PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Satisfaction with maternity care among recent migrants: an interview questionnaire-based study
AUTHORS	Bains, Sukhjeet; Sundby, Johanne; Lindskog, Benedikte; Vangen, Siri; Diep, Lien; Owe, Katrine Mari; Sorbye, Ingvil

VERSION 1 – REVIEW

REVIEWER	Schmied, V Western Sydney University, School of Nursing and Midwifery
REVIEW RETURNED	07-Feb-2021

GENERAL COMMENTS	This study is important because of the efforts made to ensure that migrant women have an avenue to report their satisfaction – the qualitative or open ended responses would also be very interesting and important to report if there is an opportunity. Background to the study is appropriate and provides enough information based on current literature to identify the need for the study The rationale for the project is as follows “The Mipreg-project is a multidisciplinary, mixed method project that seeks to identify factors that explain disparities in pregnancy outcomes among recently migrated women giving birth in urban Oslo, Norway”… However, no data or literature is used to demonstrate the situation om Norway. Methods This sentence needs correcting “The midwives informed about the study in the women´s language of choice and a written consent was obtained.”.. think should be midwives informed women about the study … Please explain a little more on how user representatives were involved in the design and conduct of the study. The following statement is made in relation to the sample calculation “Since the proportion of satisfaction was unknown before study start, maximum number of women required was estimated to 385 assuming a width of 10% for the estimated proportion with a 95% confidence interval (CI). This needs to be explained more clearly for readers. Could the sample have been calculated on previous samples that used the same survey instrument, even though it was in a different population? Overall, the results are well presented and described. In the demographic table the regions that immigrants came from are listed – what were the high income locations? – the authors indicated they were most interested in women from low and middle income but a reasonable proportion were from high income countries. Discussion

	The discussion is clear and all outcomes have been discussed In the discussion the following statement is made.."Care during pregnancy was the time-period with highest proportion of dissatisfaction in our study. Contrary, a Dutch study showed that non-western migrants were most satisfied with the antenatal care 36, while a British study found little difference in satisfaction between the three periods 32 " First I noted this should probably say .."Contrary to this, …" Also these differences might be explained by the different ways maternity care is organised and delivered between countries as noted but not just noninterventionist care but also continuity of care. In the Netherlands for example it may also be more likely that the woman sees the same care provider, particularly a midwife during antenatal care but in UK she is more likely to see different care providers. No comment is made on women's satisfaction or dissatisfaction with care providers – based on data in the table it appears that women had care from multiple providers- general practitioners midwives and obstetricians and it appears that the proportion of women dissatisfied with the health professionals was lower when reporting on obstetrician care than for GP or midwife care – is this correct – some comment on this would be valuable Limitations are clearly outlined.

REVIEWER	Scott, Hannah University College London Hospital, Obstetrics and Gynaecology
REVIEW RETURNED	15-Feb-2021

GENERAL COMMENTS	Thank you for the opportunity to read this interesting and important manuscript. Exploring migrant women's experiences of care is of critical importance, particularly at a time of increasing migration across Europe. As you have correctly described, studies consistently show that refugee and migrant women are marginalised from healthcare and have worse maternal and fetal outcomes. I commend your commitment to research in this underexplored area. I have some concerns with the study design in terms of the timing and personnel used to perform the interview which I have described below. I believe these concerns could be addressed and clarifications made which would enable publication. I support the decision of face-to-face interviewing with interpretation to avoid missing women who have limited language or literacy. However, having the interview on the postnatal ward with healthcare staff limits the ability of the participants to answer honestly about their peripartum care as their answers would be influenced by social desirability bias. If they are still inpatients and their care is on-going, they may also perceive that their care could be influenced by the answers they give. It also appears as if they were recruited by the midwife giving direct care which introduces social desirability bias. As the interviews took place so soon after delivery, I feel the study is limited in its ability to draw conclusions about the participants post-partum care. The post partum period involves the six weeks after delivery, and would include important complications such as secondary haemorrhage, post-partum infections and readmissions. It would not be possible to assess women's postpartum care this early in the post-partum period and therefore conclusions about postpartum care should be removed from the manuscript.

