

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Effectiveness of the facility based maternal near-miss case reviews in improving maternal and newborn quality of care in low and middle income countries: a systematic review
AUTHORS	Lizzerini, Marzia; Richardson, Sonia; Ciardelli, Valentina; Erenbourg, Anna

VERSION 1 – REVIEW

REVIEWER	Dr Bettina Bottcher Islamic University of Gaza Palestine
REVIEW RETURNED	08-Oct-2017

GENERAL COMMENTS	<p>Interesting paper</p> <p>Abstract</p> <p>Uses the 'First Person', I would advice to reformulate and cut this out of the manuscript.</p> <p>Background is good, Objective is clear. However, do you feel that it is possible to determine the primary outcome of 'maternal mortality' within the context of this review? Here we have only 8 studies who reported maternal mortality, only 3 showing a reduction. But numbers are small in all. More importantly though: are these effects actually due to the NMCR done or to other factors ? How can this be conclusively determine in the multifactorial causation of most cases of maternal mortality? As the design of all studies was either ITS or NCBA during the intermittent timespan, a number of other interventions, changes and events might have taken place and influenced the outcome of 'maternal mortality'.</p> <p>Secondary outcomes are more specific and easier to be confirmed to be due to the NMCRs.</p> <p>Background:</p> <p>This part of the text is mainly based on the WHO reference only and does not look at NMCRs from other perspectives. But Objective is clear, although my question from above applies here to: IS the primary outcome really achievable with this review. ?</p> <p>Overall the review follows the PRISMA.</p> <p>Methods</p> <p>Inclusion definition: WHO 2011 is clear, but 'locally adopted definition' is unclear and might be heterogeneous.</p> <p>Subgroup Analysis of low and middle income countries alone: What is the purpose of this in this context ? It does not add much, and might risk to artificially inflate the effect shown in the results. It</p>
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	<p>reduces credibility and should be left out.</p> <p>Results In view of the small numbers and multiple unknown and undisclosed factors possibly also influencing maternal mortality simultaneously and during the study period, can you really say that the review is evidence that NMCRs significantly reduce maternal mortality ? In a number of studies, the reported interventions (policy writing, guideline development, staff training etc) is also reported as outcome of the NMCRs. These interventions should not be included as structural outcome if they are the actual interventions of the NMCRs.</p> <p>Discussion The issue of Patient satisfaction. All these issues are important concerns in the result of patient satisfaction. Additionally, it is also known that patient satisfaction is often high, despite outcome and circumstance, when assessed at the time of receiving care or shortly after due to the positive effect the care received has on the patient.</p> <p>Language: Very minor and few linguistic mistakes were present in the manuscript. I have highlighted them in yellow or beige colour. The blue highlights are also discussed in this text and concerned with the content rather than the language.</p>
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REVIEWER	Barbara Madaj, Head of Monitoring and Evaluation Liverpool School of Tropical Medicine, United Kingdom
REVIEW RETURNED	18-Oct-2017

GENERAL COMMENTS	<p>Overall, this is an interesting paper which contributes to the body of knowledge and is therefore worthy of a publication. While generally clear and well presented, there are some suggestions for the authors to consider in the revision of the paper. These are presented below.</p> <p>The presentation of the results is at times unclear and would benefit from a thorough review, especially with regard to tables and figures. It is not always easy to link the materials presented in those with the text and therefore it may be helpful to condense the tables and figures to extract only the relevant and critical information. If the authors feel the contents of the tables and figures need to be preserved, then the formatting of these needs to be improved.</p> <p>It would be useful if the authors could refine the discussion and conclusions; although interesting and important points are raised, the discussion includes some new information from the analysed studies which were not presented in the results section and the depth of the critical review and recommendations could be improved to increase the standard of the article and its contribution.</p> <p>Although understandable, the paper is riddled with stylistic, grammatical and typographical errors (punctuation, spacing), as well as poor formatting (use of different fonts and especially for tables and figures are not presented in a user friendly layout and use abbreviations which are not explained or use abbreviated and full names, as well as containing superfluous or unnecessary information), which distract the reader from being able to engage</p>
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	<p>with the presented contents. It would be advisable to have the article thoroughly proofread and edited before it is finalised.</p> <p>The Reference section requires a review to amend mistakes.</p> <p>PRISMA checklist needs to be revised to correct page and table references as these do not always match the contents of the article.</p> <p>PRISMA diagram should be formatted according to the existing standard, i.e. use of (n=xx) for referring to the number of records.</p>
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REVIEWER	<p>Andrew J Scally School of Allied Health Professions and Midwifery Faculty of Health Studies University of Bradford UK</p>
REVIEW RETURNED	05-Nov-2017

