Obstetrics & Gynecology

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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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Date: Jun 06, 2019

To: "Adam Korrick Lewkowitz"

From: "The Green Journal" em@greenjournal.org

Subject: Your Submission ONG-19-930

RE: Manuscript Number ONG-19-930

Association between Intrapartum Severe Maternal Morbidity and Acute Psychiatric Illness within One Year of Delivery

Dear Dr. Lewkowitz:

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 21 days from the date of this letter. If we have not heard from you by Jun 27, 2019, we will assume you wish to withdraw the manuscript from further consideration.

REVIEWER COMMENTS:

Reviewer #1: Lewkowitz and colleagues present a retrospective cohort investigating the impact between severe maternal morbidity (SMM) and psychiatric illness and substance use disorders within one year after discharge for a hospitalization resulting in delivery of a liveborn singleton. The authors may consider the following comments as they revise their manuscript:

Title: the use of the phrase "within one year of delivery" does not align with the methods. SMM and psychiatric illness were investigated for one year post hospital discharge from the index delivery. While most women will be discharged by postpartum day 4, severe maternal morbidity is associated with prolonged admission. Please reconcile.

Abstract - conclusion: Replace "nearly" in the abstract conclusion. Additionally, while there was an increased risk of psychiatric morbidity among women with SMM, the absolute numbers were low. The authors should comment on this.

Line 121 - SAMHSA typically specifies psychiatric and substance use rather than labeling everything as a psychiatric illness. Consider expanding this terminology.

Line 131 - As previously mentioned, why 365 days after discharge and not 365 days after delivery?
How did the authors account for maternal deaths? The only place death is mentioned is line 146 "observations were censored by death". Recommend commenting on the number of maternal deaths among women who otherwise met inclusion criteria who died following an index delivery with SMM up to 365 days following hospital discharge.
Was tobacco use captured in this dataset? If so, did you control for this in the substance abuse composite and what was the aOR?

Line 152 - Sickle cell disease and eclampsia were not adjusted for as they were included in the SMM composite. Appendix A1 states severe preeclampsia was also included. I have concerns that by excluding preeclampsia it may inflate the results as preeclampsia is a risk factor for PTSD (PMID 23140343).

Line 258 - The authors state this manuscript provides insight into the prevalence of long-term postpartum psychiatric morbidity, but the results presented are 1 year after discharge of the index pregnancy. Please clarify.

Line 275 the authors state "there have been no published studies specifically analyzing the association between severe maternal morbidity as a composite or as individual conditions and postpartum psychiatric illness on a population-level." However, Wall-Wieler and colleagues present a similar manuscript (PMID 30334101) examining SMM and postpartum mental health-related outcomes in Sweden. Smaller studies have also been performed in Brazil (PMID 30365776) and England (PMID 27178126, 24708797). The authors should update their text to reflect this is not the first study examining

the association between SMM and psychiatric illness on a population-level. The discussion should also be updated to include at least the Sweden paper.

Line 295 - The authors missed an opportunity to present a public health message. In order to implement this to the women at highest risk, public payers need to fund this. Currently, the poorest women have limited to no access to mental health services while pregnant, or any services postpartum.

The authors should comment that while they found an increased risk, the absolute numbers are modest.
Is gestational age at delivery available in the dataset? Preterm births are associated with postpartum psychiatric disorders and the main demographic presented has an increased risk of preterm birth.

Reviewer #2:

Overall:
This paper describes a retrospective cohort study of a large inpatient and emergency department state wide database where ICD-9 codes for psychiatric illness and substance use were evaluated in the 12 month post-partum period for women with and without a CDC defined severe maternal morbidity (SMM) condition.

Other:
Conflicts of interest: None declared.

Funding: Funding was received from NIH and AHRQ.

Presentations: This paper was presented at the annual Society for Maternal-Fetal Medicine meeting in 02/19.

Human subjects: The study was exempted from review by the Washington University Human Research Protection Office.

Authors:
1.   Line 5: remove "Mr".

Abstract:
2. Specific and representative of the paper.

Introduction:
3. The purpose of the study is clearly stated. The study hypothesis is articulated.

Methods:
4. Well written section with sufficient detail to allow duplication. Several tables are included as appendices to support methods used. Results are presented for hospitalizations alone. Sensitivity analyses were done by extending the exclusion period for per-existing psychiatric morbidities and the exclusion of potentially confounding medical comorbidities.

5. Line 112: Please clarify - was only the index delivery within the study timeframe used (subsequent deliveries were not?).

6. Did the authors consider looking at any other post-partum time frames (such as 6 months?).

Results:
7. Data directly answer the research question. The very large sample size adds to the precision of the estimates.

8. Line 215: The previous line states that the p value for mode of delivery was marginally significant (p = 0.05) but in this sentence it states that the p value for mode of delivery was significant at p < 0.001.

Structured Discussion/Comment:
9. The results support the conclusion they arrive and support findings in the current literature. Strengths and limitations are thoroughly discussed. Presentation to the ED or hospital for psychiatric care in the first 12 months postpartum in not uncommon. However the risk is significantly higher in those women with SMM. Within that group women of color and of lower income were at higher risk.

The clinical implications of their findings is very clearly discussed. They encourage clinicians to follow ACOG's recommendations to screen all women for depression and anxiety after delivery and to individualized care with additional in-person visits as needed for 3 at least 3 months post-partum.

They speculate that their findings on risks associated with specific morbidities may contribute to the development of prediction tools.

References:
10. Pertinent literature is cited.

TABLES and FIGURES:
11. Tables are well prepared and support the paper.

Reviewer #3: Lewkowitz et al present a large retrospective cohort study evaluating the association between severe maternal morbidity and acuite psychiatric illness in the one year postpartum. They find that there is a strong association between SMM and acute psychiatric illness. Overall, this is an important contribution to the literature and is well done.

ABSTRACT:
- Line 67: The word "coding" here, after talking about SMM, is confusing in that it initially reads as a "code blue" rather than an ICD code. I would suggest rewording in the abstract.

- Conclusions: Based on the results you present in your abstract, this conclusion seems out of order - while in your manuscript you talk about how 2/100 is higher than you might have expected and therefore worth noting, this is note something that is really stressed in the abstract. I would suggest either reordering the conclusion or adding something about this in your methods section of your abstract.

INTRODUCTION:
- Line 99: The hypothesis that acute psychiatric illness is uncommon is vague and is never really explained elaborated on in the remainder of the manuscript. Please better elaborate on this hypothesis and how it will be tested in your methods.

MATERIALS AND METHODS:
- Line 113-114: Defining one year as the 365 days following discharge from the delivery hospitalization is imperfect, because those women with SMM will likely be discharged later (some much later) than those women without SMM. There is therefore differential amount of time for the possible outcome between women.

- Line 116: Why did you exclude multiple gestations? They are more likely to have SMM and you are not looking at any neonatal outcomes here.

- Lines 139-141: Please explain more. Could someone have both the primary and secondary outcome? If the secondary happened first, were they excluded from the primary?

- Lines 148-150: What is the point of this? Did you use this information elsewhere that I missed?

- Lines 150-152: How did you create your model? Were these all determined a priori? And is there a way to evaluate for preterm delivery? You state in your conclusion that there is a known association between neonatal morbidity and maternal depression.

- Line 152-153: Were these conditions included in your primary outcome and just not the adjusted analysis? Please explain more.

- Lines 161-162: I don't understand the interaction testing you did. SMM is the independent variable in your analysis... Please explain this better.

- Lines 171-172: How accurate do you think this is for assessing all underlying co-morbidity (as I believe that is your intention)? Would it be worth looking back at the year prior like you did with psychiatric conditions? Please comment on this in the discussion if nothing else.

RESULTS:
- Line 186: I'm not sure Appendix B is necessary.

- Line 199: There are too many n = ... in this sentence. I don't know which number goes with which outcome. Please also include percentages.

- Line 212-215: This relates to my prior confusion regarding the interaction - why does this matter? What do you do with this information?

- Line 229, 233, 235: Why split these all up? And are postpartum psychiatric morbidity and postpartum psychiatric condition different? This paragraph could be condensed.

- Line 237-239: Please clarify this sentence.

- Line 241-243: Likely many of the patients with sickle cell disease used narcotics prior to pregnancy. It seems strange to highlight this association given that.

- Overall there are SO many tables. I got lost reading the manuscript, meaning that I feel like I lost sight of what you are actually trying to convey and why. I would recommend either cutting down on the number of tables, or very clearly and succinctly stating why you did each analysis.

TABLES:
- Table 1: Why do you put OR rather than just p values? Do you need either?

- Appendix C: I remain confused by this. Is your dependent variable SMM or psychiatric illness?

- Appendix D1: Your headers here are different for table 1 and 2. Is severe intrapartum morbidity the same as SMM?

- Appendix D2 Table 3 and 4: Your headers here are again different. Is severe intrapartum morbidity the same as SMM?

- Appendix D4 Table 3 and 4: Same comment about the headers.

- Appendix D5 Table 2: Same comment about the headers

DISCUSSION:
- What is the baseline population risk of psychiatric presentation to the ED and admission? This would be a helpful number to compare your results too, especially because you say that these numbers are not uncommon in the postpartum period.