	I am also concerned that the participants have not had enough time to meaningfully reflect on their birth experience so soon after delivery, and they may be influenced, as mentioned above, by giving their feedback to staff who may have been directly or indirectly involved in their care or perceived as being part of the hospital team (not neutral researchers). Interviews taking place in the hospital itself is problematic for this reason. I recommend this article is rewritten, addressing the concerns described above and focusing on women's experience of antenatal care it would eliminate some of the limitations and concerns described above. I think the study makes some important points about antenatal care and this could be drawn out and compared to the existing literature. There are some specific typographical errors or other questions related to the manuscript below: Page 4- I feel more space could be given here to describing the objectives of the study Page 4. Line 20- spelling error- 'consists' Page 4. Line 21- spelling error- 'literature suggests' Page 5 Line 32- could you clarify which language the MFMCQ is being translated into here? Page 5. Line 44- Consider rephrasing this sentence. Starting: Norway has universal health coverage and essential healthcare before, during and after birth is free of charge for members of the a ….. Page 5. Line 56. Is it the midwives providing the patients care who are recruiting for the study on arrival? If so, this introduces bias as the patients may feel that their care is impacted by their willingness to partake in the study. If recruitment is by a separate research midwife not directly caring for the patient, then clarify that here. Page 7. Line 20. Suggest rephrasing the line 'a bit less than every fifth woman had a C-section' Page 9- Line 16- Rephrase/ spelling error; 'Decreased odds of being dissatisfaction ' Page 9. Line 29. Dissatisfaction during post partum- this is only 2-3 days post partum and immediately following birth. This is too early for comments on satisfaction on post partum care and I would posit too soon for meaningful reflections on the satisfaction of the birth experience. Page 9- line 40-52. This section has important findings for refugee women's experiences of their care. Could you explore further here which sub-groups of women experienced these issues as this is your defined objective? Additionally, I wonder how these figures compare to Norweigan women? It would be interesting to see if refugee women experience these factors at a higher rate than Norweigan women (although you have later specified this is not your objective. Perhaps a further sentence to explain why this is not your objective) Page 10- 'compared to other women'. Which other women? Do you mean non-refugee women? Page 11, line 24. Spelling error- 'emphasises' Page 11, line 57. Spelling error- 'indicated' Page 11, line 59. Spelling error – 'factors'

VERSION 1 – AUTHOR RESPONSE

RESPONSE TO REVIEWERS

Reviwer 1. Prof. V Schmied, Western Sydney University	**Response**
This study is important because of the efforts made to ensure that migrant women have an avenue to report their satisfaction – the qualitative or open ended responses would also be very interesting and important to report if there is an opportunity.	Thank you for reviewing our manuscript. We agree that including the open-ended questions would be interesting and thank the reviewer for the suggestion to include these. However, we believe that it would be outside of the scope of this article as the article´s objective was to determine factors associated with recently migrated women's satisfaction with maternity care. The MiPreg study also includes a qualitative work-package with in-depth interviews of migrant women and healthcare personnel. We therefore chose to include the open-ended questions in a forthcoming article we are planning where we triangulate our findings from the questionnaire and the in-depth interviews, as this will allow us to explore the open-ended responses thoroughly.
Background to the study is appropriate and provides enough information based on current literature to identify the need for the study. The rationale for the project is as follows "The Mipreg-project is a multidisciplinary, mixed method project that seeks to identify factors that explain disparities in pregnancy outcomes among recently migrated women giving birth in urban Oslo, Norway"… However, no data or literature is used to demonstrate the situation om Norway.	Thank you for your comment. We have now added a sentence to describe the situation of maternal migrant health in Norway, in the last paragraph in Background: "*Disparities in maternal health outcomes and suboptimal quality of maternity care for migrants are also reported from Norway*[4 9 23 24]*. In order to improve quality of care it is important to gain more knowledge about determinants of migrated women's satisfaction with maternity care. A literature gap exists regarding these determinants, especially for the most recently arrived groups of migrants*" (page 3, line 30-34) In addition, under Methods, we have combined the sub-headings *study design* and *study setting*, to inform the reader about the healthcare structure in Norway earlier on in the manuscript. We hope that we have sufficiently informed about the maternal health and migrant situation in Norway.