GENERAL COMMENTS	<p>My comments are focused primarily on statistical aspects of this systematic review.</p> <p>Unfortunately, the authors seem to have erred in their data extraction. In figure 2, p36, there is an implausibly wide variation in the denominators for the calculation of the before-and-after odds ratios (9 to 2,944,360 in the after period). I checked the Mohd Azri (2015) paper and it is clear that the denominator should be the total number of women seen in the maternity unit over the two audit periods. This number is in the tens of thousands. The denominators extracted from the paper relate only to those women diagnosed with eclampsia (9 in the second audit period). Given that the weightings in the meta-analysis are largely driven by the size of the denominator, it is essential that this information is accurately determined. Also, for the Mohd Azri paper, the numerators (one and two deaths, respectively) only relate to women with eclampsia. The numerator should be all-cause maternal mortality. All included papers should be thoroughly rechecked and re-evaluated to ensure that where data is extracted for the purpose of a meta-analysis, the data from all the papers are sufficiently compatible in each analysis. Although I would be willing to review a revision of the manuscript, I would expect reassurance that the data extraction has been accurately revised. I strongly advise the assistance of a statistician in the revision.</p> <p>A second methodological issue is that, in the methods section, the authors say that the presence of heterogeneity will be assessed using I² and Cochrane's Q, with p=0.05 set as the threshold for significance. This is the correct approach, but they seem to have ignored the outcome of the tests for heterogeneity. For the primary outcome measure (maternal mortality), I² is 39%, which is not exactly 'low'. In the sensitivity analysis (Figure S1, p39) the p-value for the test of heterogeneity is <0.001 and the I² is 86%, yet the authors seem only to have performed a fixed-effect model analysis rather than using a random-effects model.</p> <p>Incorrect analysis tables seem to have been incorporated on p40 under 'pooled effect by country income'. The data seems to be a repeat of the earlier sensitivity analysis.</p>
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	<p>The risk of bias table is not very informative and can be reduced to a single sentence in the text, since there is no discrimination between studies.</p> <p>Reference 57 is not cited or discussed in the text.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Dr Bettina Bottcher

Institution and Country: Islamic University of Gaza, Palestine

Please state any competing interests or state 'None declared': None declared

Interesting paper

*** Thank you for the appreciation

Abstract

Uses the 'First Person', I would advice to reformulate and cut this out of the manuscript.

*** Did you mean the "first person plural (we) in the second line of the abstract ? We have revised this. Otherwise, where else have we used the first person in the abstract?

Background is good, Objective is clear. However, do you feel that it is possible to determine the primary outcome of 'maternal mortality' within the context of this review? Here we have only 8 studies who reported maternal mortality, only 3 showing a reduction. But numbers are small in all. More importantly though: are these effects actually due to the NMCR done or to other factors ? How can this be conclusively determine in the multifactorial causation of most cases of maternal mortality? As the design of all studies was either ITS or NCBA during the intermittent timespan, a number of other interventions, changes and events might have taken place and influenced the outcome of 'maternal mortality'.

Secondary outcomes are more specific and easier to be confirmed to be due to the NMCRs.

*** We agree with the reviewer. In the discussion section, third paragraph, we have emphasised the limitation of this review, among which we mention the study designs and the low sample size.

Background:

This part of the text is mainly based on the WHO reference only and does not look at NMCRs from other perspectives. But Objective is clear, although my question from above applies here to: IS the primary outcome really achievable with this review.?

Overall the review follows the PRISMA.

*** Thank you for the appreciation and inputs.

The choice of the primary outcome is not based on the results of the existing literature, but on the importance of measuring the effectiveness of the intervention against a "hard outcome." We have developed a protocol for the review before looking at the individual studies, as suggested by PRISMA and by the Cochrane manual.

As a matter of a fact we agree with you that measuring a significant reduction in mortality may be possible only in setting with high in-hospital maternal mortality, or with very large sample size. We have further stressed this important point in the discussion section. In the discussion we underscore the need for large multicenter studies. For our knowledge there is ongoing discussion on identifying resources for such studies (but there is no protocol or further detail that we can cite in this paper).

Methods

Inclusion definition: WHO 2011 is clear, but 'locally adopted definition' is unclear and might be heterogeneous.