- Overall, it is worth pointing out some more specifics in your discussion - which conditions especially place women at risk? You have so much in your results that it is easy to get lost. The discussion is the place to re-focus.

- The last sentence of your paper should be something stronger than the statement that this could be made into a useful clinical calculator. While interesting, that sentence is a bit of a weak concluding sentence.

Reviewer #4: The article "Association between Intrapartum Severe Maternal Morbidity and Acute Psychiatric Illness within One Year of Delivery" by Lewkowitz et al is a retrospective cohort study that examines the association between severe maternal morbidity (SMM) and the increased risk of psychiatric illness in the year postpartum. The study examines the very relevant influence that poor mental illness has on this country's worsening maternal morbidity. I think the findings presented in this research have very real implications for monitoring and screening in the postpartum period, as the authors have demonstrated an association between SMM and acute psychiatric illness. The strengths of this study are its relevance and timeliness. Additionally, the large sample size over a 10 year period allows for a more diverse population. I think the list of SMM utilized for the study does a nice job of incorporating both obstetrically related morbidity (preeclampsia, gHTN) chronic illness (sickle cell) and then more acute and rare morbidities such as MI, renal failure and aneurysm. There are several clarifications worth mentioning:

Abstract: Line 50 "We compared outcomes after delivery with versus without SMM...." - this should be worded more clearly by stating "we compared women with SMM to those who did not have SMM using…"

Introduction: Line 92- define or give examples of "pregnancy complications"

I also think the introduction could benefit from some mention of why SMM is important. It might be worth while to point out the long term cost both to families and our economy of SMM -- why should people care about this who are not in Obstetrics?

Methods:
Line 119- Why did you exclude hospitalizations or ED encounters for 252 days? Is there a significance for that number?

Line 143- "….may have had more than one diagnosis within the composite but were only counted once per composite…." - can you clarify how you made the decision to select which diagnosis you utilized if they presented more than once with more than one diagnosis? Would it have been the first time they presented period?

Results:
Table 3 and 4 state that the annotation of subscript "2" is unable to report because there are less than 11 women but the N's suggest there were more than 11 woman. It's a little unclear.

Discussion:
Line 285- 288- I think this sentence should be rewritten, eliminating "normal" and "routine".

Line 368-369- There are several mentions of this clinical calculator. I think I understand at what you're getting at but unless you're actively working on this concept, I don't think you need to include that in this manuscript.

STATISTICAL EDITOR COMMENTS:

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

Table 2: Should include crude ORs to contrast with aORs. Should (could be in separate Table), the proportion of each psychiatric morbidity that might be attributed to SMM. That is, the aORs may be significantly increased compared to the referent, but the actual proportion attributable to SMM is low (typically < 1-2% of all psychiatric morbidity). Also, the counts among the SMM cohort for PTSD, acute stress reaction, adjustment disorder or alcohol use/dependence are each too few to allow for adjustment for 6 variables.

Table 3: Same comment re: need to include ORs, Need to state attributable risk %s and insufficient counts to adjust for 6 variables for all rows except for overall, transfusion, composite, ie, all the subsets for composite severe without transfusion.

Table 4: Same comment re: need to include ORs, Need to state attributable risk %s and inability to apply aORs to all subsets of composite SMM without transfusion.

Appendix B: Should include attributable risk %s.

General: As a sensitivity analysis, if one were to change the exclusion/inclusion criteria to include any prior psychiatric history, rather than just in 9 months preceding delivery, do the conclusions change?

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.

- These are not mutually exclusive, as your study shows. Acute psychiatric illness was uncommon in both those with and without SMM, but increased w/ SMM.

- As noted by one of your reviewers, the use of hospital discharge rather than delivery date is problematic. Women with SMM tend to have longer LOS after delivery which extends the 1 year time period beyond the birth date. While in most cases this is only a matter of a few days, in some in much longer. Can you do this analysis w/ HCUP data using birth date? Granted, some SMM occurs only in the post partum period so that may be an offsetting reason for using discharge data. Either way, it needs to be addressed in the discussion.

- In discussion, please explain these exclusions.

- please put the CDC SMM composite information in a box in the paper.

- I'm not sure what to make of describing this as a range. Is anemia within the range, for instance? If so, does that mean it is more severe than asthma?

- Please note that effect sizes (RR, OR) within the zone of potential bias should be noted as weak. Those effect sizes in the zone of potential interest should be emphasized in the discussion. . (Ref: False alarms and pseudo-epidemics. The limitations of observational epidemiology. Grimes DA, Schulz KF. Ob Gyn 2012;120:920-7)
As noted, SMM is not associated with the great majority of cases of psych.illness. In the discussion, you will need to address, given these issues, why this is important.

- Could this information (lines 208-210) be related to higher rates of discharge opioids in women with SMM leading to higher rates of dependence? THis woud be a different reason (although also very important) than an increased use due to psychiatric disorders resulting from the SMM and not its treatment.

- These are not significant and you have quite large numbers. We do no allow authors to describe variables or outcomes in terms that imply a difference (such us of the terms "trend" or "tendency" or "marginally different") unless there is a statistical difference. Please edit here and throughout.

- perhaps due to prior drug dependence?

- please temper this based on the above. Yes, it was significantly higher but about 3% vs 1.5% isn't a huge difference. Would care post partum differ prospectively in these 2 groups based on these findings?

- This is called a primacy claim: yours is the first, biggest, etc...In order to assert that, you need to provide the search terms used and the data base (s) searched (PubMed,GOogle Scholar, etc) to substantiate the claim. Otherwise, it needs to be deleted. It wouldn't belong in the abstract anyway, so make sure you address this in the manuscript body.

- please note other limitations from comments above.

- why no data but a +aOR for MI? and conversely, data but no aOR for aneurysm? All true for other examples.

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, as well as subsequent author queries. 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 response letter and subsequent email correspondence related to author queries.
B.   OPT-OUT: No, please do not publish my response letter and subsequent email correspondence related to author queries.

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.

Any author agreement forms previously submitted will be superseded by the eCTA. During the resubmission process, you are welcome to remove these PDFs from EM. However, if you prefer, we can remove them for you after submission.

4. All studies should follow the principles set forth in the Helsinki Declaration of 1975, as revised in 2013, and manuscripts should be approved by the necessary authority before submission. Applicable original research studies should be reviewed by an institutional review board (IRB) or ethics committee. This review should be documented in your cover letter as well in the Materials and Methods section, with an explanation if the study was considered exempt. If your research is based on a publicly available data set approved by your IRB for exemption, please provide documentation of this in your cover letter by submitting the URL of the IRB website outlining the exempt data sets or a letter from a representative of the IRB. In addition, insert a sentence in the Materials and Methods section stating that the study was approved or exempt from approval. In all cases, the complete name of the IRB should be provided in the manuscript.

5. 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.

6. Because of space limitations, it is important that your revised manuscript adhere to the following length restrictions by manuscript type: Original Research reports should not exceed 22 typed, double-spaced pages (5,500 words). Stated page limits include all numbered pages in a manuscript (i.e., title page, précis, abstract, text, tables, boxes, figure legends, and print appendixes) but exclude references.

7. 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).

8. 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: Original Research articles, 300 words. Please provide a word count.

9. 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.

10. 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.

11. 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.

12. 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.

13. 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 be found at http://edmgr.ovid.com/acd/accounts/ifauth.htm.

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.

14. If you choose to revise your manuscript, please submit your revision via Editorial Manager for Obstetrics & Gynecology at http://ong.editorialmanager.com. It is essential that your cover letter list point-by-point the changes made in response to each criticism. Also, please save and submit your manuscript in a word processing format such as Microsoft Word.

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 21 days from the date of this letter. If we have not heard from you by Jun 27, 2019, we will assume you wish to withdraw the manuscript from further consideration.

Sincerely,

Nancy C. Chescheir, MD
Editor-in-Chief

2017 IMPACT FACTOR: 4.982
2017 IMPACT FACTOR RANKING: 5th out of 82 ob/gyn journals

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6/25/2019, 9:36 AM

June 17, 2019

Dr. Chescheir,

Thank for the opportunity to revise our manuscript ONG-19-930 (entitled, "Association between Intrapartum Severe Maternal Morbidity and Acute Psychiatric Illness within One Year of Delivery') according to the comments from the reviewers and editors. We appreciate their thoughtful suggestions and have heavily revised the manuscript accordingly; we hope you find the result improved. Of note, after modifying the abstract based on reviewers' comments, we then edited the updated abstract to abide by the Journal's word limitations.

Our responses are below:
1. Response to Reviewers, Statistical Editor, and Editor

<u>Reviewer 1</u>

REVIEWER 1, COMMENT 1:
Title: the use of the phrase "within one year of delivery" does not align with the methods. SMM and psychiatric illness were investigated for one year post hospital discharge from the index delivery. While most women will be discharged by postpartum day 4, severe maternal morbidity is associated with prolonged admission. Please reconcile.
We agree that should be reconciled and have edited the title accordingly and also to align more with journal requirements.
Revised Text (Title, Page 1):
"Association between Severe Maternal Morbidity and Psychiatric Illness within One Year of Hospital Discharge after Delivery"

REVIEWER 1, COMMENT 2:
Abstract - Structured Discussion: Replace "nearly" in the abstract Structured Discussion. Additionally, while there was an increased risk of psychiatric morbidity among women with SMM, the absolute numbers were low. The authors should comment on this.
We have amended the Structured Discussion to the abstract to better align with the primary purpose of the manuscript (as suggested by Reviewer 3, Comment 2).
Revised Text (Abstract, Page 5, Lines 69-71):
"Though absolute numbers were modest, SMM was associated with increased risk of postpartum psychiatric morbidity and substance misuse. The highest period of risk extended to four months after hospital discharge."

