PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Understanding the perspectives and values of midwives, obstetricians, and obstetric registrars regarding episiotomy: qualitative interview study
AUTHORS	Seijmonsbergen-Schermers, Anna; Thompson, Suzanne; Feijen-de Jong, Esther I.; Smit, Marrit; Prins, M; van den Akker, Thomas; de Jonge, Ank

VERSION 1 – REVIEW

REVIEWER	juliet albert Imperial College Healthcare NHS Trust London, United Kingdon
REVIEW RETURNED	15-Mar-2020

GENERAL COMMENTS	It would be interesting to correlate the reasons for episiotomy with the different job role. Also to see whether those staff more recently qualified had different views and indications for carrying out episiotomy

REVIEWER	María Belén Conesa Ferrer University of Murcia
REVIEW RETURNED	29-Mar-2020

GENERAL COMMENTS	First of all, I´d like to say that I found reviewing this study very pleasant, since you are dealing with a topic of great professional interest and a big impact on the health of the woman and the newborn. I consider that authors should add more information about the rates of episiotomy in The Netherlands, whether the Dutch Health System has written programs on normal delivery care and specifically about the use of episiotomy in normal birth. I personally reckon that authors could introduce the National Institute for Health and Care Excellence Guidance (2014), which was updated in February 2017. This guidande is clear on which indications are valid for episiotomy in the section of Intrapartum care for healthy women and babies: “1.13.21. Perform an episiotomy if there is a clinical need, such as instrumental birth or suspected fetal compromise. [2007]”. Authors include studies about indications for performing episiotomy. According to the studies, one indication is the prevention of major tears. they should add that scientific evidence has confirmed major tears as an adverse effect of episiotomy. In the results section, I would strongly recommend authors to add some figures of the Code Matrix Browser of MaxQDA .

	On checklist section, authors have to add page 7 to ítem no. 11.

REVIEWER	Sophia Brismar Wendel Department of Clinical Sciences, Karolinska Institutet Danderyd Hospital, Stockholm, Sweden
REVIEW RETURNED	23-May-2020

GENERAL COMMENTS	Thank you for the opportunity to review this manuscript of a qualitative study regarding care providers' view on episiotomy. The theme is interesting and results important to inform clinical practice and direct future research. However, there are some areas of improvement in the presentation of this study. Introduction: Page 5, line 14-15: The sentence needs to be rephrased. It is not logical in the present form. "Reduction" in postpartum urinary retention, pain, and dyspareunia would be a good thing? There is not conformity in the evidence regarding pain and pelvic floor strength. Some other studies, not cited here, have demonstrated that pain and strength is rather related to the extent of tissue injury, not the mechanism (iatrogenic or spontaneous) (1, 2). Thus, pain would be worse after a third or fourth-degree perineal injury, than after an episiotomy. After episiotomy, postpartum pain and dyspareunia are similar to the pain reported after a second-degree injury. The introduction should also reflect on the benefits of episiotomy in operative vaginal delivery, reported in the review by Lund et al, and many Dutch studies (3-6). Throughout the paper, both pros and cons of episiotomy could be evolved. Methods: This part of the manuscript could be generally condensed and more stringent to improve reading pleasure. In the first paragraph (page 6, line 15-22), it needs to be more clearly described how many interviews were made by the first author, second author, and research assistants. It adds up to six described interviews, but there were in total 20 interviews. Who made them? Page 6, line 42, there is a spelling mistake ("based6"). The analysis process could be clarified regarding coding, for example by supplying the coding tree. You mention the use of a statistical software (page 8, line 35) but no details on what methods or analyses you made in the software. To be able to repeat the study, the coding process and analyses need to be clear, stringent and more detailed. Results: The results section is long and wordy. Could it be condensed, and still made clear? Maybe a language revision would be beneficial. I think it would be interesting to present and compare results in groups of midwives, obstetricians and registrars, since this was how you chose the interview subjects. Were there differences related to profession, experience or workplace (home births/hospital)? Could attitudes be explained by different types of deliveries attended (spontaneous onset/induced labor, normal/complicated pregnancies, spontaneous delivery/operative delivery)? Episiotomy, and the attitude towards it, may be different depending on these factors. Discussion: References are too often referred to as "personal communication", although there are publications on the matter. For example, the rate

