PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Differences in rates and odds for emergency caesarean section in six Palestinian hospitals: a population-based birth cohort study.
AUTHORS	Zimmo, Mohammed; Laine, Katariina; Hassan, Sahar; Fosse, Erik; Lieng, Marit; Ali-Masri, Hadil; zimmo, kaled; Anti, Marit; Bottcher, Bettina.; Sørum Falk, Ragnhild; Vikanes, Åse

VERSION 1 – REVIEW

REVIEWER	Dittakarn Boriboonhirunsarn Department of Obstetrics and Gynecology Faculty of Medicine Siriraj Hospital Mahidol University Thailand
REVIEW RETURNED	11-Oct-2017

GENERAL COMMENTS	1. Ethical approval of the study should be described. 2. Are there any guidelines for standard of care? Were all hospitals following the same guideline? 3. What is the policy for VBAC? Was VBAC offered to all women with prior cesarean? Was it the same in every hospital? 4. What are common indications for cesarean section in this study? 5. Results should be in order with Table. 6. Some variables are not continuous variables and the use of mean and SD is not appropriate: number of children, number of antenatal visit. 7. Please check the reference category of "hypertensive disorder" and "induction of lablour" in multivariate analysis.

REVIEWER	Khalid B M Saeed Department of Obstetrics and Gynaecology, University College Cork, Ireland
REVIEW RETURNED	25-Oct-2017

GENERAL COMMENTS	1) Page 2 line 10: authors state they would like to assess differences in rates and risks for emergency caesarean section in…., but in page 5 line 34 authors stated that they would exclude "multiple gestations", so I think the authors might need to state that in their objectives in page 2 line 10 that the study focus on singleton pregnancy. Authors are either interested in caesarean section rate and risk factors or in rates and risk factors for singleton pregnancy, I think if this is clearer from the outset, it would benefit the study and the reader. Could the title reflect the same point?

	2) Page 2 line 10: authors state it is a prospective study, and in page 5 line 36 authors exclude (those with missing information about mode of delivery) my query is did the authors meant that they would exclude patients with missing information about planned or the actual mode of delivery? 3) Page 2 line 34: The authors state in their methods that they would be excluding the multiple gestations so I understand that the main outcome would be "the rate and risk for emergency caesarean section in singleton pregnancy" authors might need to re-consider this point throughout the manuscript. So if women with multiple pregnancy are excluded then by default, the participants are women with singleton pregnancy, and that has to be stated clearly in the manuscript from the outset in the abstract in page 2 (objectives line 10,participants 18 ,main outcome measures line 34). 4) Page 4 line 16-19: Authors stated that the worrying rise in caesarean section rate was seen in healthy primiparous who were having a low risk caesarean section and cited reference #5. In reference #5 there was a rise in caesarean section rate from 8.0% to just over 20% in first time mothers at term with singletons in vertex presentation and does occur when 'labour was induced'. could authors highlight the factor of 'induction' as it could be the reason behind intervention. 5) Page 4 line 45: Authors wrote (caesarean section rate increased from 4.4 % 12.6% between 1993 and 2002 (authors cited reference 10). Reference 10 reads and I quote "The instrumental delivery rate decreased from 12.6% (423 deliveries) to 4.4% (153 de-liveries)"end quote. Moreover, the same quoted figures (4.4% and 12.6 %) was repeated in the Arabic and the English and the French abstracts in the aforementioned reference stating that instrumental deliveries decreased bewen 1993 and 2002 from 12.6% to 4.4%!!!!!Authors might need to consider reviewing reference 10 again. I believe reference 10 was studying the increasing rate of caesarean section in the 10-year period from 9.4% to 14.4%. 6) Page 4 line 50: Authors cited reference #9 when mention the increase in caesarean section between 1996 and 2006. Authors need to check the figures in original reference, minor difference. 7) Page 7 line 3: Authors state that the primary outcome was the rate and risks of emergency caesarean section. I would like to see a note clarifying the status of the pregnancy (singleton Vs multiple), this comment apply throughout the manuscript. 8) Page 17 line 12 : was there any attempt to compute the No of Consultants , residents and its relationship with caesarean section risk in this study as in the cited reference #13 there was a notable difference the "No OB/GYN consultants" in table 1 in the cited reference. Was that computed in this study and ? level of significance. 9) In study limitation: If authors could highlight that multiple pregnancy was exclude and the population in the study was singleton pregnancy.