REVIEWER 1, COMMENT 3:
Line 121 - SAMHSA typically specifies psychiatric and substance use rather than labeling everything as a psychiatric illness. Consider expanding this terminology.
This is an excellent point. While our outcomes clearly differentiate between psychiatric conditions and substance use, we agree that our description of the exclusion criteria lumps these two together. We have amended the manuscript to better reflect SAMHSA.

Revised Text (Methods, Page 7, lines 122-125):
"In addition, women coded during an inpatient hospitalization or ED encounter in the 252 days (9 months, a proxy for pregnancy) prior to hospital discharge of the index delivery for any condition within the primary and secondary composite outcomes were excluded from analysis due to pre-existing psychiatric morbidity or substance misuse."
> **AND**
> **Revised Text (Results, Page 11, lines 211-217):**

"898 deliveries with SMM (5.5%) and 34,969 deliveries without SMM (2.9%) were excluded due to prior psychiatric morbidity or substance misuse. **Appendix B** compares the conditions identified during the index pregnancy or during the index delivery hospitalization that resulted in exclusion from analysis. After excluding for pre-existing psychiatric illness and prior ED visits or inpatient admissions for substance misuse, the final analytic population of liveborn singleton pregnancies included 15,510 deliveries with SMM and 1,178,458 deliveries without SMM."

REVIEWER 1, COMMENT 4:
Line 131 - As previously mentioned, why 365 days after discharge and not 365 days after delivery?

The HCUP databases includes admission and discharge dates for all patients but does not directly include delivery dates for all women. Because ICD-9-CM procedure codes for operative vaginal delivery and cesarean delivery are unique, the delivery date can be directly obtained for women who have operative delivery. However, the delivery date for non-operative normal spontaneous vaginal deliveries cannot be directly obtained for all women. For some, ICD-9-CM procedure codes for procedures done routinely on day of delivery (episiotomy, manual rotation, perineal repair) provides a delivery date. For others who do not have these ICD-9-CM codes, determining delivery date is more problematic. Some women do have ICD-9-CM procedure codes related to induction of labor (artificial rupture of membranes or medical induction of labor), but the date these procedures occurred may not have occurred on actual date of delivery. Others did not have any pregnancy-related codes within their delivery hospitalization aside from the delivery ICD-9-CM procedure code. In order to use a cleanly defined start- and end-point to our follow-up period, we elected to use a year-follow-up period based on a variable included for all patients. We have modified the manuscript throughout to better explain our decision to use this follow-up period as well as emphasize the potential limitations of this decision.
> **Revised Text (Abstract, Page 4, Lines 49-50):**

"To determine whether severe maternal morbidity (SMM) is associated with increased risk of psychiatric illness in the year after delivery hospital discharge."
> **AND**
> **Revised Text (Abstract, Page 4, Lines 55-58):**

"The primary outcome was ICD-9-CM codes in ED encounter or inpatient admission within one year of hospital discharge of composite psychiatric morbidity (suicide attempt, depression, anxiety, post-traumatic stress disorder, psychosis, acute stress reaction, or adjustment disorder)."
> **AND**
> **Revised Text (Abstract, Page 4, Lines 62-65):**

"Within one year of hospital discharge, 2.9% (n=452) of women with SMM had the primary outcome: the risk of psychiatric morbidity was 75% higher versus women without SMM (n=19,279 (1.6%); adjusted odds ratio (aOR) 1.74 (95% Confidence Interval (CI) 1.58 – 1.91))."
> **AND**
> **Revised Text (Introduction, Page 6, Lines 96-101):**

"Using a state database, we aimed to determine the incidence of presentation for acute psychiatric care in the ED or inpatient hospital in the year after hospital discharge from delivery of a liveborn singleton and to ascertain whether having SMM during delivery was associated with increased risk of acute psychiatric

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illness within one year of hospital discharge compared to deliveries without SMM. We hypothesized that liveborn singleton deliveries with SMM would be associated with increased risk of psychiatric morbidity in the year after hospital discharge when compared to deliveries without SMM.

    AND

    Revised Text (Methods, Page 8, Lines 136-138):

"Because the HCUP database includes admission and discharge dates for all patients but does not include delivery date for all women with normal spontaneous vaginal delivery, outcomes were extracted from the day after index delivery hospital discharge through 365 days after discharge."

    AND

    Revised Text

    (Results, Page 12, Lines 225-228):

"After singleton liveborn deliveries without SMM, 1.6% of women (n=19,279) received primary or secondary diagnosis codes for psychiatric illness within one year of hospitalization discharge, during either ED presentations (n=12,328) or hospital admissions (n=7041)."

    AND

    Revised Text (Results, Page 14, Lines 276-278):

"The majority of SMM conditions, including blood transfusion, were associated with increased risk of coding for substance misuse during presentation in the ED or inpatient hospitalization coded within one year after discharge from delivery hospitalization."

    AND

    Revised Text (Structured Discussion, Pages 19, Lines 410-415):

"Third, due to limitations within the HCUP databases, our pre-defined follow-up period was not one year after delivery, but one year after hospital discharge from index delivery. Because women who had SMM are more likely to have prolonged delivery hospitalizations,[50] our follow-up period was more likely to extend beyond one-year of delivery for women with SMM compared to those without SMM. This discrepancy could lead to ascertainment bias that may have impacted our findings."

    AND

    Revised Text (Structured Discussion, Page 17, Lines 361-363)

"First, our findings provide clear insight into a clinical phenomenon that is not uncommon—acute postpartum psychiatric illness in the year after hospital discharge from delivery with and without SMM—and can ultimately help guide clinical practice."

    AND

    Revised Text (Structured Discussion, Page 18, Lines 366-368):

"Third, we linked the HCUP ED and inpatient databases, allowing us to more accurately exclude women with antepartum psychiatric morbidity and identify postpartum outcomes until one year after hospital discharge from delivery."

    AND

    Revised Text (Structured Discussion, Page 21, Lines 437-440):

"In conclusion, we found that postpartum psychiatric illness and substance use within one year of hospital discharge occurs significantly more often among women who suffer intrapartum severe maternal morbidity during delivery of a liveborn singleton compared to those who do not have SMM, though the absolute numbers and risk attributable to SMM were both modest."

REVIEWER 1, COMMENT 5:

How did the authors account for maternal deaths? The only place death is mentioned is line 146 "observations were censored by death". Recommend commenting on the number of maternal deaths among women who

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otherwise met inclusion criteria who died following an index delivery with SMM up to 365 days following hospital discharge.

Our follow-up period was censored by ICD-9-CM coding for the primary or secondary outcome, for subsequent pregnancy, for death, or for 365 days after discharge from index delivery hospitalization. Thus, maternal death was not accounted for in our analyses directly. However, we do agree that a varying death rate between our two analytic groups is important to present and discuss and have consequently included this in the results and Structured Discussion.

Revised Text (Results, Page 12, Lines 240-242)

"Of note, a higher proportion of women who had SMM had their follow-up period censored by death within 365 days of discharge from delivery compared to those who did not have SMM (n=330 (2.18%) versus n=243 (n=0.02%)."

AND

Revised Text (Structured Discussion, Page 20, lines 415-419)

"Fourth, our follow-up period was censored by ICD-9-CM coding for the primary or secondary outcome, for subsequent pregnancy, for death, or for 365 days after discharge from index delivery hospitalization. However, women who had SMM had higher rates of death within one year of hospital discharge, which may confound our findings since women censored by death could not have the primary or secondary outcomes."

REVIEWER 1, COMMENT 6:

Was tobacco use captured in this dataset? If so, did you control for this in the substance abuse composite and what was the aOR?

We did not capture tobacco use because this variable is thought to be under-coded in the HCUP database (PMID: 26536469). As we did not code for this variable in our study population, we could not control for this in the substance abuse composite. We have amended the methods to include this omission.

Revised Text (Methods, Page 8, lines 129-131)

"Tobacco use was not included in our study population as this variable has been shown to be under-coded in the HCUP database overall and within the state of Florida specifically.[20]"

REVIEWER 1, COMMENT 7:

Line 152 - Sickle cell disease and eclampsia were not adjusted for as they were included in the SMM composite. Appendix A1 states severe preeclampsia was also included. I have concerns that by excluding preeclampsia it may inflate the results as preeclampsia is a risk factor for PTSD (PMID 23140343).

We agree that medical comorbidities alone and in combination are associated with psychiatric morbidity, which is why we conducted a sensitivity analysis excluding women with medical comorbidities coded during the delivery hospitalization (Methods Line 9, lines 170-174 in original document). However, we did not include the citation about PTSD and preeclampsia so have added this to our manuscript. Thank you.