	of episiotomy in Sweden and Denmark can be found in the publication by Blondel et al, or online (page 14, line 42-44) (7, 8). Regional differences in episiotomy rates should be possible to find in an online database or a publication (page 15, line 13)? I miss a balanced discussion on the potential benefits of episiotomy in some situations, like operative vaginal delivery, according to the literature (9). I also miss a description of how doctors and midwives attend deliveries in the Netherlands, the prevalence of spontaneous and operative vaginal deliveries in the included hospitals/regions, and the rate of OASIS and episiotomy in these births. The working hours or way of attending births must differ a lot from how midwives and doctors in Scandinavian countries attend births. According to Table 2, many of the midwives and doctors had attended a surprisingly high number of births (up to 200/year). Do doctors attend normal births in the Netherlands, and what does the word attend imply? In a study of operative vaginal births, we observed that even in the second largest labor ward in Sweden (6700 births/year), the number of operative deliveries attended in one year varied between 1 and 22 (mean 4, median 6) per doctor (10). In the context of that study, “attend” implied being the person holding the handle of the vacuum extractor and being in charge of the choice of episiotomy or not, manual perineal protection, and speed of extraction, in collaboration with the attending midwife and supervising senior doctor in some cases. Table 1: Spelling mistake page 21, line 21 “episiotomiy”.

VERSION 1 – AUTHOR RESPONSE

Comments from reviewer 1:

1) It would be interesting to correlate the reasons for episiotomy with the different job role. Also to see whether those staff more recently qualified had different views and indications for carrying out episiotomy.

We appreciate the suggestion of the reviewer to focus on the characteristics of the participants and the outcomes. It would be very interesting to investigate the correlation between care providers’ characteristics and indications for episiotomy, particularly in quantitative research. However, we did not intend to compare particular groups in this study and did not take this into account in our sampling frame. If differences appeared to be related to different job roles, we noted this. For example on page 12, lines 376-378:

“Mainly for those working in secondary or tertiary care, consultation and supervision of colleagues was an important factor in decision-making.”

Comments from reviewer 2:

1) I consider that authors should add more information about the rates of episiotomy in The Netherlands, whether the Dutch Health System has written programs on normal delivery care and specifically about the use of episiotomy in normal birth.

We added the episiotomy rate in the Netherlands in de introduction section. Besides, in the discussion section, variation in episiotomy rates in the Netherlands have been described.

Page 5, lines 116-119:
“The episiotomy rate in the Netherlands was 46% among nulliparous and 14% among multiparous women, with an instrumental vaginal birth rate of 16% among nulliparous and 3% among multiparous women in 2013.”

Page 14, lines 455-456:
“Previous studies showed large variations in episiotomy rates. In the Netherlands, rates varied among twelve regions from between 14% to 42% for nulliparous women and from between 3% to 13% for multiparous women.”

We added additional information on the lack of guidelines on episiotomy in the Netherlands.

Page 16, lines 501-504:
“However, in some countries national uniform recommendations on episiotomy practice are available, such as the clinical guideline “Intrapartum care for healthy women and babies” from the National Institute for Health and Care Excellence Guidance. In the Netherlands, national guidelines or recommendations on episiotomy practice are lacking.”

2) I personally reckon that authors could introduce the National Institute for Health and Care Excellence Guidance (2014), which was updated in February 2017. This guidance is clear on which indications are valid for episiotomy in the section of Intrapartum care for healthy women and babies: “1.13.21. Perform an episiotomy if there is a clinical need, such as instrumental birth or suspected fetal compromise. [2007]”.

We added a reference to this clinical guideline in the discussion section.

Page 16, lines 501-504:
“However, in some countries national uniform recommendations on episiotomy practice are available, such as the clinical guideline “Intrapartum care for healthy women and babies” from the National Institute for Health and Care Excellence Guidance.”

3) Authors include studies about indications for performing episiotomy. According to the studies, one indication is the prevention of major tears. they should add that scientific evidence has confirmed major tears as an adverse effect of episiotomy.

We added a few sentences on the discussion on episiotomy benefits in the literature, also in response to the comments of peer reviewer 3.