Methods This sentence needs correcting “The midwives informed about the study in the women´s language of choice and a written consent was obtained.”.. think should be midwives informed women about the study …	We thank the reviewer for highlighting this; we have now altered the sentence accordingly: “*The research personnel informed women about the study and a written consent was obtained*” (see page 4, line 33-34).
Please explain a little more on how user representatives were involved in the design and conduct of the study.	Thank you for your suggestion, we have expanded lines 42-43, 1-3 on page 5-6 to read: “*The MiPreg-project has, from the design phase throughout the implementation phase, involved user-representatives from non-governmental organizations and relevant migrant communities within the greater Oslo-area. The user-representatives gave feedback on readability, validity and cultural sensitivity of the questionnaire before data collection. After data collection, preliminary findings were presented, and interpretations were discussed with the user-representatives*”.
The following statement is made in relation to the sample calculation “Since the proportion of satisfaction was unknown before study start, maximum number of women required was estimated to 385 assuming a width of 10% for the estimated proportion with a 95% confidence interval (CI). This needs to be explained more clearly for readers. Could the sample have been calculated on previous samples that used the same survey instrument, even though it was in a different population?	Thank you for your comment. When we planned our study, no articles using the questionnaire MFMCQ had been published. Hence, we could not use previous samples using the same survey. To our knowledge, surveys measuring satisfaction with maternity care among migrants specifically are limited. We found one article by Brown and Lumely, 1998, that uses a cross-sectional survey of more than 1000 Australian women measuring their experiences of care in labour and birth. They found that women of non-English speaking background were less likely to have a positive experience of care (58.8% versus Australian born women who had a percentage of 73.3%). Using this article to estimate sample size, we found that a sample size of 358 women was required to detect a difference of 14% between two groups. However, as we did not use the same survey nor had the same sample population and assessed satisfaction both antenatal care and care during birth, we chose to calculate sample size based on unknown population. We therefore estimated the required number of women to be 385 assuming a width of 10% for

	the estimated proportion with a 95% confidence interval. We agree that the sample size statement is not clear and have altered the section on sample calculations using the Australian study that we refer to in the discussion: "*A sample size of approximately 360 women was required to detect a difference of 14% between 2 groups with and without full satisfaction, assuming that the proportion of fully satisfied women was 73% as the reference/control group. A 2-sided significance level of .05 and 80% power was used. We decided to include approximately 400 women to take potential missing values into account. The calculation of sample size was performed with Stata/SE version 16.1*" (page 5, line 23-27) We hope that our explanation is clear and would be happy to consider alternative wording if necessary.
Overall, the results are well presented and described. In the demographic table the regions that immigrants came from are listed – what were the high income locations? – the authors indicated they were most interested in women from low and middle income but a reasonable proportion were from high income countries.	Thank you for highlighting this, we now see that the eligibility criteria were insufficiently described in the original manuscript. We used the Global Burden of Disease classification to include women in our study. The Global Burden of Disease framework has established seven super-regions based on cause of death pattern (https://www.iapb.org/learn/vision-atlas/about/definitions-and-regions/). According to their classification, the group "High income countries" consists of the regions "Western Europe", "Southern Latin America", "North America", "Asia Pacific" and "Australasia". All other countries are grouped in one out of six super-regions; "Latin America & Caribbean"; "Sub-Saharan Africa"; "North Africa & Middle East"; "South East Asia, East Asia & Oceania"; "South Asia" and "Central Europe, Eastern Europe & Central Asia".