*** The reason for including 'locally adopted definition' are mainly two: 1) The 2011 definition was developed relatively recently, and just to stick to that one will imply not assessing all the previous literature (which may bring to bias in results); 2) the WHO manual itself suggests to locally adapt the WHO definition. We believe that case definitions (eg whether you define sepsis) are not the crucial aspect of the audit cycle. The important thing is that severe cases are somehow selected (not necessarily all of them) discussed, and that recommendations to improve quality are made. Based on experience and on literature, recommendations emerging from audit are most often generic and not "condition-specific". To explain this better: even where the case selection was limited at few conditions (eg where only cases of eclampsia or haemorrhage were discussed) recommendations went beyond those related to the management of the specific condition, and usually cross-cutting themes emerged, such as the need for better communication among staff, better patient's monitoring, etc). The recommendations deriving from the discussion of a specific type of obstetric emergency (or a specific case definition) can indeed have an impact on the overall reorganisation of services (eg having a doctor available 24/24 H).

Subgroup Analysis of low and middle income countries alone: What is the purpose of this in this context ? It does not add much, and might risk to artificially inflate the effect shown in the results. It reduces credibility and should be left out.

*** A subgroups analysis does not inflate the effect since it reduces the total number of studies and therefore the power of the meta-analysis. Subgroup analysis is recommended by the Cochrane as a way of exploring heterogeneity, but it is to be regarded as only additional analysis in any case. The logic behind comparing "low income" vs "middle income" is that the effectiveness of the NMCR may be different in these two groups, due to context factors. In our specific case this subgroup analysis does not inflate the effect of the intervention. However, if there the editors or other reviewer feel that it reduces credibility, we are happy to leave it out. Heterogeneity on the primary analysis is low (39%), therefore subgroup analysis may be reasonably omitted.

Results

In view of the small numbers and multiple unknown and undisclosed factors possibly also influencing maternal mortality simultaneously and during the study period, can you really say that the review is evidence that NMCRs significantly reduce maternal mortality ?

*** Actually, in the article summary we state that the NMCR "may be effective in reducing maternal mortality, and in improving quality of maternal and newborn health care at facility level." In the discussion (first sentence and again in the second paragraph), we state that "This review suggests that the facility based individual maternal NMCR cycle may be an effective strategy for reducing maternal mortality in high burden countries, and for improving overall quality of maternal care in LMIC". In other words, we have mostly used the conditional form (may be effective), as recommended by the Cochrane and GRADE. We have further double checked the discussion section in order to be consistent with the use of conditional verbs.

In a number of studies, the reported interventions (policy writing, guideline development, staff training etc) is also reported as outcome of the NMCRs. These interventions should not be included as structural outcome if they are the actual interventions of the NMCRs.

*** Thank you for this input. We have double checked and clarified this in the table and text. Actually all studies associated to the audits the development or implementation of standards of care, used also in most cases to perform the audits (this included also short training and use of guidelines, but only for the small team performing the audits) while few studies also associated additional interventions to the whole hospital staff, such as development/dissemination of guidelines, and training on case

management (references 13,15, 23). This has been clarified in the text. Table 4 (former table 6) reports now only the effects of the intervention.

For example, in Mgaya 207:

- Developing standard of care was part of the intervention, functional to perform the audits;
- Improved awareness on standards of care and additional training to increase their uptake was an effect of the audit (as a results of the audit the hospital staff decided to implement additional training)

We hope that the concept is now clear.

Discussion

The issue of Patient satisfaction. All these issues are important concerns in the result of patient satisfaction. Additionally, it is also known that patient satisfaction is often high, despite outcome and circumstance, when assessed at the time of receiving care or shortly after due to the positive effect the care received has on the patient.

*** We agree with the reviewer

Language:

Very minor and few linguistic mistakes were present in the manuscript. I have highlighted them in yellow or beige colour. The blue highlights are also discussed in this text and concerned with the content rather than the language.

*** Thank you. We have used the file the review the paper. Thanks !

Reviewer: 2

Reviewer Name: Barbara Madaj, Head of Monitoring and Evaluation

Institution and Country: Liverpool School of Tropical Medicine, United Kingdom

Please state any competing interests or state 'None declared': None declared

Overall, this is an interesting paper which contributes to the body of knowledge and is therefore worthy of a publication.

*** Thank you for the appreciation

While generally clear and well presented, there are some suggestions for the authors to consider in the revision of the paper. These are presented below. The presentation of the results is at times unclear and would benefit from a thorough review, especially with regard to tables and figures. It is not always easy to link the materials presented in those with the text and therefore it may be helpful to condense the tables and figures to extract only the relevant and critical information. If the authors feel the contents of the tables and figures need to be preserved, then the formatting of these needs to be improved.

*** Thank you for the appreciation

We have improved formatting of tables, preserving the content (only redundant content presented already in graphs or text has been removed). The table on the types of outcomes (possibly the less interesting one) has been moved as appendix. Table 1 is now shorter. Overall there are now 2 tables less, and we hope that tables are now more user friendly.