REVIEWER	Ilir Hoxha Solidar Suisse Kosovo, Kosovo

REVIEW RETURNED	04-Dec-2017

| GENERAL COMMENTS | This is an interesting study and has value in addressing rate differences among hospitals in Palestine.

1. Why do authors think this is a prospective population-based birth cohort study? It seems like cross sectional design to me.

2. The paper states that "The primary outcome was the rate and risk of emergency caesarean section according to hospitals where the women delivered." I think this is not clear. You may want to say something like: the primary outcome of the study are the individual adjusted odds ratios of caesarean section for five Palestinian hospitals as compared to reference (name it) hospital. Secondary outcome are the individual crude odds ratios of caesarean section for five hospitals as compared to reference hospital (name it).

3. The paper mentions the Parous women. Are you referring to multiparous women? As I see you use primiparas. Then suggest you use that term.

4. The paper states "The main aim of this study was to explore any differences in rates and risks for emergency caesarean section in six governmental hospitals in Palestine." Using risk is again inappropriate. You may want to say only rates…

5. Missing data. Did you consider running multiple imputation? If not, why?

6. Why particular hospital was selected as reference in logistic regression?! Any particular reason? You mention some of characteristics but you may want to elaborate why?

7. A table visualising what are the characteristics of individual hospitals under study may provide to be useful. You can use several categories depending on what information you have such as supply with resources (equipment, specialized workforce), practice patterns, ownership of hospital, type of hospital, funding, geographic location, or whatever information you have. You may use such categorization also for developing additional variables. For example, teaching/non-teaching hospital), charity/non charity, Gaza/West bank. This will also help you understand and interpret better the results in discussion section.

8. Multicollinearity. You need to explore data further. Did you run any tests to explore multicollinearity? You may want to run regression on separate parts of data set (by hospital for example) to see what is happening. I would also appreciate that you report the results of OR in case you leave in that variable. Or you take it out completely which can be one of the options how to deal with this problem.

9. The paper reports "No/Unknown" category for some variables. Is there a way to make 3 categories? Yes, No, Unknown? That will give a clearer view of effect of "No" or "Yes" of specific variable.

10. Using variable "Antenatal visits (no)" as continuous variables is not as valuable as when you categorise. It's harder to interpret and difficult for reader to understand. You may want to dichotomise or categorise. You already use them in descriptive table 2. I would add 3rd and 4th category into one (8 or more antenatal care visits). |

	11. Variable "Children alive (no)". It makes no sense to use it as it records similar information like parity. So would take it out from analysis in both subgroups (primiparas and multiparous). 12. Table 1, 2, 3, reporting variable characteristics may be merged into one table. You may also report descriptive statistics together with crude ORs. In that case you would not present data by hospital. 13. You may want to explain the reasons for developing particular models. 14. Why variable urban/rural is together in regression analysis? It would make sense to have it as separate categories. 15. Paper in general needs a bit of polishing and editing to become more consistent. Discussion section needs to be organized and developed further. See references I provide bellow. 16. Some references to consider which may help you in organizing, understanding and interpreting you results. Burns LR, Geller SE, Wholey DR. The effect of physician factors on the cesarean section decision. Medical care. 1995;33(4):365-82. Huesch MD, Currid-Halkett E, Doctor JN. Measurement and risk adjustment of prelabor cesarean rates in a large sample of California hospitals. Am J Obstet Gynecol. 2014;210(5):443 e1-17. Carayol M, Zein A, Ghosn N, Du Mazaubrun C, Breart G. Determinants of caesarean section in Lebanon: Geographical differences. Paediatric and Perinatal Epidemiology. 2008;22(2):136-44. Kabakian-Khasholian T, Kaddour A, Dejong J, Shayboub R, Nassar A. The policy environment encouraging C-section in Lebanon. Health Policy. 2007;83(1):37-49.

VERSION 1 – AUTHOR RESPONSE

Gaza December 21st, 2017

Response to reviewers of manuscript ID bmjopen-2017-019509
"Differences in rates and risks for emergency caesarean section in six Palestinian hospitals: a population based birth cohort study"

Dear Editor and reviewers,
Thank you for your interest in our manuscript and for your valuable comments. We have carefully considered the suggested changes and have modified the manuscript accordingly. Our responses to the referees' comments are provided below.
We hope the revised version of our manuscript is to your satisfaction.
Sincerely,
Dr. Mohammed Zimmo

Comments from reviewer 1:

Reviewer Name: Dittakarn Boriboonhirunsarn
Institution and Country: Department of Obstetrics and Gynecology, Faculty of Medicine Siriraj Hospital, Mahidol University, Thailand

Competing Interests: None declared

1. Ethical approval of the study should be described.

Our reply:
We agree with the reviewer that ethical approval should be described more thoroughly, and this is now described in detail on page 20.

