- 410: The limited study designs gap is an important one. However, the authors steal their own thunder by hinting at the types of complex study designs that are required for these studies at the ends of the two prior sections - for instance, hierarchical multilevel models, policy-related regression discontinuity approaches, life-course approaches (longitudinal latent class models or growth models), structural equation modeling, etc. etc. I agree that there is a definite need to improve the complexity of these studies to account for the complexity of data. However, it would be helpful for the authors to lay out more of a roadmap with some specific methodological approaches.

We moved some of this study “roadmapping” to the paragraph in Gap 3. We also connected the connection between Gaps 2 and 3 on lines 502-513.

“In order to link the complex relationships among policy, social and built environment, SEP, health services and outcomes, future research might benefit from the use of frameworks common to the SDH literature, such as the multilevel determinants of health or life-course health development. For example, a life course approach could incorporate longitudinal latent class models exploring trajectories of morbidity. A complex systems approach may help elucidate key leverage points for intervention yet rarely has been applied to maternal health. Other research approaches include following women over the lifespan or over multiple births as they change neighborhoods, states or insurance plans or leveraging changes in local or state health and social policy or trends in SDH, for example through natural experiments with regression discontinuity.”

d. One element missing from this discussion are the often low odds ratios that are achieved when studying the distal/structural social determinants of health. Though there has been much debate in the observational and cohort study literature about "pseudo-epidemics" and false results associated with low odds ratios, it is worth noting that these multi-level, complex risk factors frequently have low odds ratios because the effects are indirect (with multiple other steps in the pathway) and the data are messy. In this reviewer's opinion, the low odds ratios are less an indicator of no effect, but a call for replication. The complexity of the data (even when perfectly collected) will remain, and therefore replication will lend robustness to the literature. Multiple studies showing similar effect sizes and direction but low odds ratios lend credibility to
the associations between large structural risk factors and outcomes. It would be helpful for the authors to draw attention to this, as skeptics of SDH work frequently cite low odds ratios among the reasons to discount the importance of SDHs compared to more proximal factors.

We agree that replication showing similar effect size and direction lends support for associations between large structural risk factors and outcomes. Therefore, we added in the “Challenges” section the following addressing your suggestion: “Finally, we note that many of the effect sizes in the SDH literature are modest. However, studies often control for proximal determinants that may be on the causal pathway from SDH to morbidity, which could attenuate the total association of interest. Further, it is worth noting that modest effects can have large population impact if the exposure is common. The use of alternative measures of effect, such as the population attributable risk, may highlight the impact of the SDH more effectively than ratio measures.” (Lines 529-533)

Reviewer #3:

1. **Results** - it would be useful for the reader to know what the sources of the data were for the studies you evaluated. Were these data obtained from birth certificates or other such large databases? Single institution data? Later on the authors refer to state-level data or otherwise, but it would be helpful to have this information presented sooner and in one cohesive paragraph.

   The majority of studies collected data from nationwide or state-level administrative data, including linked birth certificates and hospital discharge data. Fewer were obtained directly from medical records or prospectively collected surveys. We included these distinctions in our large summary table (Appendix 3) and listed the number of studies by data sources on lines 181-183: “Data sources included nationwide or multistate (30 studies), state-level (29), city-level (5), or hospital-level (3) administrative data; medical records (14) and surveys/questionnaires (2).” (Lines 230-234)

2. Line 225 - it would be helpful to list here the number of women involved in the comparison, since it would provide a more useful context for the reader to know if the quantities of women in those studies with no difference were approximately the same as those with a difference.

   We list the number of women sampled for included studies in Appendix 3. In the manuscript, we note cases where there are discrepant findings that could be readily explained by study population or small sample size (for example, in lines 327-329: “Pregnancy-related mortality ratios have been found to be higher among unmarried women than married women in state and national analyses although this association was not found in smaller city- and hospital-level studies” and 365-366: “two large studies in New York City and California found increased pregnancy-related mortality in lower-income versus higher-income communities, although this difference was not significant in a smaller, single-hospital sample”). However, for the particular case you mention,
differences in sample size for studies with non-significant findings versus significant findings were not necessarily apparent, and therefore we have not included it in the text.