It would be useful if the authors could refine the discussion and conclusions; although interesting and important points are raised, the discussion includes some new information from the analysed studies which were not presented in the results section and the depth of the critical review and recommendations could be improved to increase the standard of the article and its contribution.

*** We have revised the discussion section, putting attention in not adding results not cited in the result section. The discussion is now more focused. It is also shorter.

Although understandable, the paper is riddled with stylistic, grammatical and typographical errors (punctuation, spacing), as well as poor formatting (use of different fonts and especially for tables and figures are not presented in a user friendly layout and use abbreviations which are not explained or use abbreviated and full names, as well as containing superfluous or unnecessary information), which distract the reader from being able to engage with the presented contents. It would be advisable to have the article thoroughly proofread and edited before it is finalised.

*** The article has been now double checked and revised by an English mother language speaking author (Sonia Richardson)

The Reference section requires a review to amend mistakes.

*** This have been double checked and mistakes have been amended

PRISMA checklist needs to be revised to correct page and table references as these do not always match the contents of the article.

*** This has been double checked and corrected

PRISMA diagram should be formatted according to the existing standard, i.e. use of (n=xx) for referring to the number of records.

*** We have now used exactly the same format as the PRISMA checklist

Reviewer: 3

Reviewer Name: Andrew J Scally

Institution and Country: School of Allied Health Professions and Midwifery, Faculty of Health Studies, University of Bradford, UK

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

My comments are focused primarily on statistical aspects of this systematic review.

Unfortunately, the authors seem to have erred in their data extraction. In figure 2, p36, there is an implausibly wide variation in the denominators for the calculation of the before-and-after odds ratios (9 to 2,944,360 in the after period). I checked the Mohd Azri (2015) paper and it is clear that the denominator should be the total number of women seen in the maternity unit over the two audit periods. This number is in the tens of thousands. The denominators extracted from the paper relate only to those women diagnosed with eclampsia (9 in the second audit period). Given that the weightings in the meta-analysis are largely driven by the the size of the denominator, it is essential that this information is accurately determined. Also, for the Mohd Azri paper, the numerators (one and two deaths, respectively) only relate to women with eclampsia. The numerator should be all-cause maternal mortality. All included papers should be thoroughly rechecked and re-evaluated to ensure that where data is extracted for the purpose of a meta-analysis, the data from all the papers are sufficiently compatible in each analysis. Although I would be willing to review a revision of the manuscript, I would expect reassurance that the data extraction has been accurately revised. I strongly advise the assistance of a statistician in the revision.

*** Thank you for your inputs. However, we have double checked the paper from Mohd Azri, and the number extracted are correct. The paper is titled "Audit on management of eclampsia at Sultan Abdul Halim Hospita". The intervention was the audit of cases of eclampsia, as a type of severe obstetric complication (thus fitting our inclusion criteria). As such, only total cases of eclampsia (denominator =51) and death cases due to eclampsia (nominator= 3) are reported. Total hospital maternal mortality is not reported in the paper and it will not be appropriate to use it, since the audits were only performed on cases of eclampsia, and not on all obstetric emergencies (so why this type of audit should impact total hospital mortality? It is more appropriate to measure disease-specific mortality)

We have also double checked again all other included papers in Figure 2 (two authors independently) and we have found no errors. Differences in denominators are due to differences in the sample size, with the study from Kongnyuy 2008 being a very large study in 73 health facilities across 3 districts (see Table 1). It is not uncommon in systematic review to include studies with large differences in the sample size. To further explore how sample size impact results, we already included a sensitivity analysis pooling only studies with at least 300 cases and 30 events (please see Figure S1 and S2).

A second methodological issue is that, in the methods section, the authors say that the presence of heterogeneity will be assessed using I^2 and Cochrane's Q , with $p=0.05$ set as the threshold for significance. This is the correct approach, but they seem to have ignored the outcome of the tests for heterogeneity. For the primary outcome measure (maternal mortality), I^2 is 39%, which is not exactly 'low'.

***We haven't ignored the results of the I^2 test. From the Cochrane manual:

"Thresholds for the interpretation of I^2 can be misleading, since the importance of inconsistency depends on several factors. A rough guide to interpretation is as follows:

- 0% to 40%: might not be important >> THIS IS OUR CASE WITH $I^2 = 39%$ for the primary outcome (figure 2)
- 30% to 60%: may represent moderate heterogeneity;
- 50% to 90%: may represent substantial heterogeneity;
- 75% to 100%: considerable heterogeneity.

In the sensitivity analysis (Figure S1, p39) the p-value for the test of heterogeneity is <0.001 and the I^2 is 86%, yet the authors seem only to have performed a fixed-effect model analysis rather than using a random-effects model.