2. Are there any guidelines for standard of care? Were all hospitals following the same guideline?

Our reply:
We thank the reviewer for this comment. Yes, there are guidelines for standard care in Palestine. The Palestinian national guidelines for standardized labor management were recently updated in 2016, which may contribute to harmonize clinical practice. We now mention that more clearly; in the discussion part, page 17, reference # 23.

3. What is the policy for VBAC? Was VBAC offered to all women with prior cesarean? Was it the same in every hospital?

Our reply:
We are grateful for this important comment. As mentioned previously there are guidelines for standard obstetric care in Palestine. According to these guidelines (chapter: management of uterine scar) VBAC should be offered to all women with one previous section if there is no contraindication. However, our findings do imply that adherence to these guidelines varies between the study hospitals. We therefore added a sentence to clarify this point on page18, highlighted in yellow.

4. What are common indications for cesarean section in this study?

Our reply:
This is a very important point regarding the risk for emergency caesarian section. Since in this publication we are solely addressing whether antenatal characteristics, sociodemographic and pregnancy related characteristics, could explain the differences in risk for emergency caesarian section between the six study hospitals, we did not include indications. The next paper based on the same data is intended to explore in more detail whether differences in indication, reflecting intrapartum factors for decision making, could explain differences in risk for emergency caesarian section.

5. Results should be in order with Table.
Our reply:
We agree with the reviewer on this point. Therefore, we removed the following sentence 'Emergency caesarean section prevalence varied significantly between the hospitals, ranging from 5.0% to 15.7% (table 3)' about emergency caesarean section prevalence from page 8 (first paragraph in result part), since we mention these details further down in the script page 11.

6. Some variables are not continuous variables and the use of mean and SD is not appropriate: number of children, number of antenatal visit.
Our reply:
Thank you for notifying this. Number of children and number of antenatal visits are discrete numeric variables. The distribution of 'number of children' is positively skewed, but 'number of antenatal visits' seems to be normally distributed. Thus, in table 2, the measure of central tendency and variation of 'number of children' are changed to median and interquartile range. Further, to test for differences

between hospitals Kruskal-Wallis test are performed. The method section and the footnote are corrected accordingly, please see abstract on page 2, methods on page 7and table 2 on page 10.

7. Please check the reference category of "hypertensive disorder" and "induction of labour" in multivariate analysis.

Our reply:
Thanks for this comment. This was a writing error and the tables have been corrected accordingly; please see tables 4 and 5 on the pages 12, 13 and 14.

Comments from reviewer 2:
Reviewer Name: Khalid B M Saeed
Institution and Country: Department of Obstetrics and Gynaecology, University College Cork, Ireland
Competing Interests: None declared

1) Page 2 line 10: authors state they would like to assess differences in rates and risks for emergency caesarean section in...., but in page 5 line 34 authors stated that they would exclude "multiple gestations", so I think the authors might need to state that in their objectives in page 2 line 10 that the study focus on singleton pregnancy. Authors are either interested in caesarean section rate and risk factors or in rates and risk factors for singleton pregnancy, I think if this is clearer from the outset, it would benefit the study and the reader.
Could the title reflect the same point?

Our reply:
We totally agree with reviewer and clarified this in the manuscript, please see highlighted in yellow in abstract on page 2 and introduction on page 1, as well as introduction on page 5, methods on page 7, results on page 8 and discussion on page 15.

2) Page 2 line 10: authors state it is a prospective study, and in page 5 line 36 authors exclude (those with missing information about mode of delivery) my query is did the authors meant that they would exclude patients with missing information about planned or the actual mode of delivery?

Our reply:
We are sorry for this confusion and we added some sentences on page 5 (highlighted). All deliveries with missing information about mode of delivery were excluded. Also those with planned caesarean section were excluded, since these women were not scheduled for vaginal births. Some women have contractions and go into labour before the date for their planned caesarean section, and since these women all end up with a caesarean section we excluded them from our data set. However 1.6% of the total sample had no record for mode of delivery in the data collection sheet and were excluded from the study.