Page 15, lines 469-473:
“On one hand, literature suggests that episiotomy may be beneficial to prevent Obstetric Anal Sphincter Injury (OASI) in some women, particularly in case of instrumental vaginal birth. On the other hand, routine use of episiotomy may paradoxically result in increased rates of OASI and overuse of episiotomy results in unnecessary complaints and morbidity among many women.”

4) In the results section, I would strongly recommend authors to add some figures of the Code Matrix Browser of MaxQDA.

We understand the preference of the peer reviewer to show the Code Matrix in the results section. However, we believe the coding tree is of more added value. We added the coding tree in

Supplementary file 3.

5) On checklist section, authors have to add page 7 to ítem no. 11.

We changed this accordingly.

Comments from reviewer 3:

1) Introduction:
Page 5, line 14-15: The sentence needs to be rephrased. It is not logical in the present form.

We have rephrased this sentence:

Page 5, lines 126-128:
“This suggests that perspectives and values of care providers influence the decision to perform an episiotomy and that this decision is not only based on medical necessity.”

2) “Reduction” in postpartum urinary retention, pain, and dyspareunia would be a good thing? There is not conformity in the evidence regarding pain and pelvic floor strength. Some other studies, not cited here, have demonstrated that pain and strength is rather related to the extent of tissue injury, not the mechanism (iatrogenic or spontaneous) (1, 2). Thus, pain would be worse after a third or fourth-degree perineal injury, than after an episiotomy. After episiotomy, postpartum pain and dyspareunia are similar to the pain reported after a second-degree injury.

The part ‘reduction in’ was a mistake and has been removed. We agree that literature shows that third- or fourth-degree perineal injuries generally result in more complaints than episiotomies. However, we found many indications in the literature that episiotomies lead to more complaints than second degree perineal injuries. Besides, an episiotomy can lead to physical problems, as we stated in the introduction section. Many women will have more limited perineal damage if an episiotomy is avoided, and only a small proportion will suffer from a major perineal tear (third- or fourth degree). We described this in the introduction section in the following sentence.

Page 5, lines 121-122:
“Episiotomies can lead to physical problems, such as postpartum urinary retention, perineal pain, dyspareunia, and pelvic floor muscle strength.”

3) The introduction should also reflect on the benefits of episiotomy in operative vaginal delivery, reported in the review by Lund et al, and many Dutch studies (3-6).

We have now mentioned the potential benefits of episiotomy in the introduction section.

Page 5, lines 120-121:

“For instrumental births, episiotomy may be beneficial to prevent Obstetric Anal Sphincter Injury (OASI) in some women.”

4) Throughout the paper, both pros and cons of episiotomy could be evolved.

We described possible benefits and disadvantages of episiotomy in the introduction and discussion section.

Page 5, lines 119-120:
“For instrumental births, episiotomy may be beneficial to prevent Obstetric Anal Sphincter Injury (OASI) in some women.”

Page15, lines 469-473:
“On one hand, literature suggests that episiotomy may be beneficial to OASI in some women, particularly in case of instrumental vaginal birth. On the other hand, routine use of episiotomy paradoxically result in increased rates of OASI and overuse of episiotomy results in unnecessary complaints and morbidity among many women.”

5) Methods:
This part of the manuscript could be generally condensed and more stringent to improve reading pleasure.

We described all items from the COREQ-checklist, which is very detailed. Nevertheless, we condensed the methods section as much as possible without omitting essential information needed to comply with the COREQ-checklist.

6) In the first paragraph (page 6, line 15-22), it needs to be more clearly described how many interviews were made by the first author, second author, and research assistants. It adds up to six described interviews, but there were in total 20 interviews. Who made them?

We added more information on the number of interviews carried out by each interviewer.

Page 6, lines 153-158:
“The first author interviewed 16 of the 20 participants and is a woman of 30 years, mother, midwife with four years of clinical experience, educated in conducting qualitative studies, and employed as a PhD-candidate in her final year at the time of the study. Most of the participants were unknown to her, but two of the participants were aware of her previous publications on episiotomy in the Netherlands. The first interview was carried out by the first and second author together and one interview was carried out by the second author...”

