

Community Dentistry and Oral Epidemiology - Manuscript ID CDOE-20-202

From: Michelle Martire (onbehalf@manuscriptcentral.com)

To: ninuk.hariyani@adelaide.edu.au; ninuk_hariyani@yahoo.co.id

Date: Monday, 6 April 2020 at 06:03 pm GMT+7

06-Apr-2020

Dear Dr Hariyani:

Your manuscript entitled "Maternal caries experience influences offspring's early childhood caries - a birth cohort study" has been received by the editorial office of Community Dentistry and Oral Epidemiology. Review procedures will now be handled by the editor.

Your manuscript ID is CDOE-20-202.

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Thank you for submitting your manuscript to Community Dentistry and Oral Epidemiology.

Sincerely,
Community Dentistry and Oral Epidemiology Editorial Office

Community Dentistry and Oral Epidemiology - Decision on Manuscript ID CDOE-20-202

From: Sarah Baker (onbehalf@manuscriptcentral.com)
To: ninuk.hariyani@adelaide.edu.au; ninuk_hariyani@yahoo.co.id
Cc: cdoejournal@wiley.com
Date: Monday, 11 May 2020 at 03:46 pm GMT+7

11-May-2020

Dear Dr Hariyani:

The initial reviews for manuscript ID CDOE-20-202 entitled "Maternal caries experience influences offspring's early childhood caries - a birth cohort study" which you submitted to Community Dentistry and Oral Epidemiology, have been completed. The comments of the reviewers are at the end of this letter.

We recognise that the impact of the COVID-19 pandemic may affect your ability to return your revised manuscript to us within the requested timeframe. If this is the case, please let us know.

You will see that the reviewers have provided detailed and constructive comments on your manuscript and recommended some major revisions.

To revise your manuscript, log into <https://mc.manuscriptcentral.com/cdoe> and enter your Author Center, where you will find your manuscript title listed under "Manuscripts with Decisions." Then click on "Continue Submission." Your manuscript number has automatically been amended to denote a revision.

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When submitting your revised manuscript, please put your responses to the comments made by the reviewers (other than minor edits) in the space provided. You can use this space to document any changes you make to the original manuscript. Be sure to address all issues raised by the reviewers. If you disagree with a reviewer, this is where you justify your position.

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IMPORTANT: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing the submission.

Your revised manuscript should be uploaded within three months. If this time schedule creates difficulties for you then do let me know.

Once again, thank you for submitting your manuscript to Community Dentistry and Oral Epidemiology. I look forward to receiving your revision.

Sincerely,
Professor Sarah Baker
Editor-in-Chief, Community Dentistry and Oral Epidemiology
s.r.baker@sheffield.ac.uk

Dr Luisa Borrell
Associate Editor, Community Dentistry and Oral Epidemiology

Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author

Thank you for the opportunity to review this manuscript. Overall, it was enjoyable to read, and the author has used clear scientific language. I have detailed a few points below where I believe the text needs expanding. The introduction and discussion in particular lacked significance and the implications of the findings. With some minor edits and further exploration in some areas this paper will be ready for publication.

Introduction: Your language in the introduction, compared to the discussion flips between US and UK spelling. One style should be used.

Paragraph 3: There are many approach to and types of dyadic pair studies. Which one did this chose and what was the rationale for that choice? This paragraph introduces the importance of the mother-child dyadic relationship, but I think some significance of this type of research can be introduced by discussing the methodological approach and what the impact of studies like this have.

Paragraph 5: The importance of SSB consumption hasn't been established in this very brief introduction. Is SSB consumption a prevalent problem? What have studies found about the impact of the pattern/mode of consumption of SSB?

Paragraph 6: You've stated the aim of the study but I feel the significance of the research hasn't been established.

Page 4 lines 20-26 & 50-53: Reference/s?

Methods/results: How many examiners were there? And what was the reliability between them? If this wasn't done, state why. Did they measure SSB consumption in the children?

There was a significant difference in age, education and income between the number of participants in the study overall and those who were examined. Did the authors consider a weighted analysis either to the full study sample or SA population due to the lack of participation from younger and lower income mothers? Further detail should be included in the methods to justify the choice of methods, as the discussion states a strength of the study was its population-based sample. The bivariate analysis didn't find any significant difference between education, income and ECC (maternal age wasn't in this table but is in the others?), but the methods and results should be clearer about the effect/lack of effect from this slightly skewed sample.

Discussion: The discussion seemed to overall cover the right points, but lacked depth in terms of exploring the significance of the findings, what the direction of future research should be and the policy/intervention implications of the findings. This should be expanded in each part of the discussion and tied together in the final paragraph.

Page 9 line 49: Full stop should be before reference

Reviewer: 2

Comments to the Author

This manuscript assessed the association between the experience of caries in mothers and their offspring. The theme is relevant to dental public health; the database is admirable; the study is well organized; the text reads smoothly. I consider that this manuscript is a nice documentation of the theme. Results reported may implement the planning of dental health.

That said, let us focus on some criticism aimed at improving the communication or, at least, accumulating reflexive thinking with the authors.