3) Page 2 line 34: The authors state in their methods that they would be excluding the multiple gestations so I understand that the main outcome would be "the rate and risk for emergency caesarean section in singleton pregnancy" authors might need to re-consider this point throughout the manuscript. So if women with multiple pregnancy are excluded then by default, the participants are women with singleton pregnancy, and that has to be stated clearly in the manuscript from the outset in the abstract in page 2 (objectives line 10,participants 18 ,main outcome measures line 34).
Our reply:
We agree with the reviewer as mentioned above, and have adapted our manuscript accordingly, in abstract and limitation of the study page 2 and introduction on page1, as well as introduction on page 5, methods on page 7, results on page 8 and discussion on page 15.

4) Page 4 line 16-19: Authors stated that the worrying rise in caesarean section rate was seen in healthy primiparous who were having a low risk caesarean section and cited reference #5.
In reference #5 there was a rise in caesarean section rate from 8.0% to just over 20% in first time mothers at term with singletons in vertex presentation and does occur when 'labour was induced'. could authors highlight the factor of 'induction' as it could be the reason behind intervention.

Our reply:
Thanks very much and we agree with the reviewer as mentioned above, and have adapted this point on page 4

5) Page 4 line 45: Authors wrote (caesarean section rate increased from 4.4 % 12.6% between 1993 and 2002 (authors cited reference 10).
Reference 10 reads and I quote "The instrumental delivery rate decreased from 12.6% (423 deliveries) to 4.4% (153 de- liveries)"end quote. Moreover, the same quoted figures (4.4% and 12.6 %) was repeated in the Arabic and the English and the French abstracts in the aforementioned reference stating that instrumental deliveries decreased bewen 1993 and 2002 from 12.6% to 4.4%!!!!!Authors might need to consider reviewing reference 10 again.
I believe reference 10 was studying the increasing rate of caesarean section in the 10-year period from 9.4% to 14.4%.
Our reply:
Thanks to the reviewer for making us aware of this mistake and we corrected the numbers on page 4.

6) Page 4 line 50: Authors cited reference #9 when mention the increase in caesarean section between 1996 and 2006. Authors need to check the figures in original reference, minor difference.
Our reply:
Thanks very much for the reviewer for this clarification and we corrected the number (14.8%) on page 4.

7) Page 7 line 3: Authors state that the primary outcome was the rate and risks of emergency caesarean section. I would like to see a note clarifying the status of the pregnancy (singleton Vs multiple), this comment apply throughout the manuscript.

Our reply:
Thanks for this comment and we mentioned this in the abstract limitations of the study on page 2 and introduction on page 1, as well as introduction on page 5, methods on page 7, results on page 8 and discussion on page 15.

8) Page 17 line 12 : was there any attempt to compute the No of Consultants , residents and its relationship with caesarean section risk in this study as in the cited reference #13 there was a notable difference the "No OB/GYN consultants" in table 1 in the cited reference. Was that computed in this study and ? level of significance.

Our reply:
We considered this important aspect of risk for caesarean sections. Our plan was to register data on who was attending the delivery as well as who was the final decision maker for caesarian section. Unfortunately this data did not seem reliable when checking. However, due to common practice in Palestine and elsewhere consultants are responsible for the decisions regarding emergency caesarean section. Furthermore, this paper focused mainly on patient related factors, which were examined and discussed. Therefore, in this paper, no other analysis of staff working patterns has been made.

9) In study limitation: If authors could highlight that multiple pregnancy was exclude and the population in the study was singleton pregnancy.

Our reply:
We agree with the reviewer and clarified in line with the reviewer's comments that the main focus of this study was singletons, please see page 19
Comments from reviewer 3:
Reviewer Name: Ilir Hoxha
Institution and Country: Solidar Suisse Kosovo, Kosovo
Competing Interests: None declared

This is an interesting study and has value in addressing rate differences among hospitals in Palestine.

1. Why do authors think this is a prospective population-based birth cohort study? It seems like cross sectional design to me.

Our reply:
We do understand the reviewer, that there may be some confusion regarding the study design. However, given the fact that the data was collected in a prospective manner, that the women were included prospectively during a one year period from March 2015 to March 2016, and that the data for each woman was collected by case registration forms, we consider this study to be a prospective population-based birth cohort study. This is the largest birth cohort study in Palestine so far.

2. The paper states that "The primary outcome was the rate and risk of emergency caesarean section according to hospitals where the women delivered." I think this is not clear. You may want to say something like: the primary outcome of the study are the individual adjusted odds ratios of caesarean section for five Palestinian hospitals as compared to reference (name it) hospital. Secondary outcome are the individual crude odds ratios of caesarean section for five hospitals as compared to reference hospital (name it).