Revised Text (Methods, Pages 10, Lines 197-199)

"Second, we excluded women who had medical comorbidities identified during their delivery hospitalization as medical comorbidities alone[25] or in combination[26] have been associated with increased risk of depression or PTSD.[27]"

REVIEWER 1, COMMENT 8:

Line 258 - The authors state this manuscript provides insight into the prevalence of long-term postpartum psychiatric morbidity, but the results presented are 1 year after discharge of the index pregnancy. Please clarify.

Excellent point. We have clarified that our findings were limited to one year after hospital discharge.

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Revised Text (Structured Discussion, Page 15, Lines 295-298)

"In this large retrospective cohort study, we provide insight into the incidence of year-long postpartum psychiatric morbidity in women with liveborn singleton pregnancies with and without severe maternal morbidity."

REVIEWER 1, COMMENT 9:

Line 275 the authors state "there have been no published studies specifically analyzing the association between severe maternal morbidity as a composite or as individual conditions and postpartum psychiatric illness on a population-level." However, Wall-Wieler and colleagues present a similar manuscript (PMID 30334101) examining SMM and postpartum mental health-related outcomes in Sweden. Smaller studies have also been performed in Brazil (PMID 30365776) and England (PMID 27178126, 24708797). The authors should update their text to reflect this is not the first study examining the association between SMM and psychiatric illness on a population-level. The Structured Discussion should also be updated to include at least the Sweden paper.

Thank you for this comment and for providing these citations, which disprove our report of primacy. We have updated the manuscript accordingly.

Revised Text (Structured Discussion, Page 16, Lines 318-327)

"Our findings support one prior population-wide study analyzing the association between severe maternal morbidity and physician-diagnosed postpartum psychiatric illness[32] as well as smaller studies describing the association between SMM and patient-reported psychological symptoms.[33,34] In addition, our findings contribute to the clear association between admission to an intensive care unit (ICU) and increased odds of psychiatric morbidity, which has been shown to persist from three months[35] to one year[36] after ICU discharge. Finally, the increased risk within our study population of postpartum psychiatric illness and specific conditions within the SMM composite support prior data suggesting that patients have increased risk of longstanding residual depression after tracheostomy for treatment of head-and-neck cancer,[37] surviving acute myocardial infarction,[38] or diagnosis of adult respiratory distress syndrome.[39] "

REVIEWER 1, COMMENT 10:

Line 295 - The authors missed an opportunity to present a public health message. In order to implement this to the women at highest risk, public payers need to fund this. Currently, the poorest women have limited to no access to mental health services while pregnant, or any services postpartum.

Thank you for pointing this out. We agree that our findings have significant public health ramifications—particularly with the additional Cox modeling—and have edited the Structured Discussion to better highlight this.

Revised Text (Structured Discussion, Pages 16-17, Lines 340-349)

"In addition, our findings could have significant public health ramifications for postpartum care in the United States. Though the absolute number of women with risk attributable to SMM was modest, we found that women who have SMM have a significantly increased risk of postpartum psychiatric morbidity for a year after hospital discharge from their delivery, and that this risk was highest in the first four months. Women with increased risk of SMM were non-White, in the lowest income quartiles by zip code, and had public insurance, which often discontinues at six weeks postpartum. Thus, women at higher risk for SMM may be left without access to medical or psychiatric care during the highest risk time for acute psychiatric morbidity, which extends for months after their hospital discharge. Our findings suggest that public health insurance should extend medical and mental health insurance benefits beyond six weeks postpartum, ideally for all women but particularly for those who have SMM during delivery."

AND

Revised Text (Structured Discussion, Page 21, Lines 443-445):

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"These findings suggest that additional psychosocial support—as well as access to medical and mental healthcare—should be available to all women in the year after delivery, particularly those with intrapartum SMM in the first four months after hospital discharge."

REVIEWER 1, COMMENT 11:
The authors should comment that while they found an increased risk, the absolute numbers are modest.
> Excellent point. We have revised the manuscript to discuss the modest absolute increase.
> **Revised Text (Structured Discussion, Pages 15, Lines 301-303):**

"However, the risk was higher for both the psychiatric morbidity composite and substance use composite when deliveries with SMM were compared to those without SMM, though the absolute numbers and risk attributable to SSM were both modest."
> **AND**
> **Revised Text (Structured Discussion, Page 19, Lines 393-395):**

"In conclusion, we found that postpartum psychiatric illness and substance use within one year of hospital discharge occurs significantly more often among women who suffer intrapartum severe maternal morbidity during delivery of a liveborn singleton compared to those who do not have SMM, though the absolute numbers and risk attributable to SMM were both modest..."

REVIEWER 1, COMMENT 12:
Is gestational age at delivery available in the dataset? Preterm births are associated with postpartum psychiatric disorders and the main demographic presented has an increased risk of preterm birth.
> Some limitations to HCUP include the lack of gestation age at delivery as well as linked neonatal outcomes. The ICD-9-CM code 650 is supposed to be used only for term deliveries; however, the other ICD-9-CM code we used to identify deliveries (V270) does not specific gestational age. There is one ICD-9-CM code available for preterm delivery, but this does not stratify by gestational age and has not been well-validated. Thus, while we agree that the lack of gestational age may impact our findings, we are unable to control for this. We have included this as a limitation.
> **Revised Text (Structured Discussion, Page 19, Lines 407-410):**

"Second, the HCUP database does not contain gestational age or neonatal outcomes, though Neonatal Intensive Care Unit admission is known as a risk factor for postpartum depression and anxiety.[49] The lack of gestational age or neonatal outcomes may confound our results."

Reviewer 2

REVIEWER 2, COMMENT 1:
Authors: Line 5: remove "Mr".
> We have done so.
> **Revised Text (Title Page, Page 1, Line 5)**
> Text deleted

REVIEWER 2, COMMENTS 2-4:
2. Abstract: Specific and representative of the paper.
3. Introduction: The purpose of the study is clearly stated. The study hypothesis is articulated.
4. Methods: Well written section with sufficient detail to allow duplication. Several tables are included as appendices to support methods used. Results are presented for hospitalizations alone. Sensitivity analyses were done by extending the exclusion period for per-existing psychiatric morbidities and the exclusion of potentially confounding medical comorbidities.

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Thank you for this positive feedback.

REVIEWER 2, COMMENT 5:
   Line 112: Please clarify - was only the index delivery within the study timeframe used (subsequent deliveries were not?).
   Yes, only the index delivery within the study timeframe were included.
   Revised Text (Methods, Page 7, Line 115)
   "Only the index delivery was included for analysis."

REVIEWER 2, COMMENT 6:
Did the authors consider looking at any other post-partum time frames (such as 6 months?).
This is an excellent and highly clinically relevant point. When the study was developed, we defined our follow-up period to be one year. However, we agree with this comment that other postpartum time frames should be considered and elected to conduct additional exploratory analyses with the primary outcome to identify whether an interval within the twelve month study period corresponded to higher risk. When breaking the study period into smaller intervals, we decided to conduct Cox proportional hazards as this modeling is a preferred compared to logistic regression in terms of variation over time. We have amended the abstract, methods, results, and Structured Discussion to reflect these new analyses and their findings.
   Revised Test (Abstract, Page 4, Lines 60-61)
"Cox proportional hazard models identified the highest risk period after hospital discharge for the primary outcome."
   AND
   Revised Text (Abstract, Page 4, Lines 65-66)
"The highest risk interval was within four months of discharge (adjusted hazard ratio (aHR) 2.53 (95% CI 2.05 – 3.12))."
   AND
   Revised Text (Methods, Pages 9-10, lines 180-189)
"Second, we conducted Cox proportional hazard ratios to examine the association between stillbirth (and term livebirth) and the primary outcome over the 12-month follow-up period. The logrank test was used to compare survival functions. The proportional hazards assumption was assessed by significance testing of a time-dependent interaction. In anticipation that the risk of postpartum psychiatric morbidity was not consistent during the year-long follow-up period, additional Cox proportional hazard models were created to explore the highest risk window for postpartum psychiatric morbidity in clinically relevant intervals: the 12-month follow-up period was to be divided into six-month, four-month, and three-month intervals if needed. Once significance testing of a time-dependent interaction showed the proportional hazards assumption was not violated, the Heaviside unit step function was utilized to accommodate for variations in time-dependent interactions in the follow-up period.[24]"
   AND
   Revised Text (Results, Pages 12-13, Lines 243-249)
"Cox proportional hazard ratios were used to examine the association between SMM and the primary outcome over the 12-month follow-up period. The proportional hazards assumption was violated for the 12-month ($p < 0.001$) and six-month ($p = 0.003$) follow-up periods but not violated when follow-up was limited to the first four months ($p = 0.2$). After adjusting for maternal age, race/ethnicity, insurance status, and income quartile by zip code, we found a statistically significant association between SMM compared to no SMM and the development of the primary outcome for the first four months after hospital discharge (adjusted hazard ratio of 2.53 (95% CI 2.05 – 3.12))."

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AND

Revised Text (Structured Discussion, Page 15, Lines 305-309)

"Lastly, we identified that the highest risk of postpartum psychiatric morbidity was in the first four months after hospital discharge from delivery with SMM vs without SMM (adjusted hazard ratio 2.53 (95% CI 2.05 – 3.12)), suggesting that access to medical and mental healthcare is needed beyond the six-week postpartum period traditionally covered by public health insurance."