*** The I^2 test on our primary analysis (figure 2) results in a value of 39%. S1 is just an additional analysis, and as such should not be regarded as the key one.

According to the Cochrane manual (see section 9.5.4 in the manual), REM should be applied only when the idea of a 'random' distribution of intervention effects can be justified, which we believe is not our case. Also, REM may exacerbate the effects of publication bias, and reduce the weight of small studies. As suggested by the Cochrane manual (section 9.5.4) we have included sensitivity analysis to further explore the effect of excluding small studies, as recommended approach.

Incorrect analysis tables seem to have been incorporated on p40 under 'pooled effect by country income'. The data seems to be a repeat of the earlier sensitivity analysis.

*** Thank you for this input, that was a mistake in inserting the image. We have now added the correct graph.

The risk of bias table is not very informative and can be reduced to a single sentence in the text, since there is no discrimination between studies.

*** The table is reported as appendix We still believe that it is more transparent to detail the ROB by single study, as also recommended by the Cochrane.

Reference 57 is not cited or discussed in the text

*** Many thanks you for this input, we have removed Ref 57, which was coming from a previous version of the paper+

VERSION 2 – REVIEW

REVIEWER	Andrew J Scally School of Allied Health Professions and Midwifery, Faculty of Health Studies, University of Bradford, UK
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REVIEW RETURNED	03-Jan-2018
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GENERAL COMMENTS	<p>I have read the authors' response to my comments and revisited the Mohd Azri paper. I accept that the intervention in this paper focused specifically on eclampsia. On re-reading the manuscript, I can see that the populations in the included papers vary significantly in their definition, with large variation in the range of conditions included, which explains the wide variation in sample size.</p> <p>This does, however, lead to a second issue - that of heterogeneity. Statistical tests of heterogeneity are generally of low power and should be used to supplement critical judgement rather than replace it. The wide variation in inclusion criteria in the papers included in this review inevitably mean that heterogeneity IS present. This also seems to be self-evident from the variation in the odds ratios from the different studies.</p> <p>Having said this, my concern would be the excessive influence of the Kongnyuy (ref 26) paper in a fixed-effect model due to its overwhelming weighting (72.5%), which would be reduced in a random-effects model. However, given that this most influential study in the pooled analysis pulls the effect towards the null, the overall effect (though likely to be slightly biased) is a conservative estimate so I would not insist on the authors re-analysing the data using a random-effects model, though they may wish to consider this in light of my comments as I believe this would improve the robustness of the paper.</p> <p>A very accessible brief discussion of fixed v. random effects can be found here: https://www.meta-analysis.com/downloads/M-a_f_e_v_r_e_sv.pdf</p>
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REVIEWER	Dr Bettina Bottcher Islamic University of Gaza, Palestine
REVIEW RETURNED	05-Jan-2018

GENERAL COMMENTS	<p>Thank you for re-submitting this interesting article after revisions. It is a pleasure to review it once more.</p> <p>The abstract is very nice: complete and clear.</p> <p>Please note that the line numbering in the PDF is single spaced but the actual document lines are 1.5 spaced. Therefore, document line numbering does not always correspond exactly with a document line and at some points, document lines fall between line numbers. That is why sometimes two lines are mentioned as the reference lines.</p> <p>Background:</p> <p>Page 4 line 18: there is a single 'g' written ?</p> <p>Page 4 lines 29-30: replace 'legal implication' with 'legal implications'</p> <p>Page 4 line 34: there seems to be an extra space before 'usually', please make sure you have no extra spaces throughout your document.</p> <p>The Background is good and to the point. The aim is clearly set out.</p> <p>Methods:</p> <p>Page 5 line 34: 'in this review' is written twice: remove it once from the text</p> <p>Inclusion and exclusion criteria are clearly set out.</p> <p>Page 5 line 54: patients' satisfaction should be replaced by patient satisfaction as a general term (like cost or adverse events), where 'patient' is used as an adjective to satisfaction.</p>
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Page 5 lines 54 and 55: insert to into: 'according to the Donabedian model of quality improvement, which differentiates between 1) And please note the corrections in line 55: 'which differentiates between 1) . . .' is correct as the subject is singular (the Donabedian model of quality improvement and the verb 'differentiates' has to agree with the subject in its form.)