1. Longitudinal or cross-sectional design

I have heard some bells ringing at the Introduction. The target association had already been assessed, but previous evidence was limited, the studies followed a cross-sectional design. Would this study follow a longitudinal design? The answer presumably is yes, if we stand with the acknowledgment of strengths. However, the answer is presumably no if we notice that dental examinations of mothers and children were concurrent!

Although this study used data collected for a cohort, I have not seen anything other than a cross-sectional assessment. Variables were gathered into the analysis without reference to when it was measured. The assessment of all covariates was irrespective of time. The statistical assessment did not use any resource of longitudinal data analysis. The multivariable models have not classified the factors by time.

Neither this study was properly longitudinal, nor is a cross-sectional study on this issue less valuable. Mother's caries experience is progressive in time; their current condition may be an effective proxy of their previous status. The same can be thought to socioeconomic status. Income and education would not differ substantially from childbearing to celebrating the second birthday.

2. Correspondence between Methods and Results

Why is it necessary to assess goodness of fit twice by almost the same statistic? Are the AIC and the DIC so different that both of them have to be assessed? If they do, why is this information missing in Results? The models observed a conceptual framework of analysis; the authors have not considered the goodness of fit in statistical modeling. Why, then, have they announced the two information criteria?

The same can be said to the interaction term between maternal and child brushing. Multivariable models already consider additive interaction between factors included in the analysis. When considering a multiplicative interaction, it is desired an explanation of its conceptual meaning. However, the interaction term was not considered in the conceptual DAG; it was not reported in any table, nor was its effect on the goodness of fit.

3. Study limitations

Less than half a sentence to acknowledge study limitations is not enough.

4. Naturalizing socioeconomic inequalities in health

Maternal country of birth is a critical covariate in this assessment. Having visited Adelaide once, I would dare to hypothesize that, in that city, women born in Australia, New Zealand, and the UK might have experienced better socioeconomic conditions during the lifetime than women born elsewhere. Immigration is a matter of serious concern in the promotion of health and social justice.

However, so few were said to explore the link between maternal country of birth and the ECC status of children. I found it horrible (anecdotal evidence not supported by empirical data or literature) to state that this link might be due to cooking methods or preference for certain types of food.

Community Dentistry and Oral Epidemiology - Manuscript ID CDOE-20-202.R1

From: Michelle Martire (onbehalf@manuscriptcentral.com)

To: ninuk.hariyani@adelaide.edu.au; ninuk_hariyani@yahoo.co.id

Date: Saturday, 4 July 2020 at 10:25 pm GMT+7

04-Jul-2020

Dear Dr Hariyani:

Your manuscript entitled "Maternal caries experience influences offspring's early childhood caries - a birth cohort study" has been received by the editorial office of Community Dentistry and Oral Epidemiology. Review procedures will now be handled by the editor.

Your manuscript ID is CDOE-20-202.R1.

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Thank you for submitting your manuscript to Community Dentistry and Oral Epidemiology.

Sincerely,
Community Dentistry and Oral Epidemiology Editorial Office

Community Dentistry and Oral Epidemiology - Decision on Manuscript ID CDOE-20-202.R1

From: Luisa Borrell (onbehalf@manuscriptcentral.com)
To: ninuk.hariyani@adelaide.edu.au; ninuk_hariyani@yahoo.co.id
Cc: cdoejournal@wiley.com
Date: Friday, 24 July 2020 at 03:58 pm GMT+7

24-Jul-2020

Dear Dr Hariyani:

I am pleased to tell you that your manuscript entitled "Maternal caries experience influences offspring's early childhood caries - a birth cohort study" is now accepted for publication in Community Dentistry and Oral Epidemiology.

As part of the journal's continued commitment to its authors, the Editorial Office and Publisher wish to keep you informed about what happens next. As the following information contains important information regarding journal publication and services for authors, you may wish to save it for future reference.

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With best wishes,

Dr Luisa Borrell
Associate Editor, Community Dentistry and Oral Epidemiology
Luisa.Borrell@sph.cuny.edu

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Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author

The initial review was favorable; the authors revised the manuscript appropriately.

Reviewer: 2

Comments to the Author

I am happy to recommend an acceptance on the article on the proviso that the authors address the following:

Response to reviewer 1, comment 7: Please include this explanation in the text of the manuscript. Your rationale is justified and will add clarity and rigour to your manuscript to include this brief explanation. Similarly, the explanation provided in response to comment 9 needs to be included in the limitations paragraph of the discussion.

The language still has both UK and US spelling.



Department of Dental Public Health

Faculty of dental medicine
Universitas Airlangga

Ninuk Hariyani
Lecturer and Researcher

4th July 2020

Professor Sarah Baker
Editor-in-Chief
Community Dentistry and Oral Epidemiology

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Dear Professor Baker,

We are very grateful to you for the opportunity to revise our manuscript and we acknowledge the time spent by the editors and reviewers in commenting on this paper. Please find below a point-by-point reply to reviewers' and editors' comments. We hope that we have now addressed the concerns raised and believe that the manuscript has been substantially improved.