AND

Revised Text (Structured Discussion, Page 21, Lines 443-445)

"These findings suggest that additional psychosocial support—as well as access to medical and mental healthcare—should be available to all women in the year after delivery, particularly those with intrapartum SMM in the first four months after hospital discharge."

REVIEWER 2, COMMENT 7:

Data directly answer the research question. The very large sample size adds to the precision of the estimates.

Thank you. No edits needed.

REVIEWER 2, COMMENT 8:

Line 215: The previous line states that the p value for mode of delivery was marginally significant (p = 0.05) but in this sentence it states that the p value for mode of delivery was significant at p < 0.001.

Excellent catch. The second mention of "mode of delivery" was added in error. We have deleted this error as well as the Structured Discussion of marginal significance.

Revised Text (Results, Page 13, Lines 251-254)

"The p values for interactions were not statistically significant for payer type or mode of delivery ($p = 0.07$ and $p = 0.05$, respectively) but were statistically significant for race/ethnicity ($p = 0.003$), income quartile by zip code ($p < 0.001$), and maternal co-morbidities ($p < 0.001$)."

REVIEWER 2, COMMENTS 9-11:

9. Structured Discussion/Comment: The results support the Structured Discussion they arrive and support findings in the current literature. Strengths and limitations are thoroughly discussed. Presentation to the ED or hospital for psychiatric care in the first 12 months postpartum in not uncommon. However the risk is significantly higher in those women with SMM. Within that group women of color and of lower income were at higher risk.

The clinical implications of their findings is very clearly discussed. They encourage clinicians to follow ACOG's recommendations to screen all women for depression and anxiety after delivery and to individualized care with additional in-person visits as needed for 3 at least 3 months post-partum.

They speculate that their findings on risks associated with specific morbidities may contribute to the development of prediction tools.

10. References: Pertinent literature is cited.

11. TABLES and FIGURES: Tables are well prepared and support the paper.

Thank you for these positive comments.

Reviewer 3

REVIEWER 3, COMMENT 1:

Abstract: Line 67: The word "coding" here, after talking about SMM, is confusing in that it initially reads as a "code blue" rather than an ICD code. I would suggest rewording in the abstract.

We have added the word ICD-9-CM to clarify that we do not refer to a code blue in the abstract.

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Revised Text (Abstract, Pages 4, Lines 67-68):
"Women with SMM had nearly twofold higher risk of substance use (170 (1.1%) versus 6861 (0.6%); aOR 1.91 (95% CI 1.64 – 2.23))."

REVIEWER 3, COMMENT 2
Abstract Structured Discussions: Based on the results you present in your abstract, this Structured Discussion seems out of order - while in your manuscript you talk about how 2/100 is higher than you might have expected and therefore worth noting, this is note something that is really stressed in the abstract. I would suggest either reordering the Structured Discussion or adding something about this in your methods section of your abstract.

Thank you for this suggestion. We have revised the Structured Discussion of the abstract as stated in our response to Reviewer 1, Comment 2.

REVIEWER 3, COMMENT 3:
Introduction: Line 99: The hypothesis that acute psychiatric illness is uncommon is vague and is never really explained elaborated on in the remainder of the manuscript. Please better elaborate on this hypothesis and how it will be tested in your methods.

We agree and have amended our hypothesis accordingly.
Revised Text (Introduction, Page 6, Lines 99-101)
"We hypothesized that liveborn singleton deliveries with SMM would be associated with increased risk of psychiatric morbidity in the year after hospital discharge when compared to deliveries without SMM."

REVIEWER 3, COMMENT 3:
Material and Methods: Line 113-114: Defining one year as the 365 days following discharge from the delivery hospitalization is imperfect, because those women with SMM will likely be discharged later (some much later) than those women without SMM. There is therefore differential amount of time for the possible outcome between women.

Thank you for this comment. Indeed, the variation of the follow-up period between the two groups (SMM vs no SMM) is an important limitation to our analyses. Please refer to our response to Reviewer 1, Comment 4 for how we edited the manuscript to emphasize this limitation.

REVIEWER 3, COMMENT 4:
Material and Methods: Line 116: Why did you exclude multiple gestations? They are more likely to have SMM and you are not looking at any neonatal outcomes here.

We agree that women with multiples are more likely to have SMM and that our database does not include neonatal outcomes. However, we excluded multiples (and stillbirths) from our analyses as each have been associated with increased risk of postpartum psychiatric morbidity. We have edited the manuscript to explain our decision to exclude both stillbirth and multiple gestations, which we view as a study strength.
Revised Text (Methods, Page 7, Lines 116-121)
"Women whose index deliveries were coded as both liveborn singletons and stillbirth ≥23 weeks gestation (ICD-9-CM codes 656.40, 656.41, and V271) or multiple gestation (ICD-9-CM codes 651.00, 651.01, 651.10, 651.11, 651.20, 651.21, V272.2 – V27.7) were excluded from analysis because stillbirth[18] and multiple gestation[19] have each been associated with increased risk of postpartum psychiatric morbidity, which would have confounded our analyses."
AND
Revised Text (Structured Discussion, Page 18, Lines 374-376):

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"Fourth, our study population was restricted to livebirth singletons, which decreased the potential for confounding given both stillbirth[18] and multiple gestation[19] have each been associated with increased risk of postpartum psychiatric morbidity."

REVIEWER 3, COMMENT 5:
Material and Methods: Lines 139-141: Please explain more. Could someone have both the primary and secondary outcome? If the secondary happened first, were they excluded from the primary?
        This is an important clarification that we have included in our manuscript.
        Revised Text (Methods, Pages 8-9, Lines 150-153):
"Women could have both the primary and secondary outcome if they were coded for conditions in both composites during the same ED or hospital encounter, but having ICD-9-CM coding for one outcome excluded women from being eligible for the other outcome at subsequent encounters."

REVIEWER 3, COMMENT 6:
Material and Methods: Lines 148-150: What is the point of this? Did you use this information elsewhere that I missed?
        Our outcomes were censored by 365 days after index delivery hospital discharge, death, or subsequent hospitalization coded for pregnancy. This portion of the text clarified how we identified subsequent pregnancy in the methods and referred to an Appendix including the specific ICD-9-CM coding we used to censor for subsequent pregnancy.

REVIEWER 3, COMMENT 7:
Material and Methods: Lines 150-152: How did you create your model? Were these all determined a priori? And is there a way to evaluate for preterm delivery? You state in your Structured Discussion that there is a known association between neonatal morbidity and maternal depression.
        We have edited our methods to better explain how our model was created, which adjusted for statistically significant differences between the two analytic groups (the factors were not determined a priori). In terms of evaluation for preterm delivery, refer to our response from Reviewer 1, Comment 12.
        Revised Text (Methods, Page 9, Lines 163-165):
"Multivariable logistic regression analyses were adjusted for differences between sociodemographic and medical factors of women with versus without SMM during delivery hospitalization that were identified to be statistically significant: age, race/ethnicity, payer, income quartile by zip code, mode of delivery, and maternal medical comorbidities."

REVIEWER 3, COMMENT 8:
Material and Methods: Line 152-153: Were these conditions included in your primary outcome and just not the adjusted analysis? Please explain more.
        Sickle cell disease and eclampsia were conditions included within the SMM composite, but some ICD-9-CM codes used for these conditions were also included within the morbidity composite. Because they were in both composites, we couldn't adjust for them in the multivariable model.
        Revised Text (Methods, Page 9, lines 167-169)
"Sickle cell disease and eclampsia were conditions included within the SMM composite, but some ICD-9-CM codes used for these conditions were also included within the morbidity composite; thus, we were unable to adjust for Sickle cell disease or eclampsia in the multivariable models."

REVIEWER 3, COMMENT 9:

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Material and Methods: Lines 161-162: I don't understand the interaction testing you did. SMM is the independent variable in your analysis... Please explain this better.

You are correct; SMM is an independent variable. We reviewed our syntax in SAS and confirmed the analyses were performed correctly but described incorrectly in the methods and one sentence in the results (though the actual results were described correctly). We apologize for this confusion and have amended the manuscript to better describe the interaction testing.

Revised Text (Methods, Page 10, Lines 189-190):

"Third, we tested whether significant interactions existed between independent variables within the primary logistic model."

AND

Revised Text (Results, Page 13, Lines 250-251):

"Multivariable analyses testing whether significant interactions between independent variables existed in the primary outcome's model are shown in **Appendix C**."

REVIEWER 3, COMMENT 10:

Material and Methods: Lines 171-172: How accurate do you think this is for assessing all underlying co-morbidity (as I believe that is your intention)? Would it be worth looking back at the year prior like you did with psychiatric conditions? Please comment on this in the Structured Discussion if nothing else.

The maternal comorbidity composite we utilized has been validated in terms of its predictive value for SMM. Thus, we believe the composite is highly accurate in terms of assessing underlying comorbidities. Nevertheless, the accuracy may have been strengthened by looking back during pregnancy like we did for psychiatric conditions. We have included this as a limitation in our Structured Discussion.