One general question to the methods used:

The authors exclude small studies with < 300 index cases < 30 near misses for understandable reasons of reducing bias. However, the facility based NMCRs are usually small, as, if they are facility based, which in the introduction / background was put as being more effective, is per definition small. Is the big study from Malawi including 73 facilities, it is more difficult to follow the same facility based approach, which is stipulated to be the inclusive and staff driven method, yielding much of the effects. Therefore, I wonder if in order to review cases of NMCR smaller studies - based only in few facility - are more representative of the actual process and that which is supposed to be effective of it. Actually, these points have been discussed in the discussion to sufficient extent, although, more emphasis would have been interesting and encouraging.

Page 6, line 33: Mantel-Haenszel weighting method!! NOT Haenszel

Results:

Page 7, line 41: insert 'the' after '. . . half of the studies, cases . . .' and comma after studies (as shown)

Page 7, lines 51/52: insert comma after cases: '. . . and two cases, where this . . .'

Page 8 lines 3 and 4: insert comma after cases: 'In three studies, cases . . .'

Page 8, line 8 replace 'were' with 'where' in: '. . . and a study in Moldova, where, despite no predefined criteria, . . .'

Page 8, line 11: Insert The at the beginning of the sentence before Number: 'The number . . .'

Page 8, line 15: Insert comma after experiences: 'Only in four experiences, women . . .'

Page 8, line 32/33: replace 'patients satisfaction' with 'patient satisfaction'

Page 8, lines 42 – 44: insert commas: In a meta-analysis including eight studies, maternal mortality, measured before and after implementation of the NMCR cycle, significantly decreased . . .'

Page 9, line 37: replace 'disseminating' with 'dissemination': '. . . through dissemination of guidelines . . .'

Page 9, line 51/52: replace 'changed' with 'change': '. . . did not significantly change . . .'

Discussion

A good summary of the findings of this systematic review and its main conclusion in first 2 paragraphs!

In general the discussion is much improved and gives a good representation of possible learning points and claims. It includes recommendations and points to take forward, which makes it especially valuable.

Page 10, lines 32 - 35: replace 'criterion-base audit' with 'criterion-based audit'

Page 10, line 41: remove 'a' before new knowledge: '. . . and adds as new knowledge . . .'

Page 10, line 47: remove 'what' before could: '. . . than could be included . . .'

Page 10, line 47: replace 'reviews' with review: '. . . than could be included in this review . . .'

Page 10, line 55: insert after sample size: 'Several studies had a low

	<p>sample size, which . . . ‘</p> <p>Page 11, line 8: remove extra space before ‘at the baseline’ and add) at the end of the citations.</p> <p>Page 11, line 33: replace ‘implementing’ with ‘the implementation of : ‘. . . starting the implementation of . . . ‘</p> <p>Page 11, line 44: replace ‘criterion-base audits’ with criterion-based audits’</p> <p>Page 11, line 53: insert ‘a’ before few months: ‘. . . in a few months . . . ‘</p> <p>Page 11, line 54: replace at with as: ‘. . . as the baseline . . . ‘</p> <p>Page 12, line 17: remove comma in brackets before number 51: (51,52)</p> <p>Page 12, line 30: insert comma after mechanism</p> <p>Page 12, line 30: insert ‘as’ before crucial: ‘. . . are recognized by WHO as crucial . . . ‘</p> <p>Page 12, line 35: insert space after brackets and before hyphen</p> <p>Page 12, line 55: replace ‘patient’s experience’ with ‘patient experience’ – as this is used here without article and as a general term: patient is and adjective to describe experience.</p> <p>Page 12, line 56: insert ‘the’ before UK: the UK – some places need the ‘the’ in front of it, these are listed in the dictionary: the UK, the USA, the Netherlands, the Hague, the Gabon . . .</p> <p>Page 13, lines 12-15: use plural form of focus group: focus groups if they were more than one or use an article ‘a’ before focus group if it was only one. Define who partook in the focus group or focus groups: nurses, midwives, doctors, patients who was interviewed. Insert commas as demonstrated: Qualitative findings, collected through focus groups in a study in Uganda (15), pointed out, among issues that may have hampered the effectiveness of NMCR, health facility factors such as: Conclusions Appropriate and ‘to the point’ conclusions very interesting as they address policy makers as well as researcher.</p> <p>Page 14, line 5: replace ‘patient’s satisfaction’ with ‘patient satisfaction’</p> <p>This is an interesting article with an important message that is very interesting to read and stimulating for clinical practice and the research community alike.</p>
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VERSION 2 – AUTHOR RESPONSE

Author's Response to Decision Letter for (bmjopen-2017-019787.R1)

Effectiveness of the facility based maternal near-miss case reviews in improving maternal and newborn quality of care in low and middle income countries: systematic review

Reviewer: 3

Reviewer Name: Andrew J Scally

Institution and Country: School of Allied Health Professions and Midwifery, Faculty of Health Studies, University of Bradford, UK

Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

I have read the authors' response to my comments and revisited the Mohd Azri paper. I accept that the intervention in this paper focused specifically on eclampsia. On re-reading the manuscript, I can

see that the populations in the included papers vary significantly in their definition, with large variation in the range of conditions included, which explains the wide variation in sample size.