7) Page 6, line 42, there is a spelling mistake (“based6”).

We changed this accordingly.

Page 6, line 172:
“…purposive sampling was based on…”

8) The analysis process could be clarified regarding coding, for example by supplying the coding tree. You mention the use of a statistical software (page 8, line 35) but no details on what methods or analyses you made in the software. To be able to repeat the study, the coding process and analyses need to be clear, stringent and more detailed.

We added the coding tree in Supplementary file 3 and referred to this in the methods section. Besides, we moved the sentence on the statistical software to the paragraph describing the analyses methods. After this sentence, the methods of coding in the statistical software program has been described.

Page 8, lines 218-227:
“Inductive thematic analysis was conducted, described by Braun and Clarke (2006), making use of statistic software program MAXQDA. Data were read and re-read to become familiarized with them.

Initial codes were generated by coding interesting features of the data and relationships between codes were identified. A first coding tree was developed, and the first five interviews were coded again to identify over-arching codes. During the analyses of the subsequent interviews, the codes were increasingly collated into potential themes and all data relevant to each theme were gathered. After potential themes were identified, these were reviewed by checking the relation to the coded extracts and the entire data set, generating a thematic network. Subsequently, the authors applied a name and a description for each theme (see the code tree in Supplementary file 3)."

9) Results:
The results section is long and wordy. Could it be condensed, and still made clear? Maybe a language revision would be beneficial.

We made some revisions to shorten the results section. These changes are marked with track changes.

10) I think it would be interesting to present and compare results in groups of midwives, obstetricians and registrars, since this was how you chose the interview subjects. Were there differences related to profession, experience or workplace (home births/hospital)? Could attitudes be explained by different types of deliveries attended (spontaneous onset/induced labor, normal/complicated pregnancies, spontaneous delivery/operative delivery)? Episiotomy, and the attitude towards it, may be different depending on these factors.

We appreciate the suggestion of the reviewer to compare results in groups of professional background. It would be very interesting to investigate the correlation between the professional background and the perspectives, particularly in quantitative research. However, we did not intend to compare particular groups in this study and did not take this into account in our sampling frame. If differences appeared to be related to different job roles, we noted this. For example on page 12, lines 376-378:

"Mainly for those working in secondary or tertiary care, consultation and supervision of colleagues was an important factor in decision-making."

11) Discussion:
References are too often referred to as "personal communication", although there are publications on the matter. For example, the rate of episiotomy in Sweden and Denmark can be found in the publication by Blondel et al, or online (page 14, line 42-44) (7, 8). Regional differences in episiotomy rates should be possible to find in an online database or a publication (page 15, line 13)?

The studies referred to as 'personal communication' were all studies that were almost published. In the meantime, all of these studies have been published and we changed 'personal communication' into the final references.

12) I miss a balanced discussion on the potential benefits of episiotomy in some situations, like operative vaginal delivery, according to the literature (9).

We discussed potential benefits and disadvantages of episiotomy, also in response to the comments of peer reviewer 1.

Page 15, lines 469-473:
"On one hand, literature suggests that episiotomy may be beneficial for some women in instrumental vaginal births. On the other hand, routine use of episiotomy may result in increased rates of OASI and overuse of episiotomy results in unnecessary complaints and morbidity in many women."

13) I also miss a description of how doctors and midwives attend deliveries in the Netherlands, the prevalence of spontaneous and operative vaginal deliveries in the included hospitals/regions, and the rate of OASIS and episiotomy in these births.
The working hours or way of attending births must differ a lot from how midwives and doctors in Scandinavian countries attend births. According to Table 2, many of the midwives and doctors had attended a surprisingly high number of births (up to 200/year). Do doctors attend normal births in the Netherlands, and what does the word attend imply? In a study of operative vaginal births, we observed that even in the second largest labor ward in Sweden (6700 births/year), the number of operative deliveries attended in one year varied between 1 and 22 (mean 4, median 6) per doctor (10). In the context of that study, "attend" implied being the person holding the handle of the vacuum extractor and being in charge of the choice of episiotomy or not, manual perineal protection, and speed of extraction, in collaboration with the attending midwife and supervising senior doctor in some cases.