	We only included women born in one of these six super-regions. We did not include women born in high-income countries. This may be confusing to some readers as some countries in the super-region “Central Europe, Eastern Europe & Central Asia” belong to the “high-income group” in other classification systems, such as the World Bank classification. To clarify, we have therefore altered the ´Study participants‘ to: “*We included internationally migrated, recently pregnant women with a length of stay in Norway ≤ 5 years, giving birth in urban Oslo. We excluded migrants born in high income countries, according to the Global Burden of Disease framework*”. (page 4, line 14-16) Correspondingly, we removed the super-region “high-income” under the sub-heading ´explanatory variables´ for clarification.
Discussion The discussion is clear and all outcomes have been discussed In the discussion the following statement is made..”Care during pregnancy was the time-period with highest proportion of dissatisfaction in our study. Contrary, a Dutch study showed that non-western migrants were most satisfied with the antenatal care 36, while a British study found little difference in satisfaction between the three periods 32 “ First I noted this should probably say ..”Contrary to this, …” Also these differences might be explained by the different ways maternity care is organised and delivered between countries as noted but not just noninterventionist care but also continuity of care. In the Netherlands for example it may also be more likely that the woman sees the same care provider, particularly a midwife during antenatal care but in UK she is more likely to see different care providers.	We thank the reviewer for valuable input. We have now altered the sentence and have added the suggestion of including ´continuity of care´.
No comment is made on women’s satisfaction or dissatisfaction with care providers – based on data in the table it appears that women had care from multiple providers- general practitioners midwives and obstetricians and it appears that	We thank the reviewer for highlighting this interesting finding. Table 1 shows column percentage and we see that among the dissatisfied women most received care from a general practitioner (83%) and/or a midwife

the proportion of women dissatisfied with the health professionals was lower when reporting on obstetrician care than for GP or midwife care – is this correct – some comment on this would be valuable	(86%), while fewer received care from an obstetrician (28%). This is in line with the organisation of the antenatal care in Norway where only high-risk pregnancies normally receive care from obstetricians. When looking at the row-percent, we see an almost similar percentage of dissatisfied women among the ones receiving care from an obstetrician (28.1%), a general practitioner (28.7%) and a midwife (29.0%). We have updated the paragraph in the Results section according to your comments: "*No difference in dissatisfaction was found for women receiving maternity care from a general practitioner (28.7%), a midwife (29.0%) or an obstetrician (28.1%)".* (page 7, line 13-15) We have also included a sentence about this finding in the Discussion where we refer to a study that showed higher satisfaction with care when the women received maternity care by midwives as compared to doctors: "*Contradicting previous research we found no difference in women´s satisfaction with maternity care given by a general practitioner or a midwife[38].* (page 11, line 25-26)
Limitations are clearly outlined.	Thank you.

Reviewer: 2. Dr. Hannah Scott, University College London Hospital	**Response**
Thank you for the opportunity to read this interesting and important manuscript. Exploring migrant women's experiences of care is of critical importance, particularly at a time of increasing migration across Europe. As you have correctly described, studies consistently show that refugee and migrant women are marginalised from healthcare and have worse maternal and fetal outcomes. I commend your	Thank you for reviewing our manuscript and for all your relevant comments and suggestions, especially on timing and personnel used for interviewing the migrant women. We agree with the reviewer that social desirability bias may have been introduced as healthcare personnel conducted the interviews.

commitment to research in this underexplored area. I have some concerns with the study design in terms of the timing and personnel used to perform the interview which I have described below. I believe these concerns could be addressed and clarifications made which would enable publication. I support the decision of face-to-face interviewing with interpretation to avoid missing women who have limited language or literacy. However, having the interview on the postnatal ward with healthcare staff limits the ability of the participants to answer honestly about their peripartum care as their answers would be influenced by social desirability bias. If they are still inpatients and their care is on-going, they may also perceive that their care could be influenced by the answers they give. It also appears as if they were recruited by the midwife giving direct care which introduces social desirability bias. As the interviews took place so soon after delivery, I feel the study is limited in its ability to draw conclusions about the participants post-partum care. The post partum period involves the six weeks after delivery, and would include important complications such as secondary haemorrhage, post-partum infections and readmissions. It would not be possible to assess women's postpartum care this early in the post-partum period and therefore conclusions about postpartum care should be removed from the manuscript. I am also concerned that the participants have not had enough time to meaningfully reflect on their birth experience so soon after delivery, and they may be influenced, as mentioned above, by giving their feedback to staff who may have been directly or indirectly involved in their care or perceived as being part of the hospital team (not neutral researchers). Interviews taking place in the hospital itself is problematic for this reason. I recommend this article is rewritten, addressing the concerns described above and focusing on women's experience of antenatal care it would	However, recruitment and interviews were done by separate research healthcare personnel, three midwives and a medical doctor, who did not provide healthcare to the women. Thus, if social desirability bias did occurr, we expect its effect to be minor. We have clarified this in the text. Under Methods, Data collection: "*The research personnel informed women about the study and a written consent was obtained. Thereafter, they conducted the interviews face-to-face in the women's own language of choice after birth, using an interpreter when needed*" (page 4, line 33-36) We have also clarified this under limitations: "*Social desirability bias could also affect the answers, since the interviews were conducted by healthcare personnel in the postnatal ward. However, the interviewing healthcare personnel did not provide care to the participating women ...".* (page 12, line 24-27) We share the concern related to timing of the questionnaire shortly after birth and the interviews taking place at the hospital. We chose this design due to easier access to interpreting services and to include answers from hard-to-reach groups such as undocumented migrants, women who do not attend postnatal follow up or women who do not have a general practitioner. One option was to conduct the interviews post-partum though phone contact. However, after discussion with our user-representatives, we understood that it could be difficult to organize the interviews with phone interpreter, compared to face-to-face interpreter at the hospital, and that some migrant women may be reluctant to participate due to confidentiality issues. When it comes to reporting on the migrant women's experiences of their post-partum care, we agree that the study does not cover the postpartum period. In line with the reviewer's