Revised Text (Structured Discussion, Page 20, Lines 419-422)

"Fifth, though the maternal comorbidity composite we utilized has been validated in terms of its accuracy in identifying comorbidities in population-based data,[4] we limited identification of comorbidities to the delivery hospitalization, which may have decreased our ability to identify all comorbidities within our study population."

REVIEWER 3, COMMENT 11:

Results: Line 186: I'm not sure Appendix B is necessary.

We believe it is interesting and important to note that more women who ended up having SMM during their delivery received ICD-9-CM coding in ED visits or hospital admissions in the nine months prior to delivery for psychiatric illness or substance use than those who did not have SMM in their delivery. This resulted in us excluding proportionally more women in the SMM group than the non-SMM group. However, we defer to the judgment of the Editor should she prefer to eliminate Appendix B.

REVIEWER 3, COMMENT 12:

Results: Line 199: There are too many n = ... in this sentence. I don't know which number goes with which outcome. Please also include percentages.

The "n=" correspond to the number of women who presented to the ED or were admitted to the hospital with ICD-9-CM coding for the primary outcome.

Revised Text (Results, Page 11, Lines 226-228):

"After singleton liveborn deliveries without SMM, 1.6% of women (n=19,279) received primary or secondary diagnosis codes for psychiatric illness within one year of hospitalization discharge, during either ED presentations (n=12,328) or hospital admissions (n=7041)."

REVIEWER 3, COMMENT 13:

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Results: Line 212-215: This relates to my prior confusion regarding the interaction - why does this matter? What do you do with this information?

We hope our response to Reviewer 3, Comment 9 explains the interaction testing better than prior. We have also amended the Structured Discussion to explain that the interaction testing helped us identified sociodemographic factors associated with increased risk of postpartum psychiatric morbidity after deliveries with SMM.

Revised Text (Structured Discussion, Page 17, Lines 351-355):

"Our identification of specific morbidities within the SMM composite and specific maternal demographic factors that were associated with significantly increased risk of acute postpartum psychiatric illness and substance misuse—like acute renal failure, acute MI, tracheostomy, and sickle cell disease with crises— lays the groundwork for additional investigations into risk factors associated with adverse postpartum psychiatric morbidity."

REVIEWER 3, COMMENT 14:

Results: Line 229, 233, 235: Why split these all up? And are postpartum psychiatric morbidity and postpartum psychiatric condition different? This paragraph could be condensed.

Thanks for this comment. We split our findings up between more common and less common conditions within the SMM composite. Postpartum psychiatric morbidity and postpartum psychiatric condition are the same; using the same terminology to refer to our primary outcome in the same paragraph felt too repetitive. We have condensed this paragraph.

Revised Text (Results, Pages 13-14, Lines 267-273):

"Some of the more common conditions within the SMM composite were associated with increased risk of postpartum psychiatric morbidity, including acute renal failure and adult respiratory distress syndrome (unadjusted OR 3.87 (95% CI 2.72 – 5.51) with 4.4% risk attributable to SSM; unadjusted OR 3.67 (95% CI 2.81 – 4.80) with 4.1% risk attributable to SMM; respectively). Other conditions, including acute myocardial infarction and temporary tracheostomy occurred rarely but were associated with particularly high risk for postpartum psychiatric morbidity during an ED or inpatient hospitalization."

REVIEWER 3, COMMENT 15:

Results: Line 237-239: Please clarify this sentence.

Thanks for helping us improve our manuscript's readability.

Revised Text (Results, Page 14, Lines 276-278):

"The majority of SMM conditions, including blood transfusion, were associated with increased risk of coding for substance misuse during presentation in the ED or inpatient hospitalization coded within one year after discharge from delivery hospitalization."

REVIEWER 3, COMMENT 16:

Results: Line 241-243: Likely many of the patients with sickle cell disease used narcotics prior to pregnancy. It seems strange to highlight this association given that.

The substance abuse composite includes ICD-9-CM diagnosis codes for alcohol or recreational drug use, abuse, or dependence, not prescribed narcotic use. Though patients with SCD are more likely to use narcotics prior to pregnancy, antepartum narcotic use should be unrelated to the substance use outcome. However, given multiple reviewers and the Editor commented on the potential confounding with antepartum narcotic use among women with SCD and postpartum substance use, we agree that this sentence should be removed from the manuscript for clarity.

Revised Text (Results, Page 14, Lines 281-283)

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Text deleted

REVIEWER 3, COMMENT 17:
Results: Overall there are SO many tables. I got lost reading the manuscript, meaning that I feel like I lost sight of what you are actually trying to convey and why. I would recommend either cutting down on the number of tables, or very clearly and succinctly stating why you did each analysis.

We did include many tables, which present our analyses of our primary and secondary outcomes as well as multiple sensitivity analyses. We think we have clearly and succinctly stated why we did each analyses in the methods and also highlighted the implications of these analyses in the structured discussion. We also view the sensitivity analyses as a study strength. However, we have deleted Appendix D.5 to eliminate some tables and, to try to improve readability, reformatted each table within Appendix D. Should the Editor desire to remove more tables, we will defer to her judgment.

Revised Text (Results, Page 14, Lines 291-292):
"Removing psychosis as a condition for exclusion and from the primary outcome composite did not significantly change the findings (**Appendix D.4**)."
AND
Revised Text: Appendix D
AND
Revised Text: Deleted Appendix D.5

REVIEWER 3, COMMENT 18:
Table 1: Why do you put OR rather than just p values? Do you need either?

We chose to present unadjusted OR in Table 1 to allow us to present the risk each individual subgroup of sociodemographic factors has for SMM during liveborn singleton when compared to that factor's reference group. This allowed us conduct more granular analyses instead of using one p value to describe differences between the two groups for each sociodemographic factor overall (of note, each factor Table 1 had all $p < 0.0001$). Often, when OR are presented, p values are not as the 95% CI demonstrates the statistical significance of the finding. However, if the Editor prefers we add the p values, we will happily do so.

REVIEWER 3, COMMENT 19:
Appendix C: I remain confused by this. Is your dependent variable SMM or psychiatric illness?

Refer to our response to Reviewer 3, Comment 9.

REVIEWER 3, COMMENTs 20-23:
Appendix D1: Your headers here are different for table 1 and 2. Is severe intrapartum morbidity the same as SMM?
Appendix D2 Table 3 and 4: Your headers here are again different. Is severe intrapartum morbidity the same as SMM?
Appendix D4 Table 3 and 4: Same comment about the headers.
Appendix D5 Table 2: Same comment about the headers

We have reformatted all tables within Appendix D to improve readability and consistency.
Revised Text: Appendix D

REVIEWER 3, COMMENT 24:
Structured Discussion: What is the baseline population risk of psychiatric presentation to the ED and admission? This would be a helpful number to compare your results too, especially because you say that these numbers are not uncommon in the postpartum period.

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We agree that this would be a helpful number to compare our results to. Unfortunately, we cannot calculate the baseline population risk of psychiatric presentation to the ED and admission with the databases we used for these analyses. In HCUP, the only way we can follow women of reproductive age who didn't have a delivery is if they were hospitalized for something else, which would mean the reference population of women would not necessarily be representative of the overall reproductive age population. In order to obtain the baseline population risk, we would need access to claims data that includes information for all women in the population, not just women who presented for medical care to the ED or were admitted to the hospital.

REVIEWER 3, COMMENT 25:
Structured Discussion: Overall, it is worth pointing out some more specifics in your Structured Discussion - which conditions especially place women at risk? You have so much in your results that it is easy to get lost. The Structured Discussion is the place to re-focus.
> Thank you for this comment. We have refocused our Structured Discussion to highlight specific conditions within the SMM composite that are associated with increased risk of postpartum psychiatric morbidity.
> **Revised Text (Structured Discussion, Page 15, Lines 303-305:**
"We also identified specific conditions within the SMM composite—both common (adult respiratory distress syndrome) and rare (acute myocardial infarction)—that were associated with increased risk of psychiatric morbidity and substance use."
> **AND**
> **Revised Text (Structured Discussion, Page 17, Lines 351-355):**
"Our identification of specific morbidities within the SMM composite and specific maternal demographic factors that were associated with significantly increased risk of acute postpartum psychiatric illness and substance misuse—like acute renal failure, acute MI, tracheostomy, and sickle cell disease with crises—lays the groundwork for additional investigations into risk factors associated with adverse postpartum psychiatric morbidity."

REVIEWER 3, COMMENT 26:
Structured Discussion: The last sentence of your paper should be something stronger than the statement that this could be made into a useful clinical calculator. While interesting, that sentence is a bit of a weak concluding sentence.
> We have formatted our Structured Discussion to strengthen the implications of findings through a public-health lens.
> **Revised Text (Structured Discussion, Page 21, Lines 443-445):**
"These findings suggest that additional psychosocial support—as well as access to medical and mental healthcare—should be available to all women in the year after delivery, particularly those with intrapartum SMM in the first four months after hospital discharge."
> **AND**
> **Revised Text (Structured Discussion, Page 21, Lines 445-447):**
> Text deleted

Reviewer 4

REVIEWER 4, COMMENT 1:
Abstract: Line 50 "We compared outcomes after delivery with versus without SMM...." - this should be worded more clearly by stating "we compared women with SMM to those who did not have SMM using..."
> This revision clarifies the abstract; thank you.