Our reply:
Thank you for this suggestion. We changed the primary outcome accordingly in abstract page 2 and in methods page 7. Since this is a cohort study we can calculate rates of emergency caesarean section, as presented in table 3 and as crude OR in table 4, so we do not use secondary outcomes. The risks of emergency caesarean section are presented as ORs from logistic regression analysis (tables 4 and 5). The odds ratio is very similar to the risk ratio, particularly if a disease is rare. The general rule though is that if the prevalence of the disease is <10% or so, the relative risk and the odds ratio will be approximately the same. The rarer the disease the closer the approximation. In this study, the rate of emergency caesarean section was 9% (2932/32321).

3. The paper mentions the parous women. Are you referring to multiparous women? As I see you use primiparas. Then suggest you use that term.

Our reply:
We do understand the reviewer's concern. But we expect that the reader is familiar with the distinction between primiparous and parous, as these terms are frequently used in studies. Moreover, a clear definition is now given for both terms, primiparous and parous women in methodology on page 6. Multiparous may imply more than one previous delivery whereas parous is a more inclusive term.*

4. The paper states "The main aim of this study was to explore any differences in rates and risks for emergency caesarean section in six governmental hospitals in Palestine." Using risk is again inappropriate. You may want to say only rates…

Our reply:

Thank you for this clarification and we understand that it is important to recognize the difference between risks and odds ratio (odds ratio is a measure of a relative risk), however for this paper odds ratio was calculated in the attempt to estimate the strength of an association between exposure (sociodemographic and obstetric characteristics) and the outcome (emergency caesarean section). We have changed the text from 'risk' to 'odds ratios' where we refer to numbers, but otherwise we prefer to use the term 'risk'. Please see more details above in our reply to comment number 2.

5. Missing data. Did you consider running multiple imputation? If not, why?

Our reply:

The number of missing data and the differences between the hospitals is a result itself, thus presented as separate rows in tab 1 + 2. Further, as the fraction of missing is low; BMI (5%) and maternal age (2.5 %), even lower (i.e. <2%) for the remaining variables, multiple imputation was not considered.

6. Why particular hospital was selected as reference in logistic regression?! Any particular reason? You mention some of characteristics but you may want to elaborate why?

Our reply:

We chose the hospital with the lowest prevalence in emergency caesarean section (5.0%) as reference hospital.

7. A table visualising what are the characteristics of individual hospitals under study may provide to be useful. You can use several categories depending on what information you have such as supply with resources (equipment, specialized workforce), practice patterns, ownership of hospital, type of hospital, funding, geographic location, or whatever information you have. You may use such categorization also for developing additional variables. For example, teaching/non-teaching hospital), charity/non charity, Gaza/West bank. This will also help you understand and interpret better the results in discussion section.

Our reply:

We do agree with the reviewer that such a table could be of interest. Since all hospitals are governmental they have the same access to very similar or identical equipment and funding. Unfortunately this study did not collect data on the exact equipment available in the study hospitals or their specialized workforce or practice patterns. This point is added as a limitation, which is done on page 19. The reviewer's concern is important, as the findings of this study may reflect resources, including equipment, specialized workforce and practice patterns as discussed in the paper. In view of the fact, that these are all governmental hospitals, it is assumed by the authors that differences are not too great. Also, the focus of this study was the sociodemographic and obstetric characteristics rather than location of delivery.

8. Multicollinearity. You need to explore data further. Did you run any tests to explore multicollinearity? You may want to run regression on separate parts of data set (by hospital for example) to see what is happening. I would also appreciate that you report the results of OR in case you leave in that variable. Or you take it out completely which can be one of the options how to deal with this problem.

Our reply:

Thanks for this comment. Multicollinearity refers to a situation in which two or more explanatory variables in a model are highly linearly related (https://en.wikipedia.org/wiki/Multiple_regression" \o

"Multiple regression). We have assessed multicollinearity by calculation of the variance inflation factor. This is now stated in the method section page 7. To split the data into separate parts is not relevant in this setting.

9. The paper reports "No/Unknown" category for some variables. Is there a way to make 3 categories? Yes, No, Unknown? That will give a clearer view of effect of "No" or "Yes" of specific variable.

Our reply:
The DHIS2 software used for the data collection did only recognize two categories for hypertension disorder and induction of labor with one being yes and the remaining data being collected in one category which includes women with no as well as missing data. Therefore, it is not possible for us to analyze these two variables in three categories.