eliminate some of the limitations and concerns described above. I think the study makes some important points about antenatal care and this could be drawn out and compared to the existing literature.	suggestion, we have therefore removed the responses for the care after birth. In the previous version of the manuscript, overall dissatisfaction was a combined variable for dissatisfaction during pregnancy, birth and postpartum. This has now been changed to a combined variable for dissatisfaction during pregnancy and birth. All corresponding tables/figures and presented findings in Results and Discussion sections have been changed accordingly. We would, however, like to include the time period ´during birth´ in our analysis of satisfaction. Unlike countries with ´continuum of care´, the maternity care in Norway is fragmented. We therefore believe that it is important to include the responses for the satisfaction during birth. Although there are concerns related to immediate postpartum assessments of childbirth experiences, there is no consensus as to the right time. In a systematic review on pain and satisfaction during birth the author, Hodnett, writes: *"There is insufficient evidence on which to base conclusions about the impact of timing of assessment of childbirth satisfaction. There may be no optimum time; it may be dependent on the purpose of the study. Two studies that purported to determine the impact of timing of assessment were excluded because of serious methodological flaws. A qualitative study found that women's ratings in the immediate postpartum period were very similar to those 15 to 20 years later, but some long-term memories were more negative. In a UK trial of midwife-managed care, women's ratings of aspects of their intrapartum care were lower at 7 months than they had been at 7 weeks' postpartum »* (https://doi.org/10.1016/S0002-9378(02)70189-0) We hope that we have sufficiently addressed the concerns described.

There are some specific typographical errors or other questions related to the manuscript below: Page 4- I feel more space could be given here to describing the objectives of the study	Thank you, we have added a sentence in the Background section to describe the situation for migrant maternal health in Norway: "*Disparities in maternal health outcomes and suboptimal quality of maternity care for migrants are also reported from Norway*[4 9 23 24]*. In order to improve quality of care it is important to gain more knowledge about determinants of migrated women's satisfaction with maternity care. A literature gap exists regarding these determinants, especially for the most recently arrived groups of migrants*" (page 3, line 30-34) In Methods, we have added a sentence under ´Explanatory variables´ to describe the healthcare experiences: "*Healthcare experiences were examined by asking the women about eleven specific healthcare experiences, grouped binary as positive or negative experiences".* (page 5, line 19-21)
Page 4. Line 20- spelling error- 'consists'	Thank you, we have corrected the spelling error (page 3, line 15).
Page 4. Line 21- spelling error- 'literature suggests'	Thank you, we have corrected the spelling error (page 3, line 16).
Page 5 Line 32- could you clarify which language the MFMCQ is being translated into here?	We have added the languages under ´Data collection´, see page 4 and line 37-38.
Page 5. Line 44- Consider rephrasing this sentence. Starting: Norway has universal health coverage and essential healthcare before, during and after birth is free of charge for members of the a …..	Thank you for the suggestion, we have altered the sentence to: "*Norway has universal health coverage and essential maternity care is free of charge for all legal citizens*" (page 4 and line 6-7)
Page 5. Line 56. Is it the midwives providing the patients care who are recruiting for the study on arrival? If so, this introduces bias as the patients may feel that their care is impacted by their willingness to partake in the study. If recruitment	We are grateful to the reviewer for highlighting this bias and suggesting re-phrasing the section to clarify that the interviewers did not partake in direct care for the women. Further comments and revised sentences are found in the first response for reviewer 2.