3. Discussion line 330 - the way the sentence is written indicates that Hispanic ethnicity is associated with higher rates of maternal mortality and morbidity, however the authors elsewhere showed some evidence for lower rates for Hispanic women (line 196-198, reference 32, 33). Please revise.

Thank you for pointing this out. What we mean is that studies consistently show an association between Hispanic ethnicity and higher risk of in-hospital mortality and SMM, although the crude MMR as mentioned in line 266 may be lower. We changed our language accordingly and also added clarification in that section of the results (lines 266-276):

“Prior to 2006, crude maternal mortality rates or risk have been found to be higher among Hispanic women compared to white women, but lower compared to black women. More recently (2008-2014), the unadjusted MMR for Hispanic women fell below the MMR for non-Hispanic white women, largely due to an increase in mortality among non-Hispanic white women. However, multivariate studies continue to demonstrate that Hispanic ethnicity increases risk of adjusted in-hospital mortality and cause-specific deaths.”

4. Discussion/Clinical Implications - the authors correctly state in line 433 that more SDH and interventions should be studied, however the authors should temper statements regarding interventions or changes in practice as a result of this study (line 429). This study design does not lend itself to recommendations for provision of clinical care; the subsection should perhaps be re-titled.

We agree that this systematic review does not necessarily provide results that may change clinical practice for an individual clinician. We therefore changed the subsection to be called “Implications for Clinicians” instead of “Clinical Implications” and re-worded the first sentence to reframe it as a learning versus implementation opportunity.

5. Figure 1 - the arrow leaving Health Behaviors on the left side ends without a conclusion. This is also the case for the arrow leaving stress/psychosocial factors on the right.

Thank you for noticing this. We adjusted the diagram so both arrows point to the box with pregnancy complications (as both health behaviors and stress/psychosocial factors can lead to increased risk of pregnancy complications).

Statistical Editor:

lines 133-134 and Fig 2: It is not clear how the studies met inclusion (n = 199) fits within the counts cited on lines for agreements and conflicts. Need a more complete flow diagram. How many of the n = 86 (line 50) vs n = 85 (Fig 2) included in the final analysis were initially agreed upon vs how many were initially disputed by the two primary researchers?
We changed the typo in the abstract to match the rest of the paper. After a second look, we ended up removing one study because it was judged to be a report and not a study, and another because exposure was a mental health diagnosis and therefore not captured within our concept of SDH. Neither of these changes affected the results, analysis or discussion. That leaves the total number of studies to be n=83.

The percent agreement was initially calculated as a way to capture inter-rater reliability for screening based on our inclusion and exclusion criteria. Kappa could not be calculated because of limitations of the Covidence software, and we believed percent agreement would be an appropriate substitute. That being said, we did not include it in the PRISMA diagram as it does not change the final number of records that met inclusion criteria.

Results (General): The studies are summarized by outcomes and by SEP, Insurance, Education etc. But it seems that quantitative results are not specifically cited. It might be useful to either enlarge those summaries (? actual Tables of results or ranges) or limit the presentation to more qualitative research, citing themes or areas that require more research. The present format seems neither qualitative nor quantitative in approach.

We included the quantitative results (ORs or RRs, CIs) of each study within Appendix 3 as indicated. Given the breadth of what we considered “social determinants of health” and diversity of study methodologies, our synthesis is more descriptive rather than qualitative (thematic) or quantitative (meta-analysis). We therefore presented the data in a way that demonstrates the general consensus on available studies. However, to help address your point, we added the number of studies with “positive findings” by SDH in Table 2. This gives readers a sense of a) the quantity of studies that investigated that SDH and b) within those, whether positive associations were found. The former shows us what areas require more research based on our analysis, as indicated in our discussion, and the latter gives more substance to the overall findings.

Editor:

The Precis should be no more than 25 words.

We edited the Precis to be 25 words.

Isn't your study to look at whether SDH actually do influence rates of pregnancy complications, etc? If so, then line 85 should read ..."may experience......"

The word “may” was added to that sentence.

Line 88: define social autopsy

We clarified what social autopsy means on lines 103-106: “questions designed to identify the social, behavioral and health system factors that may have contributed to deaths.”

Line 126: Do you mean all studies that met the inclusion criteria?
We clarify this in line 232-234: “All studies were rated as Good or Fair by the NIH Study Quality Assessment Tool, and thus none were excluded from the analyses (see Appendix 4 for individual assessments of quality).”