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Revised Text (Abstract, Page 4, Lines 59-60):
"We compared women with SMM to those without SMM using multivariable logistic regression adjusting for sociodemographic factors and medical comorbidities."

REVIEWER 4, COMMENT 2:
Introduction: Line 92- define or give examples of "pregnancy complications"
 This study included first trimester miscarriage among the reported pregnancy complications, which also included stillbirth, ectopic pregnancy, serious genetic abnormality, operative vaginal delivery, or any complication leading to prolonged neonatal hospitalization. We have highlighted that the majority of complications referred to miscarriage.
Revised Text (Introduction, Page 6, Lines 91-93):
"The only published data available from the United States are limited by small sample size and self-disclosure of "pregnancy complications," of which nearly 75% were first trimester miscarriage.[15]"

REVIEWER 5, COMMENT 3:
Introduction: I also think the introduction could benefit from some mention of why SMM is important. It might be worth while to point out the long term cost both to families and our economy of SMM -- why should people care about this who are not in Obstetrics?
 Thank you; we agree and have added content in the introduction to emphasize the significance of SMM.
Revised Text (Introduction, Page 6, Lines 82-83):
"SMM has been associated with maternal mortality, increased hospital costs, and prolonged delivery hospitalizations.[4]"

REVIEWER 6, COMMENT 4:
Methods: Line 119- Why did you exclude hospitalizations or ED encounters for 252 days? Is there a significance for that number?
 We chose 252 days prior to date of admission for delivery hospitalization because that is 252 days is nine months, which is a proxy for pregnancy. We have amended the methods to highlight this.
Revised Text (Methods, Page 7, Lines 122-125):
"In addition, women coded during an inpatient hospitalization or ED encounter in the 252 days (9 months, a proxy for pregnancy) prior to hospital discharge of the index delivery for any condition within the primary and secondary composite outcomes were excluded from analysis due to pre-existing psychiatric morbidity or substance misuse."

REVIEWER 4, COMMENT 5:
Methods: Line 143- "....may have had more than one diagnosis within the composite but were only counted once per composite...." - can you clarify how you made the decision to select which diagnosis you utilized if they presented more than once with more than one diagnosis? Would it have been the first time they presented period?
 Please refer to our response to Reviewer 3, Comment 5.

REVIEWER 4, COMMENT 6:
Results: Table 3 and 4 state that the annotation of subscript "2" is unable to report because there are less than 11 women but the N's suggest there were more than 11 woman. It's a little unclear.
 We apologize for this confusion. The total number of women who had the condition within the SMM composite was more than 11, but the number of women with that condition who then developed acute

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psychiatric morbidity within a year of hospital discharge was less than 11. We have clarified this in the methods as well as in the Tables and Appendices

Revised Text (Methods, Page 9, Lines 172-173)

"Due to HCUP restrictions designed to preserve patient privacy, counts <11 for an exposure were presented as n<11 while counts <11 for an outcome were reported as "--"."

AND Tables 2, 3 and 4
AND Appendix D

REVIEWER 4, COMMENT 7:

Structured Discussion: Line 285- 288- I think this sentence should be rewritten, eliminating "normal" and "routine".

We have edited this sentence.

Revised Text (Structured Discussions, Page 16, Lines 329-332):

"The finding that postpartum psychiatric morbidity was not uncommon after delivery of a liveborn singleton without SMM aligns with recent data suggesting that even routine deliveries and postpartum periods can be a powerful stimulants for psychiatric illness,[7,8] particularly postpartum depression and anxiety.[28]"

REVIEWER 5, COMMENT 8:

Structured Discussion: Line 368-369- There are several mentions of this clinical calculator. I think I understand at what you're getting at but unless you're actively working on this concept, I don't think you need to include that in this manuscript.

We have deleted the mention of the clinical calculator in the last section of the Structured Discussion.

Revised Text (Structured Discussions Page 21, Lines 445-447)

Text Deleted

STATISTICAL EDITOR

STATISICAL EDITOR, COMMENTS 1-3:

Table 2: Should include crude ORs to contrast with aORs. Should (could be in separate Table), the proportion of each psychiatric morbidity that might be attributed to SMM. That is, the aORs may be significantly increased compared to the referent, but the actual proportion attributable to SMM is low (typically < 1-2% of all psychiatric morbidity). Also, the counts among the SMM cohort for PTSD, acute stress reaction, adjustment disorder or alcohol use/dependence are each too few to allow for adjustment for 6 variables.

Table 3: Same comment re: need to include ORs, Need to state attributable risk %s and insufficient counts to adjust for 6 variables for all rows except for overall, transfusion, composite, ie, all the subsets for composite severe without transfusion.

Table 4: Same comment re: need to include ORs, Need to state attributable risk %s and inability to apply aORs to all subsets of composite SMM without transfusion.

We have added unadjusted odds ratios and attributable risks to Tables 2-4 and removed the adjusted Odds Ratios for all conditions within the composite in which 50 women or less were coded. We have also amended the methods, results, and Structured Discussion accordingly.

Revised Text (Methods, Page 9, 160-169):

"Unadjusted logistic regression was conducted for all primary and secondary outcomes. In addition, multivariable logistic regression analyses were conducted for all conditions within the psychiatric and substance abuse composites that had more than 50 women with SMM coded for that individual outcome.

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Multivariable logistic regression analyses were adjusted for differences between sociodemographic and medical factors of women with versus without SMM during delivery hospitalization that were identified to be statistically significant: age, race/ethnicity, payer, income quartile by zip code, mode of delivery, and maternal medical comorbidities. Sickle cell disease and eclampsia were conditions included within the SMM composite, but some ICD-9-CM codes used for these conditions were also included within the morbidity composite; thus, we were unable to adjust for Sickle cell disease or eclampsia in the multivariable models."

AND

Revised Text (Methods, Page 10, Lines 177-179):

"Unadjusted logistic regression was conducted for all conditions within the SMM composite. In addition, multivariable logistic regression analyses were conducted for blood transfusion and for the SMM composite excluding blood transfusion."

AND

Revised Text (Results, Page 13, Lines 229-235):

"The risk of readmission or ED encounter with coding for psychiatric illness was nearly 75% higher after deliveries with SMM (n=452 (2.9%) with 222 admissions and 230 ED visits; adjusted odd ratios (aOR) 1.74 (95% Confidence Interval (CI): 1.58 – 1.91); risk attributable to SMM: 1.3%). Depression and anxiety were the most common psychiatric morbidities, and the risk was significantly higher among deliveries with SMM versus without SMM for both conditions (aOR 1.86 (95% CI 1.60 – 2.16) for depression with 0.6% risk attributable to SMM; aOR 1.78 (95% CI 1.56 – 2.03) for anxiety with 0.7% risk attributable to SMM).

AND

Revised Text (Results, Page 12, Lines 237-240):

"Women also had nearly two times higher risk of being coded for drug or alcohol use or dependence in an inpatient admission or ED encounter in the year following delivery with SMM than without SMM (168 (1.1%) versus 6856 (0.6%); aOR 1.89 (95% CI 1.62 – 2.21); 0.5% risk attributable to SMM)."

AND

Revised Text (Results, Page 13, Lines 267-271):

"Some of the more common conditions within the SMM composite were associated with increased risk of postpartum psychiatric morbidity, including acute renal failure and adult respiratory distress syndrome (unadjusted OR 3.87 (95% CI 2.72 – 5.51) with 4.4% risk attributable to SSM; unadjusted OR 3.67 (95% CI 2.81 – 4.80) with 4.1% risk attributable to SMM; respectively)."

AND

Revised Text (Results, Page 14, Lines 273-274):

Text deleted

AND

Revised Text (Results, Page 14, Lines 278-281):

"Some of the more common conditions within the SMM composite were associated with high risk of the substance use composite, including sepsis (unadjusted OR 4.20 (95% CI 2.86 – 6.15)) and pulmonary edema (unadjusted OR 2.41 (95% CI 1.60 – 3.62))."

AND

Revised Text (Structured Discussion, Page 15, Lines 301-303):

"However, the risk was higher for both the psychiatric morbidity composite and substance use composite when deliveries with SMM were compared to those without SMM, though the absolute numbers and risk attributable to SSM were both modest."

AND

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Revised Text (Structured Discussion, Page 21, Lines 437-440):
"In conclusion, we found that postpartum psychiatric illness and substance use within one year of hospital discharge occurs significantly more often among women who suffer intrapartum severe maternal morbidity during delivery of a liveborn singleton compared to those who do not have SMM, though the absolute numbers and risk attributable to SMM were both modest."

AND

Revised text (Tables 2-4)

STATISTICAL EDITOR, COMMENT 4:

Appendix B: Should include attributable risk %s.

We have updated the Appendix as requested, as well as added headings that should help clarify this Appendix to the reader

Revised Text: Appendix B

STATISTICAL EDITOR, COMMENT 5:

General: As a sensitivity analysis, if one were to change the exclusion/inclusion criteria to include any prior psychiatric history, rather than just in 9 months preceding delivery, do the Structured Discussions change?