is by a separate research midwife not directly caring for the patient, then clarify that here.	
Page 7. Line 20. Suggest rephrasing the line ‘a bit less than every fifth woman had a C-section’	Thank you for your suggestion. Wording has been changed to: *The majority of women were primiparous. Almost one in four women had induction of labour and almost every fifth women a caesarean section (Table 1).* (page 7, line 12-13)
Page 9- Line 16- Rephrase/ spelling error; ‘Decreased odds of being dissatisfaction ‘	Thank you, we have changed the phrase to: *decreased odds of being dissatisfied*. (page 9 and line 7)
Page 9. Line 29. Dissatisfaction during post partum- this is only 2-3 days post partum and immediately following birth. This is too early for comments on satisfaction on post partum care and I would posit too soon for meaningful reflections on the satisfaction of the birth experience.	Thank you for your comment. We agree that the use of the term ´postpartum´ is not appropriate. As per our first response to reviewer 2, we have now removed ‘*postpartum´* from the text and all analysis.
Page 9- line 40-52. This section has important findings for refugee women’s experiences of their care. Could you explore further here which sub-groups of women experienced these issues as this is your defined objective?	Thank you for your suggestion. We have added a sentence on country of birth for the refugees: *“The majority of refugee women originated from Eritrea (34.1%), Syria (19.5%), Iraq (7.3%) and Somalia (7.3%)”* (page 10, line 4-5) Further subgroup analysis by reason for migration was difficult due to the limited number of refugees in our sample.
Additionally, I wonder how these figures compare to Norweigan women? It would be interesting to see if refugee women experience these factors at a higher rate than Norweigan women (although you have later specified this is not your objective. Perhaps a further sentence to explain why this is not your objective	We agree with the reviewer that it would be an interesting comparison. However, we could not find any publications on Norwegian women´s perception for the specific healthcare experiences as the ones we have measured in MFMCQ. A previous article, using another measurement tool, did not find systematic differences between groups of different geographic origin regarding their experiences with maternity care in Norway (https://doi.org/10.1186/s12884-016-1214-3).

	One of their main limitations is that very few migrants are included in the study as the questionnaire was self-administered and only provided in English/Norwegian. A German article using the same MFMCQ as we have done, found no difference regarding overall satisfaction with care during labor and birth between migrants and non-migrants (https://doi.org/10.1007/s00404-019-05227-4). In our study, we chose to only include migrants, as we wanted to assess migrant specific factors. Furthermore, the MFMCQ questionnaire is developed to be used on a migrant population. We aimed for a ´thick description´ with a relative large, targeted sample size of recently arrived migrants. We agree with the reviewer that comparing migrants to non-migrants would be interesting and we believe it could be an aim for future studies with a different measurement tool.
Page 10- 'compared to other women'. Which other women? Do you mean non-refugee women?	We thank the reviewer for highlighting this; we have now removed 'compared to other women' as the comparison is indicated further on in the sentence. The sentence is therefore changed for clarification: "*More refugee women felt treated differently by healthcare personnel because of religion, skin colour, language etc. (24.4% vs 9.3%, p 0.022) and understood less information (51.2% vs 27.2%, p 0.008), compared to women who migrated due to family reunification and work/education, respectively (Table 2)"* (page 10, line 1)
Page 11, line 24. Spelling error- 'emphasises'	Thank you, we have corrected the typo. (page 11, line 12).
Page 11, line 57. Spelling error- 'indicated'	Thank you, we have corrected the typo. (page 11, line 44).
Page 11, line 59. Spelling error – 'factors'	Thank you, we have corrected the typo. (page 11, line 44).