Line 133: for Clarity, please put the first sentence here which reports the final number of included articles AFTER the second sentence which reports the total #, etc

We changed the order of reporting so that now it flows from total number screened to final number analyzed (lines 186-190):

“A total of 9835 records were identified and screened (Figure 2). During the screening process, there were 9473 agreements for inclusion and 362 conflicts resolved by a third researcher, for a percent agreement of 96.3%. The selection search and screening process yielded a final number a total of 199 studies, of which 83 were included in our results based on the three outcome categories analyzed (see Methods section).”

Line 143: Some authors have argued that suicide might be a considered a pregnancy associated death given the risks of suicide associated with post partum depression. Perhaps worth a note of this in your discussion. SDH’s have certainly also been credited with risk for PP Depression.

Yes, we agree. Unfortunately, this was not included within the scope of our review based on our definition of pregnancy-related mortality, although it is nevertheless relevant. We noted this in our limitations section on lines 558-563:

“Second, we excluded some measures of maternal “pregnancy-associated” mortality and morbidity such as those related to substance use, mental health, suicide or homicide. These are which are beyond the scope of this review but are no less important in contributing to maternal deaths, particularly as SDH may strongly influence these indicators, pregnancy may exacerbate these conditions, and the opioid epidemic continues to grow.”

- Line 138: please give a definition of direct maternal morbidity

A clarification of the definition has been made on lines 168-170: ““Non-severe,” “direct” maternal morbidity describes less-severe cases of maternal ill health not captured by the WHO or CDC definition of maternal near-miss or SMM and is defined by the framework in Chou et al. (2016).”

- Line 167: you mentioned above that you only included those w/ Good or Fair ratings so not necessary to repeat here.

The previous statement was in reference to our methodology; this sentence in the results mentions what we found in terms of quality.

- Line 177-180: For the sections on Race/Ethnicity and Socioeconomic, Political and Cultural context, you described how the factors were associated with maternal death or SMM. In this section on Socioeconomic position, you just tell us that these were the most commonly studied,
but don't give us any information about the results of the studies. I'm not sure how your paper unfolds from here, but these paragraphs should have parallel construction, including results of the studies.

We structured the results along three broad categories (“Socioeconomic, Political and Cultural Context,” “Socioeconomic Position” and “Material and Physical Circumstances”) based on our Figure 1 and the WHO Conceptual Framework (described in lines 90-100). Within each broad category, we summarize findings for each specific social determinant by outcome (mortality morbidity, hospitalization). For the first category, Socioeconomic, Political and Cultural Context, there were so few studies that we only had substance for one paragraph. The second and third categories have subcategories.

To clarify this, we added numbers to the three bucket categories. We can also add letters to the sub-categories if you think this needs further clarification.

- line 190: "Higher excess" seems redundant. Are they higher risks or excess risks?

  We corrected this to indicate “higher risks.”

- Line 204: Please tell us in the methods section how you arrived at these Odds Ratios. Please also include confidence intervals.

  We noted that these are the ranges of odds ratios or risk ratios that we encountered among the studies looking at race and SMM and are not intended to be an aggregate estimate. We clarified this with “Depending on the study, black women had adjusted risk ratios ranging from…” (line 280-291). We also added in the methods section that we extracted relevant odds ratios and risk ratios from the data in Appendix 3 where applicable (229-230).

- Line 273: Assault during delivery? I've never read a paper to my recollection on assault during delivery. This doesn't seem like SDH as much as a trauma event.

  We used the incorrect terminology here and meant assault during pregnancy and not delivery. We corrected this.

- Line 316: Do you mean hospitals with the highest quartile of proportions of minorities? I'm thrown by the top 25% with highest proportion--that seems to be redundant.

  Yes, that study looked at hospitals with the highest quartile of proportions of minorities, and we readjusted the language.

2. The Editors of Obstetrics & Gynecology are seeking to increase transparency around its peer-review process, in line with efforts to do so in international biomedical peer review publishing. If your article is accepted, we will be posting this revision letter as supplemental digital content to the published article online. Additionally, unless you choose to opt out, we will also be including your point-by-point response to the revision letter. If you opt out of including your response, only the revision letter will be posted. Please reply to this letter with one of two responses:
A. **OPT-IN:** Yes, please publish my point-by-point response letter.