One of our concerns was that 9 months preceding delivery may be insufficient to capture adequately pre-existing psychiatric morbidity. Thus, we performed sensitivity analyses excluding women who had psychiatric morbidity or substance use in the two years prior to delivery (Appendix D.2). However, we did not change the exclusion/inclusion criteria to include any psychiatric history prior to delivery because this would result in a non-standard antecedent time period based on delivery year. In other words, women who delivered in 2005 would only have a few months prior to delivery in which psychiatric morbidity could be captured, whereas those who delivered in 2014 would have nearly a decade of pre-delivery time. This discrepancy would result in an ascertainment bias that would significant undermine our findings.

EDITOR

EDITOR, COMMENT 1:

These are not mutually exclusive, as your study shows. Acute psychiatric illness was uncommon in both those with and without SMM, but increased w/ SMM.

Excellent point. We have changed our hypothesis due to a prior comment (please see Reviewer 3, Comment 3) and believe the updated version also responds to this comment.

EDITOR, COMMENT 2:

As noted by one of your reviewers, the use of hospital discharge rather than delivery date is problematic. Women with SMM tend to have longer LOS after delivery which extends the 1 year time period beyond the birth date. While in most cases this is only a matter of a few days, in some in much longer. Can you do this analysis w/ HCUP data using birth date? Granted, some SMM occurs only in the post partum period so that may be an offsetting reason for using discharge data. Either way, it needs to be addressed in the Structured Discussion.

We hope our response to Reviewer 1, Comment 4 adequately addresses why we elected to use hospital discharge rather than delivery date, how this decision could impact our findings, and how we edited the manuscript to better describe our methodology and the limitations of our methodology.

EDITOR, COMMENT 3:

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In Structured Discussion, please explain these exclusions.

Our response to Reviewer 3, Comment 4 explains why we excluded stillbirths and multiple gestation from our analyses and how the manuscript was updated to explain these exclusions.

EDITOR, COMMENT 4:
Please put the CDC SMM composite information in a box in the paper.

Please refer to newly created Figure 1.

Revised Text (Methods, Page 8, Lines 132-133):

"The primary exposure was the CDC's SMM composite (**Figure 1**); the individual indices were extracted using ICD-9-CM diagnosis and procedure codes as defined by the CDC[3] (**Appendix A.2**)."

   AND
   Revised Text: Figure 1

EDITOR, COMMENT 5:
I'm not sure what to make of describing this as a range. Is anemia within the range, for instance? If so, does that mean it is more severe than asthma?

The word "range" was meant to signify "from A to Z," not in a medical range of severity. We apologize for the confusion and have edited the manuscript accordingly.

Revised Text (Results, Page 11, Lines 222-223):

"They were also more likely to have medical comorbidities."

EDITOR, COMMENT 6:
Please note that effect sizes (RR, OR) within the zone of potential bias should be noted as weak. Those effect sizes in the zone of potential interest should be emphasized in the Structured Discussion. . (Ref: False alarms and pseudo-epidemics. The limitations of observational epidemiology. Grimes DA, Schulz KF. Ob Gyn 2012;120:920-7)

This is an excellent point that we have now highlighted as a limitation to our study. Thank you.

Revised Text (Structured Discussion, Page 19, Lines 403-407):

"Furthermore, the unadjusted and adjusted odds ratios for most of our findings are less than 4, which is within the zone of potential bias.[48] Thus, despite the fact that our findings persisted in multiple sensitivity analyses— which may decrease the impact bias has on our results—the relatively weak association we found between SMM and our primary outcome could be the result of a Type I error."

EDITOR, COMMENT 7:
As noted, SMM is not associated with the great majority of cases of psych.illness. In the Structured Discussion, you will need to address, given these issues, why this is important.

We agree that the absolute numbers of women with SMM (and without SMM) who present for postpartum psychiatric illness were modest, and the risk attributable to SMM was low. However, our findings have significant implications clinically and in terms of public health. In particular, the additional cox regression modeling performed for this revision provides insight into the need for an increased duration of medical and mental health coverage postpartum as the risk for psychiatric morbidity extended until 4 months after hospital discharge.

Revised Text (Structured Discussion, Page 15, Lines 301-309):

"However, the risk was higher for both the psychiatric morbidity composite and substance use composite when deliveries with SMM were compared to those without SMM, though the absolute numbers and risk attributable to SSM were both modest. We also identified specific conditions within the SMM composite—both common (adult respiratory distress syndrome) and rare (acute myocardial

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infarction)—that were associated with increased risk of psychiatric morbidity and substance use. Lastly, we identified that the highest risk of postpartum psychiatric morbidity was in the first four months after hospital discharge from delivery with SMM vs without SMM (adjusted hazard ratio 2.53 (95% CI 2.05 – 3.12)), suggesting that access to medical and mental healthcare is needed beyond the six-week postpartum period traditionally covered by public health insurance."

AND

Revised Text (Structured Discussion, Pages 16-17, lines 340-349):

"In addition, our findings could have significant public health ramifications for postpartum care in the United States. Though the absolute number of women with risk attributable to SMM was modest, we found that women who have SMM have a significantly increased risk of postpartum psychiatric morbidity for a year after hospital discharge from their delivery, and that this risk was highest in the first four months. Women with increased risk of SMM were non-White, in the lowest income quartiles by zip code, and had public insurance, which often discontinues at six weeks postpartum. Thus, women at higher risk for SMM may be left without access to medical or psychiatric care during the highest risk time for acute psychiatric morbidity, which extends for months after their hospital discharge. Our findings suggest that public health insurance should extend medical and mental health insurance benefits beyond six weeks postpartum, ideally for all women but particularly for those who have SMM during delivery."

AND

Revised Text (Structured Discussion, page 21, lines 437-440):

"In conclusion, we found that postpartum psychiatric illness and substance use within one year of hospital discharge occurs significantly more often among women who suffer intrapartum severe maternal morbidity during delivery of a liveborn singleton compared to those who do not have SMM, though the absolute numbers and risk attributable to SMM were both modest."

AND

Revised Text (Structured Discussion, page 21, lines 443-445):

"These findings suggest that additional psychosocial support—as well as access to medical and mental healthcare—should be available to all women in the year after delivery, particularly those with intrapartum SMM in the first four months after hospital discharge."

EDITOR, COMMENT 8:

Could this information (lines 208-210) be related to higher rates of discharge opioids in women with SMM leading to higher rates of dependence? This would be a different reason (although also very important) than an increased use due to psychiatric disorders resulting from the SMM and not its treatment.

This is interesting. As stated in our response to Reviewer 3, Comment 16, the substance abuse composite includes ICD-9-CM diagnosis codes for alcohol or recreational drug use, abuse, or dependence, not prescribed narcotic use. Furthermore, our database unfortunately does not include whether women receive prescriptions for narcotics at hospital discharge (much less the number of morphine milliequivalents prescribed at discharge), so we are unable to tell whether the increased rate of discharge narcotics received by women having SMM during delivery correlates to increased rates of opioid use disorder. This would be an interesting research question that would require a more granular retrospective database or prospective design.

EDITOR, COMMENT 9:

These are not significant and you have quite large numbers. We do not allow authors to describe variables or outcomes in terms that imply a difference (such us of the terms "trend" or "tendency" or "marginally different") unless there is a statistical difference. Please edit here and throughout.

Thank you for pointing this out. We have modified how we present this analysis to more accurately reflect that there is no statistically significant difference.

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Revised Text (Results, Page 13, Lines 251-254)

"The *p* values for interactions were not statistically significant for payer type or mode of delivery (*p* = 0.07 and *p* = 0.05, respectively) but were statistically significant for race/ethnicity (*p* = 0.003), income quartile by zip code (*p* <0.001), and maternal co-morbidities (*p* <0.001)."

EDITOR, COMMENT 10:
Perhaps due to prior drug dependence?
We have removed this sentence (refer to our response from Reviewer 3, Comment 16).

EDITOR, COMMENT 11:
Please temper this based on the above. Yes, it was significantly higher but about 3% vs 1.5% isn't a huge difference. Would care post partum differ prospectively in these 2 groups based on these findings?
We have tempered our Structured Discussion to highlight the modest attributable risk to SSM. For the importance of our findings, including how postpartum care may need to differ between the two groups, please refer to our response to EDITOR, COMMENT 7 .

EDITOR, COMMENT 12:
- This is called a primacy claim: yours is the first, biggest, etc…In order to assert that, you need to provide the search terms used and the data base (s) searched (PubMed,GOogle Scholar, etc) to substantiate the claim. Otherwise, it needs to be deleted. It wouldn't belong in the abstract anyway, so make sure you address this in the manuscript body.
Thank you for this comment. We have deleted our primacy claim and edited the manuscript as per Reviewer 1, Comment 9.

EDITOR, COMMENT 13:
Please note other limitations from comments above.
We have added additional limitations to our manuscript per the comments from the four reviewers.

EDITOR, COMMENT 14
Why no data but a +aOR for MI? and conversely, data but no aOR for aneurysm? All true for other examples.
Please refer to our response to Reviewer 4, Comment 6.

2. The Editors of Obstetrics & Gynecology are seeking to increase transparency around its peer-review process:
   OPT-IN

Please let us know if there remain any questions or if you feel there are additional ways in which we could improve this manuscript.

Thank you for your consideration,

Adam K. Lewkowitz, MD, MPHS
Fellow, Division of Maternal-Fetal Medicine