10. Using variable "Antenatal visits (no)" as continuous variables is not as valuable as when you categorise. It's harder to interpret and difficult for reader to understand. You may want to dichotomise or categorise. You already use them in descriptive table 2. I would add 3rd and 4th category into one (8 or more antenatal care visits).

Our reply:
Thank you for pointing this out. The general statistical advice is to keep continuous exposure variables as continuous in statistical analysis. I.e. due to several methodological drawbacks, categorization should be avoided.* We have checked if the linearity assumption is met, as it is. In table 4, the crude OR for antenatal visits as continuous variables is 1.07. If categorized, the ORs are 1.08, 1.58, and 2.08 for 4-7 visits, 8-12 visits, and 13-20 visits, respectively, compared to 1-3 visits. We have kept 'antenatal visits' as continuous in the analyses (table 4 and 5). In table 2 we present we merge the last two categories together and we correct the categorization in methods part, page 6
*References:
•   Altman DG, Royston P: Statistics notes - The cost of dichotomising continuous variables. British Medical Journal. 2006.
•   van Walraven C, Hart RG: Leave 'em alone - Why continuous variables should be analyzed as such. Neuroepidemiology. 2008

11. Variable "Children alive (no)". It makes no sense to use it as it records similar information like parity. So would take it out from analysis in both subgroups (primiparas and multiparous).
Our reply:
In general this is absolutely true. However in the reality of Palestine, the mortality rates in infant, children and young people are not negligible. Therefore, some women have higher parity compared to number of living children. Culturally among the local population, this is an important factor in family planning and obstetric care decisions.
'Children alive' is not included in the analysis of primiparous women. In the analysis of parous women, we checked the correlation between the independent factors by multicollinearity analysis as described above. No multicollinearity was observed.

12. Table 1, 2, 3, reporting variable characteristics may be merged into one table. You may also report descriptive statistics together with crude ORs. In that case you would not present data by hospital.
Our reply:
As this paper focused on comparing differences in rates and risks for emergency caesarean section in relation to sociodemographic and obstetric characteristics, it seeks to examine if such differences could be explained by variations in these patient characteristics. Therefore, the data are presented

according to hospitals in order to study the differences. Presenting the data as one number without reference to the different hospitals, would make this difficult.

13. You may want to explain the reasons for developing particular models.
Our reply:
Logistic regression analyses are suited to study the effect of a set of risk factors on a dichotomous outcome (emergency caesarean section). This is described in page 7; "In order to study the effect of the hospital on the risk of emergency caesarean section, logistic regression analyses were applied".

14. Why variable urban/rural is together in regression analysis? It would make sense to have it as separate categories.

Our reply:
Unfortunately, a degree of confusion existed among the data collection teams as to what was defined as 'rural' and what as 'urban'. Therefore it was impossible to distinguish between the two in a reliable way. This confusion could be explained by the different geographical and political situations in the West Bank and Gaza. However the definition of refugee camp was clear. Therefore, residency was either defined as camp or rural/urban.

15. Paper in general needs a bit of polishing and editing to become more consistent. Discussion section needs to be organized and developed further. See references I provide bellow.
Our reply:
Thank you for this advice and helpful references. We have looked at them with care, developed the discussion and improved the structure, as well as worked over the full paper.

16. Some references to consider which may help you in organizing, understanding and interpreting you results.
Burns LR, Geller SE, Wholey DR. The effect of physician factors on the cesarean section decision. Medical care. 1995;33(4):365-82.
Huesch MD, Currid-Halkett E, Doctor JN. Measurement and risk adjustment of prelabor cesarean rates in a large sample of California hospitals. Am J Obstet Gynecol. 2014;210(5):443 e1-17.
Carayol M, Zein A, Ghosn N, Du Mazaubrun C, Breart G. Determinants of caesarean section in Lebanon: Geographical differences. Paediatric and Perinatal Epidemiology. 2008;22(2):136-44.
Kabakian-Khasholian T, Kaddour A, Dejong J, Shayboub R, Nassar A. The policy environment encouraging C-section in Lebanon. Health Policy. 2007;83(1):37-49.
Our reply:
Thank you for this advice and helpful references.