B. **OPT-OUT:** No, please do not publish my point-by-point response letter.

We opt-in to posting this revision letter.

3. As of December 17, 2018, Obstetrics & Gynecology has implemented an "electronic Copyright Transfer Agreement" (eCTA) and will no longer be collecting author agreement forms. When you are ready to revise your manuscript, you will be prompted in Editorial Manager (EM) to click on "Revise Submission." Doing so will launch the resubmission process, and you will be walked through the various questions that comprise the eCTA. Each of your coauthors will receive an email from the system requesting that they review and electronically sign the eCTA.

Please check with your coauthors to confirm that the disclosures listed in their eCTA forms are correctly disclosed on the manuscript's title page.

We will confirm this.

4. **Figure 1:** Has this figure been previously published elsewhere? If yes, please provide a copy of the original source.

Tables, figures, and supplemental digital content should be original. The use of borrowed material (eg, lengthy direct quotations, tables, figures, or videos) is discouraged, but should it be considered essential, written permission of the copyright holder must be obtained. Permission is also required for material that has been adapted or modified from another source.

Both print and electronic (online) rights must be obtained from the holder of the copyright (often the publisher, not the author), and credit to the original source must be included in your manuscript. Many publishers now have online systems for submitting permissions request; please consult the publisher directly for more information.

When you submit your revised manuscript, please upload 1) the permissions license and 2) a copy of the original source from which the material was reprinted, adapted, or modified (eg, scan of book page(s), PDF of journal article, etc.).

Figure 1 is original but adapted using concepts from the WHO Conceptual Framework for Action on the Social Determinants of Health. We obtained permission from the WHO (see uploaded permissions license) and credited the original source throughout the manuscript and in Figure 1.

5. As of January 1, 2020, authors of systematic reviews must prospectively register their study in PROSPERO (https://www.crd.york.ac.uk/PROSPERO), an international database of prospectively registered systematic reviews. Please refer to the PROSPERO registration number in your submitted cover letter and include it at the end of the abstract.

We added the PROSPERO registration number to both the cover letter and abstract.
6. Standard obstetric and gynecology data definitions have been developed through the reVITALize initiative, which was convened by the American College of Obstetricians and Gynecologists and the members of the Women’s Health Registry Alliance. Obstetrics & Gynecology has adopted the use of the reVITALize definitions. Please access the obstetric and gynecology data definitions at https://www.acog.org/About-ACOG/ACOG-Departments/Patient-Safety-and-Quality-Improvement/reVITALize. If use of the reVITALize definitions is problematic, please discuss this in your point-by-point response to this letter.

The reVITALize definitions are consistent with those in our manuscript.

7. Because of space limitations, it is important that your revised manuscript adhere to the following length restrictions by manuscript type: Review articles should not exceed 25 typed, double-spaced pages (6,250 words). Stated page limits include all numbered pages in a manuscript (i.e., title page, précis, abstract, text, references, tables, boxes, figure legends, and print appendixes) but exclude references.

Our revised manuscript is 5766 words and 24 pages without Table 2 and 34 pages with it. Although we meet the word limit, because of the long tables that summarize our data, we do not meet the page limit. We kindly ask for your advice on this matter.

8. Specific rules govern the use of acknowledgments in the journal. Please note the following guidelines:

* All financial support of the study must be acknowledged.

* Any and all manuscript preparation assistance, including but not limited to topic development, data collection, analysis, writing, or editorial assistance, must be disclosed in the acknowledgments. Such acknowledgments must identify the entities that provided and paid for this assistance, whether directly or indirectly.

* All persons who contributed to the work reported in the manuscript, but not sufficiently to be authors, must be acknowledged. Written permission must be obtained from all individuals named in the acknowledgments, as readers may infer their endorsement of the data and conclusions. Please note that your response in the journal’s electronic author form verifies that permission has been obtained from all named persons.

* If all or part of the paper was presented at the Annual Clinical and Scientific Meeting of the American College of Obstetricians and Gynecologists or at any other organizational meeting, that presentation should be noted (include the exact dates and location of the meeting).