This does, however, lead to a second issue - that of heterogeneity. Statistical tests of heterogeneity are generally of low power and should be used to supplement critical judgement rather than replace it. The wide variation in inclusion criteria in the papers included in this review inevitably mean that heterogeneity IS present. This also seems to be self-evident from the variation in the odds ratios from the different studies.

Having said this, my concern would be the excessive influence of the Kongnyuy (ref 26) paper in a fixed-effect model due to its overwhelming weighting (72.5%), which would be reduced in a random-effects model. However, given that this most influential study in the pooled analysis pulls the effect towards the null, the overall effect (though likely to be slightly biased) is a conservative estimate so I would not insist on the authors re-analysing the data using a random-effects model, though they may wish to consider this in light of my comments as I believe this would improve the robustness of the paper.

A very accessible brief discussion of fixed v. random effects can be found here: https://www.meta-analysis.com/downloads/M-a_f_e_v_r_e_sv.pdf

*** Thank you for your inputs.

We agree with the referee that results are influenced by one large study (Kongnyuy, ref 26), and we have now added this consideration in the discussion section. We appreciate that the referee is not insisting on using a random effect model (REM). According to the Cochrane manual (see section 9.5.4 in the manual), "REM should be applied only when the idea of a 'random' distribution of intervention effects can be justified", and we believe this is not our case (why should effect occur at random?) Also, "REM may exacerbate the effects of publication bias, and reduce the weight of small studies". As suggested by the Cochrane manual (section 9.5.4) we have included sensitivity analysis to further explore the effect of excluding small studies, as a recommended approach. To further explore how sample size impact results, we included a sensitivity analysis pooling only studies with at least 300 cases and 30 events (please see Figure S1 and S2).

Reviewer: 1

Reviewer Name: Dr Bettina Bottcher

Institution and Country: Islamic University of Gaza, Palestine

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Thank you for re-submitting this interesting article after revisions. It s a pleasure to review it once more.

The abstract is very nice: complete and clear.

*** Thank you for your appreciation

Please note that the line numbering in the PDF is single spaced but the actual document lines are 1.5 spaced. Therefore, document line numbering does not always correspond exactly with a document line and at some points, document lines fall between line numbers. That is why sometimes two lines are mentioned as the reference lines.

Background:

Page 4 line 18: there is a single 'g' written ? *** Corrected

Page 4 lines 29-30: replace 'legal implication' with 'legal implications' *** Corrected

Page 4 line 34: there seems to be an extra space before 'usually', please make sure you have no extra spaces throughout your document. *** Corrected

The Background is good and to the point. The aim is clearly set out.

*** Thank you for your appreciation

Methods:

Page 5 line 34: 'in this review' is written twice: remove it once from the text

Inclusion and exclusion criteria are clearly set out. *** Corrected

Page 5 line 54: patients' satisfaction should be replaced by patient satisfaction as a general term (like cost or adverse events), where 'patient' is used as an adjective to satisfaction. *** Corrected

Page 5 lines 54 and 55: insert to into: 'according to the Donabedian model of quality improvement, which differentiates between 1) *** Corrected

And please note the corrections in line 55: 'which differentiates between 1) . . .' is correct as the subject is singular (the Donabedian model of quality improvement and the verb 'differentiates' has to agree with the subject in its form.) *** Corrected

One general question to the methods used:

The authors exclude small studies with < 300 index cases < 30 near misses for understandable reasons of reducing bias. However, the facility based NMCRs are usually small, as, if they are facility based, which in the introduction / background was put as being more effective, is per definition small. In the big study from Malawi including 73 facilities, it is more difficult to follow the same facility based approach, which is stipulated to be the inclusive and staff driven method, yielding much of the effects. Therefore, I wonder if in order to review cases of NMCR smaller studies - based only in few facility - are more representative of the actual process and that which is supposed to be effective of it. Actually, these points have been discussed in the discussion to sufficient extent, although, more emphasis would have been interesting and encouraging.

*** Your point is interesting. We agree with you that the implementation of the NMCR may be easier in smaller facilities. Large studies have the merit to show that the process can also be scaled up in a large number of hospitals (each facility implementing locally the NMCR). Will be interesting to have more details on how much resources are needed (human and economical) to effectively implement the NMCR in a large number of facilities. However the studies did not provide this information, and probably this goes beyond the scope of our review. We have emphasized this point further in the discussion session, and we have further reported the WHO recommendations.