We agree with the peer reviewer that the workload differs in several ways from how midwives and doctors in Scandinavian countries are working. Obstetricians mostly attend vaginal births with high risks, breech position, instrumental vaginal births, etcetera. However, obstetric registrars attend all kinds of medium- and high-risk births in the clinic, because they are physicians in training to become an obstetrician. The number of attended births varies largely among the participants, improving the generalizability. We now specified 'attended' in the methods section.
Our participants were from all over the country, working in different hospitals. We therefore added the national episiotomy and instrumental vaginal birth rate in the introduction section. We believe describing the OASI rate will not add any additional value to the understanding of the topic of this article, which focuses on perspectives on episiotomy and not on OASI.

Page 7, lines 182-184:
"An 'attended birth' was specified to the participants as a birth where the decision to perform an episiotomy would be made by the themselves."

Page 5, lines 115-118:
"The episiotomy rate in the Netherlands was 46% among nulliparous and 14% among multiparous women, with an instrumental vaginal birth rate of 16% among nulliparous and 3% among multiparous women in 2013."

14) Table 1:
Spelling mistake page 21, line 21 "episiotomiy".

We changed this accordingly.

Our study shows that the decision to perform episiotomy is mainly based on care providers' own insight, and that recommendations from the literature and women's values are of secondary importance in the decision-making. We believe our study will contribute to insights on the process of decision-making on episiotomy. We therefore hope that you will consider publication of our article.

VERSION 2 – REVIEW

REVIEWER	María Belén Conesa Ferrer University of Murcia
REVIEW RETURNED	08-Oct-2020

GENERAL COMMENTS	I consider that the authors have made all the changes required.

REVIEWER	Sophia Brismar Wendel Department of Clinical Sciences, Karolinska Institutet, Danderyd Hospital, Stockholm, Sweden.
REVIEW RETURNED	03-Oct-2020

GENERAL COMMENTS	(https://bmjopenquality.bmj.com/pages/authors/#original_research) and the abstract up to 300 words. In the abstract, there is no methods section. This should be added. 2. Page 3, line 77, 88 and 89. Results should be presented in the past tense (were and was). 3. Page 5, line 119: "For instrumental births, episiotomy may be beneficial to prevent Obstetric Anal Sphincter Injury (OASI) in some women". Which women do you mean? The reference Lund et al refers to all nulliparous women with instrumental delivery. There is no good way of identifying which women are "most suited" for episiotomy. You could omit "in some women" since you already introduce a relevant uncertainty by writing "may". 4. Page 7, line 182: Spelling mistake "the themselves", omit the word "the". 5. Page 14, line 435: No need to repeat "In the Netherlands, rates varied among twelve regions from between 14% to 42% for nulliparous women and from between 3% to 13% for multiparous women" if almost the same is written in the introduction. Choose one place to describe the situation in the Netherlands. Page 14, line 459: "...a higher rate of episiotomy was found in regions with lower rates of home births, also among women in obstetrician-led care. This suggests that vision may be an important contributor to the tendency to intervene." I think there may also be a selection bias towards more complicated births and women with higher risk? Or is the higher frequency only driven by the lack of home birth alternatives? You may want to comment on this or rephrase. Page 15, line 481: "...national uniform recommendations on episiotomy practice are available, such as the clinical guideline “Intrapartum care for healthy women and babies” from the National Institute for Health and Care Excellence Guidance." This guideline states that one should "Perform an episiotomy if there is a clinical need, such as instrumental birth or suspected fetal compromise." However, the problem is that "clinical need" is not clearly defined. Using "such as" implies other possible situations and leaves also this guideline with gaps. You may want to rephrase. Page 17, line 557: In the conclusion the past tense should be used to present the findings of this study. The present tense gives the impression that these statements are commonly accepted facts. Tables and figures are nice.

VERSION 2 – AUTHOR RESPONSE

Requests from reviewer 1:

1) The word count is not correct. I get 5904 words (main text from introduction to conclusion) and according to author instructions it should be no more than 4000 words in the main text (https://bmjopenquality.bmj.com/pages/authors/#original_research) and the abstract up to 300 words. In the abstract, there is no methods section. This should be added.
We are aware that our article exceeds the recommended word count in the author instructions. We explained this in our first cover letter to the editor. The quotes of the participants are written down in the results section, which results in a higher word count. The author guidelines of BMJ Open through the link https://bmjopen.bmj.com/pages/authors/ states that the maximum word count is flexible and exceeding the word count should be mentioned in the cover letter.
We changed the text and stated the correct word count of 5,923 words.