All of these acknowledgements are noted in the paper.

9. The most common deficiency in revised manuscripts involves the abstract. Be sure there are no inconsistencies between the Abstract and the manuscript, and that the Abstract has a clear conclusion statement based on the results found in the paper. Make sure that the abstract does not contain information that does not appear in the body text. If you submit a revision, please check the abstract carefully.
In addition, the abstract length should follow journal guidelines. The word limits for different article types are as follows: Reviews, 300 words. Please provide a word count.

The abstract has been checked thoroughly to match the paper. The abstract word count is 296.

10. Only standard abbreviations and acronyms are allowed. A selected list is available online at http://edmgr.ovid.com/ong/accounts/abbreviations.pdf. Abbreviations and acronyms cannot be used in the title or précis. Abbreviations and acronyms must be spelled out the first time they are used in the abstract and again in the body of the manuscript.

We verified that abbreviation use is in line with these requirements.

12. The journal does not use the virgule symbol (/) in sentences with words. Please rephrase your text to avoid using "and/or," or similar constructions throughout the text. You may retain this symbol if you are using it to express data or a measurement.

We replaced all virgule symbols in the manuscript with an appropriate alternative.

13. In your Abstract, manuscript Results sections, and tables, the preferred citation should be in terms of an effect size, such as odds ratio or relative risk or the mean difference of a variable between two groups, expressed with appropriate confidence intervals. When such syntax is used, the P value has only secondary importance and often can be omitted or noted as footnotes in a Table format. Putting the results in the form of an effect size makes the result of the statistical test more clinically relevant and gives better context than citing P values alone.

If appropriate, please include number needed to treat for benefits (NNTb) or harm (NNTb).
When comparing two procedures, please express the outcome of the comparison in U.S. dollar amounts.

Please standardize the presentation of your data throughout the manuscript submission. For P values, do not exceed three decimal places (for example, "P = .001"). For percentages, do not exceed one decimal place (for example, 11.1").

We changed any results to reflect confidence intervals rather than p-values wherever possible. We also standardized the presentation of the data as indicated.

14. Line 457: We discourage claims of first reports since they are often difficult to prove. How do you know this is the first report? If this is based on a systematic search of the literature, that search should be described in the text (search engine, search terms, date range of search, and languages encompassed by the search). If on the other hand, it is not based on a systematic search but only on your level of awareness, it is not a claim we permit.

This was also mentioned by Reviewer #1, and we changed our language accordingly (see above).

15. Please review the journal's Table Checklist to make sure that your tables conform to journal style. The Table Checklist is available online here: http://edmgr.ovid.com/ong/accounts/table_checklist.pdf.
We reviewed the Table Checklist verifying that our tables conform to the journal style.

16. The American College of Obstetricians and Gynecologists' (ACOG) documents are frequently updated. These documents may be withdrawn and replaced with newer, revised versions. If you cite ACOG documents in your manuscript, be sure the reference you are citing is still current and available. If the reference you are citing has been updated (ie, replaced by a newer version), please ensure that the new version supports whatever statement you are making in your manuscript and then update your reference list accordingly (exceptions could include manuscripts that address items of historical interest). If the reference you are citing has been withdrawn with no clear replacement, please contact the editorial office for assistance (obgyn@greenjournal.org). In most cases, if an ACOG document has been withdrawn, it should not be referenced in your manuscript (exceptions could include manuscripts that address items of historical interest). All ACOG documents (eg, Committee Opinions and Practice Bulletins) may be found via the Clinical Guidance & Publications page at https://www.acog.org/Clinical-Guidance-and-Publications/Search-Clinical-Guidance.

The ACOG Committee opinion we cite is the most updated.

17. Figures

Figure 1: Is this original to the manuscript?

The figure is original to the manuscript but adapted from the WHO Framework (see response above). We added a sentence at the bottom of the figure to note this.

Figure 2: Should n=3,162 be the number of duplicates removed, rather than the number remaining?

Yes, we changed the figure, as discussed in our response to Reviewer #2.

This is a confirmation that we have read the Instructions for Authors. We hope that the manuscript will be suitable for publication in Obstetrics & Gynecology and look forward to hearing from you soon. You may also contact me at any time if you have further questions and comments.

Sincerely,

Eileen Wang