VERSION 2 – REVIEW

REVIEWER	Iliir Hoxha
	Solidar Suisse Kosovo, Kosovo
REVIEW RETURNED	12-Jan-2018

GENERAL COMMENTS	The authors have addressed most of points from first review.
	Bellow are some additional points. I keep original comments and replies by authors in order to make it easier to follow. Otherwise my additional comments are in last paragraph of each point.
	2. The paper states that "The primary outcome was the rate and risk of emergency caesarean section according to hospitals where the

women delivered." I think this is not clear. You may want to say something like: the primary outcome of the study are the individual adjusted odds ratios of caesarean section for five Palestinian hospitals as compared to reference (name it) hospital. Secondary outcome are the individual crude odds ratios of caesarean section for five hospitals as compared to reference hospital (name it).

Our reply:
The risks of emergency caesarean section are presented as ORs from logistic regression analysis (tables 4 and 5). The odds ratio is very similar to the risk ratio, particularly if a disease is rare. The general rule though is that if the prevalence of the disease is <10% or so, the relative risk and the odds ratio will be approximately the same. The rarer the disease the closer the approximation. In this study, the rate of emergency caesarean section was 9% (2932/32321).

IH:
With regard to point you raise (on similarity of OR and RR) I don't see a need for moving discussion in that direction! For the matter of conceptual/language consistency in the manuscript I suggest you use odds ratios...or odds and not introduce new terms (risk) which may confuse the reader.

4. The paper states "The main aim of this study was to explore any differences in rates and risks for emergency caesarean section in six governmental hospitals in Palestine." Using risk is again inappropriate. You may want to say only rates…

Our reply:
Thank you for this clarification and we understand that it is important to recognize the difference between risks and odds ratio (odds ratio is a measure of a relative risk), however for this paper odds ratio was calculated in the attempt to estimate the strength of an association between exposure (sociodemographic and obstetric characteristics) and the outcome (emergency caesarean section). We have changed the text from 'risk' to 'odds ratios' where we refer to numbers, but otherwise we prefer to use the term 'risk'. Please see more details above in our reply to comment number 2.

IH:
Suggest you are consistent throughout the manuscript (see 2).

5. Missing data. Did you consider running multiple imputation? If not, why?

Our reply:
The number of missing data and the differences between the hospitals is a result itself, thus presented as separate rows in tab 1 + 2. Further, as the fraction of missing is low; BMI (5%) and maternal age (2.5 %), even lower (i.e. <2%) for the remaining variables, multiple imputation was not considered.

IH:
You may explain/add this in the manuscript in the methods section.

13. You may want to explain the reasons for developing particular models.

Our reply:
Logistic regression analyses are suited to study the effect of a set of risk factors on a dichotomous outcome (emergency caesarean section). This is described in page 7; "In order to study the effect of the hospital on the risk of emergency caesarean section, logistic regression analyses were applied".

IH:
My comment relates to data presented in table 4 and 5. There you

	present results from 3 different models (Model 1, 2, 3). It would be worth to mention in methods section the reasons for such modelling. 14. Why variable urban/rural is together in regression analysis? It would make sense to have it as separate categories. Our reply: Unfortunately, a degree of confusion existed among the data collection teams as to what was defined as 'rural' and what as 'urban'. Therefore it was impossible to distinguish between the two in a reliable way. This confusion could be explained by the different geographical and political situations in the West Bank and Gaza. However the definition of refugee camp was clear. Therefore, residency was either defined as camp or rural/urban. IH: You may explain this in methods and/or study limitations.

REVIEWER	Khalid Saeed Department of Obstetrics and Gynaecology, University College Cork, Cork, Ireland
REVIEW RETURNED	15-Jan-2018

GENERAL COMMENTS	Authors reply "Comments to reviewer 2" checked and satisfactory. No statistical review attempted.

<center>

VERSION 2 – AUTHOR RESPONSE

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Gaza February 04th, 2018

Response to reviewers of manuscript ID bmjopen-2017-019509
"Differences in rates and risks for emergency caesarean section in six Palestinian hospitals: a population based birth cohort study"

Dear Editor and reviewers,
Thank you for your interest in our manuscript and for your valuable comments again. We have carefully considered the suggested changes and have modified the manuscript accordingly. Our responses to the referees' comments are provided below.
We hope the revised version of our manuscript is to your satisfaction.
Sincerely,
Dr. Mohammed Zimmo

Comments from reviewer 2:
Reviewer Name: Khalid B M Saeed
Institution and Country: Department of Obstetrics and Gynaecology, University College Cork, Ireland
Competing Interests: None declared

Authors reply "Comments to reviewer 2" checked and satisfactory.
No statistical review attempted.