2) Page 3, line 77, 88 and 89. Results should be presented in the past tense (were and was).
We changed this accordingly.
3) Page 5, line 119: "For instrumental births, episiotomy may be beneficial to prevent Obstetric Anal Sphincter Injury (OASI) in some women". Which women do you mean? The reference Lund et al refers to all nulliparous women with instrumental delivery. There is no good way of identifying which women are "most suited" for episiotomy. You could omit "in some women" since you already introduce a relevant uncertainty by writing "may".
We changed this sentence into “For instrumental births, episiotomy may be beneficial to prevent Obstetric Anal Sphincter Injury (OASI)”.
4) Page 7, line 182: Spelling mistake "the themselves", omit the word "the".
We removed the word ‘the’.
5) Page 14, line 435: No need to repeat "In the Netherlands, rates varied among twelve regions from between 14% to 42% for nulliparous women and from between 3% to 13% for multiparous women" if almost the same is written in the introduction. Choose one place to describe the situation in the Netherlands.
We replaced this sentence in the introduction section and removed it from the discussion section.
Introduction section, page 5, lines 114-119:
“There is major variation in episiotomy practice worldwide1 3, with rates varying from 4% in Denmark4 to 91% in Thailand5. The episiotomy rate in the Netherlands was 46% among nulliparous and 14% among multiparous women, with an instrumental-vaginal birth rate of 16% among nulliparous and 3% among multiparous women in 20136. Rates varied among twelve regions from 14% to 42% for nulliparous women and from 3% to 13% for multiparous women7.”

6) Page 14, line 459: "...a higher rate of episiotomy was found in regions with lower rates of home births, also among women in obstetrician-led care. This suggests that vision may be an important contributor to the tendency to intervene." I think there may also be a selection bias towards more complicated births and women with higher risk? Or is the higher frequency only driven by the lack of home birth alternatives? You may want to comment on this or rephrase.
This phrase was indeed somewhat unclear. We changed this into the following sentences to clarify this. It is not probably that this situation was caused by selection bias, because these ORs were adjusted for maternal characteristics. Besides, the analyses were based on the women’s residential postal code instead of the postal code of the place of birth, thus the presence or absence of a university hospital in a region will have had limited influence on the results and regions were therefore quite comparable.
Discussion section, page 14, line 459:

“In a study of Seijmonsbergen et al. on regional variation of episiotomy in the Netherlands, a higher rate of episiotomy was found in regions with lower rates of home births. In regions with lower rates of home births, episiotomy rates in obstetrician-led care were also higher7.”

7) Page 15, line 481: "...national uniform recommendations on episiotomy practice are available, such as the clinical guideline “Intrapartum care for healthy women and babies” from the National Institute for Health and Care Excellence Guidance." This guideline states that one should "Perform an episiotomy if there is a clinical need, such as instrumental birth or suspected fetal compromise." However, the problem is that "clinical need" is not clearly defined. Using "such as" implies other possible situations and leaves also this guideline with gaps. You may want to rephrase.

We added the following sentence on page 15, line 484:

“On the other hand, this guideline leaves room for different understandings of the clinical need for an episiotomy.”

8) Page 17, line 557: In the conclusion the past tense should be used to present the findings of this study. The present tense gives the impression that these statements are commonly accepted facts.

We changed this accordingly.

VERSION 3 – REVIEW

REVIEWER	Sophia Brismar Wendel Karolinska Institutet, Sweden
REVIEW RETURNED	10-Nov-2020

GENERAL COMMENTS	Thank you for the opportunity to review the revised manuscript "Understanding the perspectives and values of midwives, obstetricians, and obstetric registrars regarding episiotomy: qualitative interview study" and for your patient answers. My only remaining question is whether there should be a methods section in the abstract. This should be resolved by the editors. I wish you good luck with the publication and future research.