Page 6, line 33: Mantel-Haenszel weighting method!! NOT Haenszwe! *** Corrected

Results:

Page 7, line 41: insert 'the' after '. . . half of the studies, cases . . .' and comma after studies (as shown) *** Corrected

Page 7, lines 51/52: insert comma after cases: '. . . and two cases, where this . . . '*** Corrected

Page 8 lines 3 and 4: insert comma after cases: 'In three studies, cases . . . '*** Corrected

Page 8, line 8 replace 'were' with 'where' in: '. . . and a study in Moldova, where, despite no predefined criteria, . . . '*** Corrected

Page 8, line 11: Insert The at the beginning of the sentence before Number: 'The number . . . '*** Corrected

Page 8, line 15: Insert comma after experiences: 'Only in four experiences, women . . . '*** Corrected

Page 8, line 32/33: replace 'patients satisfaction' with 'patient satisfaction'*** Corrected

Page 8, lines 42 – 44: insert commas: In a meta-analysis including eight studies, maternal mortality, measured before and after implementation of the NMCR cycle, significantly decreased . . . '*** Corrected

Page 9, line 37: replace 'disseminating' with 'dissemination': '. . . through dissemination of guidelines . . . '*** Corrected

Page 9, line 51/52: replace 'changed' with 'change': '. . . did not significantly change . . . '*** Corrected

Discussion

A good summary of the findings of this systematic review and its main conclusion in first 2 paragraphs!In general the discussion is much improved and gives a good representation of possible learning points and claims. It includes recommendations and points to take forward, which makes it especially valuable.

*** Thank you for your appreciation

Page 10, lines 32 - 35: replace 'criterion-base audit' with 'criterion-based audit' *** Corrected

Page 10, line 41: remove 'a' before new knowledge: '. . . and adds as new knowledge . . . '*** Corrected

Page 10, line 47: remove 'what' before could: '. . . than could be included . . . '*** Corrected

Page 10, line 47: replace 'reviews' with review: '. . . than could be included in this review . . . '*** Corrected

Page 10, line 55: insert after sample size: 'Several studies had a low sample size, which . . . '*** Corrected

Page 11, line 8: remove extra space before 'at the baseline' and add) at the end of the citations. *** Corrected

Page 11, line 33: replace 'implementing' with 'the implementation of ': '. . . starting the implementation of . . . '*** Corrected

Page 11, line 44: replace 'criterion-base audits' with criterion-based audits'

Page 11, line 53: insert 'a' before few months: '. . . in a few months . . . ' *** Corrected

Page 11, line 54: replace at with as: '. . . as the baseline . . . '*** Corrected

Page 12, line 17: remove comma in brackets before number 51: (51,52)

Page 12, line 30: insert comma after mechanism*** Corrected

Page 12, line 30: insert 'as' before crucial: '. . . are recognized by WHO as crucial . . . '

Page 12, line 35: insert space after brackets and before hyphen*** Corrected

Page 12, line 55: replace 'patient's experience' with 'patient experience' – as this is used here without article and as a general term: patient is and adjective to describe experience. *** Corrected

Page 12, line 56: insert 'the' before UK: the UK – some places need the 'the' in front of it, these are listed in the dictionary: the UK, the USA, the Netherlands, the Hague, the Gabon . . . *** Corrected

Page 13, lines 12-15:use plural form of focus group: focus groups if they were more than one or use an article 'a' before focus group if it was only one. Define who partook in the focus group or focus groups: nurses, midwives, doctors, patients who was interviewed. *** Corrected

Insert commas as demonstrated:

Qualitative findings, collected through focus groups in a study in Uganda (15), pointed out, among issues that may have hampered the effectiveness of NMCR, health facility factors such as: *** Corrected

Conclusions

Appropriate and 'to the point' conclusions very interesting as they address policy makers as well as researcher.

*** Thank you for your appreciation

Page 14, line 5: replace 'patient's satisfaction' with 'patient satisfaction'*** Corrected

This is an interesting article with an important message that is very interesting to read and stimulating for clinical practice and the research community alike.

*** Thank you for your appreciation

VERSION 3 – REVIEW

REVIEWER	Andrew J Scally School of Allied Health Professions and Midwifery, Faculty of Health Studies, University of Bradford, UK
REVIEW RETURNED	02-Feb-2018

GENERAL COMMENTS

The researchers have engaged with my comment, which was a recommendation rather than a condition. I do not think they are quite correct in their interpretation of a random-effects model, but I am content to let go of this issue as it does not impact substantially on the conclusions of the review.