Our reply:
Thanks very much for this feedback.

Comments from reviewer 3:
Reviewer Name: Ilir Hoxha
Institution and Country: Solidar Suisse Kosovo, Kosovo
Competing Interests: None declared

Bellow are some additional points. I keep original comments and replies by authors in order to make it easier to follow. Otherwise my additional comments are in last paragraph of each point.

2. The paper states that "The primary outcome was the rate and risk of emergency caesarean section according to hospitals where the women delivered." I think this is not clear. You may want to say something like: the primary outcome of the study are the individual adjusted odds ratios of caesarean section for five Palestinian hospitals as compared to reference (name it) hospital. Secondary outcome are the individual crude odds ratios of caesarean section for five hospitals as compared to reference hospital (name it).

Our reply:
Thank you for this suggestion. We changed the primary outcome accordingly in abstract page 2 and in methods page 7. Since this is a cohort study we can calculate rates of emergency caesarean section, as presented in table 3 and as crude OR in table 4, so we do not use secondary outcomes. The risks of emergency caesarean section are presented as ORs from logistic regression analysis (tables 4 and 5). The odds ratio is very similar to the risk ratio, particularly if a disease is rare. The general rule though is that if the prevalence of the disease is <10% or so, the relative risk and the odds ratio will be approximately the same. The rarer the disease the closer the approximation. In this study, the rate of emergency caesarean section was 9% (2932/32321).
IH:
With regard to point you raise (on similarity of OR and RR) I don't see a need for moving discussion in that direction! For the matter of conceptual/language consistency in the manuscript I suggest you use odds ratios...or odds and not introduce new terms (risk) which may confuse the reader.
Our second reply:
Thank you very much for your clarification. Now we use only one term (odds or odds ratio). We changed the term (risk) accordingly in the title page 1, abstract pages 2 and 3, Introduction page 5, methods page 7, and in the discussion pages 15-19

4. The paper states "The main aim of this study was to explore any differences in rates and risks for emergency caesarean section in six governmental hospitals in Palestine." Using risk is again inappropriate. You may want to say only rates…

Our reply:
Thank you for this clarification and we understand that it is important to recognize the difference between risks and odds ratio (odds ratio is a measure of a relative risk), however for this paper odds ratio was calculated in the attempt to estimate the strength of an association between exposure (sociodemographic and obstetric characteristics) and the outcome (emergency caesarean section). We have changed the text from 'risk' to 'odds ratios' where we refer to numbers, but otherwise we prefer to use the term 'risk'. Please see more details above in our reply to comment number 2.
IH:
Suggest you are consistent throughout the manuscript (see 2).
Our second reply:

We agree completely with this suggestion and some corrections accordingly described in the previous reply.

5. Missing data. Did you consider running multiple imputation? If not, why?

Our reply:
The number of missing data and the differences between the hospitals is a result itself, thus presented as separate rows in tab 1 + 2. Further, as the fraction of missing is low; BMI (5%) and maternal age (2.5 %), even lower (i.e. <2%) for the remaining variables, multiple imputation was not considered.
IH:
You may explain/add this in the manuscript in the methods section.
Our second reply:
Thank you for pointing this out. We added a sentence to clarify this in the methods page 7.

13. You may want to explain the reasons for developing particular models.
Our reply:
Logistic regression analyses are suited to study the effect of a set of risk factors on a dichotomous outcome (emergency caesarean section). This is described in page 7; "In order to study the effect of the hospital on the risk of emergency caesarean section, logistic regression analyses were applied".
IH:
My comment relates to data presented in table 4 and 5. There you present results from 3 different models (Model 1, 2, 3). It would be worth to mention in methods section the reasons for such modelling.
Our second reply:
Unfortunately, we understand your important point. We would like to test the influences of the sociodemographic and obstetric characteristics on the odds for emergency caesarean section and we added a (highlighted) sentence in the methods to explain this reason.

14. Why variable urban/rural is together in regression analysis? It would make sense to have it as separate categories.

Our reply:
Unfortunately, a degree of confusion existed among the data collection teams as to what was defined as 'rural' and what as 'urban'. Therefore it was impossible to distinguish between the two in a reliable way. This confusion could be explained by the different geographical and political situations in the West Bank and Gaza. However the definition of refugee camp was clear. Therefore, residency was either defined as camp or rural/urban.
IH:
You may explain this in methods and/or study limitations.

Our second reply:
Yes this is absolutely true. Therefore, we added this point in the limitations of the study in page 19