Thanking you.

Yours sincerely,

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Reviewer(s)' Comments to Author:

Reviewer: 1

Comment 1:- Thank you for the opportunity to review this manuscript. Overall, it was enjoyable to read, and the author has used clear scientific language. I have detailed a few points below where I believe the text needs expanding. The introduction and discussion in particular lacked significance and the implications of the findings. With some minor edits and further exploration in some areas this paper will be ready for publication.

Response:- Thank you for your constructive feedback on our manuscript.

Comment 2:- Introduction: Your language in the introduction, compared to the discussion flips between US and UK spelling. One style should be used.

Response:- The changes have been made.

Comment 3:- Paragraph 3: There are many approach to and types of dyadic pair studies. Which one did this chose and what was the rationale for that choice? This paragraph introduces the importance of the mother-child dyadic relationship, but I think some significance of this type of research can be introduced by discussing the methodological approach and what the impact of studies like this have.

Response:- These points have been added (page 3 line 88-90).

Comment 4:- Paragraph 5: The importance of SSB consumption hasn't been established in this very brief introduction. Is SSB consumption a prevalent problem? What have studies found about the impact of the pattern/mode of consumption of SSB?

Response:- The explanation of this point has been added in the introduction (page 4 line 104-109) and in the reference section (page 11-12 line 340-351).

Comment 5:- Paragraph 6: You've stated the aim of the study but I feel the significance of the research hasn't been established.

Response:- It has been added in page 4 line 121-122.

Comment 6:- Page 4 lines 20-26 & 50-53: Reference/s?

Response:- The references have been added (in the introduction page 3 line 74-76 and line 91-93 and in the reference section page 11 line 309-310 and line 329-333).

Comment 7:- Methods/results: How many examiners were there? And what was the reliability between them? If this wasn't done, state why.

Response:- There were 4 examiners including two authors (LGD and DHH). All were trained and calibrated by LGD in a two-day training session. Because of children's young age, re-examinations were not conducted. Instead, examiners worked together as examiner/recorder pairs during the first stage to develop consistency. The examiners took turn to examine and discussed procedures and observations.

Comment 8:- Did they measure SSB consumption in the children?

Response:- Yes, there are measurements of SSB consumption in the children as part of a 3-day diet diary, collected when children had turned 1 year and a 87-item Food Frequency Questionnaire, collected when children had turned two years. We did not adjust for children's SSB consumption in the models because it is related only to children's ECC, not mothers' caries experience.

Comment 9:- There was a significant difference in age, education and income between the number of participants in the study overall and those who were examined. Did the authors consider a weighted analysis either to the full study sample or SA population due to the lack of participation from younger and lower income mothers? Further detail should be included in the methods to justify the choice of methods, as the discussion states a strength of the study was its population-based sample. The bivariate analysis didn't find any significant difference between education, income and ECC (maternal age wasn't in this table but is in the others?), but the methods and results should be clearer about the effect/lack of effect from this slightly skewed sample.

Response:- Attrition is inherent in longitudinal research. The relatively lower retention rate among the low SES groups than the high SES groups was expected. We attempted to enrol more participants from the low SES groups in order to maintain adequate numbers of participants in the low SES groups in follow-up analysis. While the younger and lower income mothers failed to

follow-up at a greater rate, the numbers in the followed-up sample were still adequate for the analysis. Our objective was the scientific goal of understanding a phenomenon through multivariable analysis. In such cases, a fully representative sample is not always necessary (Rothman et al. Why representativeness should be avoided. Int J Epidemiol 2013; 42(4): 1012–1014). The discussion of the strength of the study as having a population-based sample was because the sample has all necessary components of the targeted population. That allowed us to control for known confounding factors. The bivariate analysis of maternal age also has been added in Table 2 (page 15 line 395).

Comment 10:- Discussion: The discussion seemed to overall cover the right points, but lacked depth in terms of exploring the significance of the findings, what the direction of future research should be and the policy/intervention implications of the findings. This should be expanded in each part of the discussion and tied together in the final paragraph.

Response:- These points have been added in the discussion (page 10 line 280-287)

Comment 11:- Page 9 line 49: Full stop should be before reference

Response:- The change has been made (page 9 line 250-251)

Reviewer: 2

Comment 1:- This manuscript assessed the association between the experience of caries in mothers and their offspring. The theme is relevant to dental public health; the database is admirable; the study is well organized; the text reads smoothly. I consider that this manuscript is a nice documentation of the theme. Results reported may implement the planning of dental health.

Response:- Thank you very much for your constructive feedback on our manuscript.

Comment 2:- Longitudinal or cross-sectional design

I have heard some bells ringing at the Introduction. The target association had already been assessed, but previous evidence was limited, the studies followed a cross-sectional design. Would this study follow a longitudinal design? The answer presumably is yes, if we stand with the acknowledgment of strengths. However, the answer is presumably no if we notice that dental examinations of mothers and children were concurrent!

Although this study used data collected for a cohort, I have not seen anything other than a cross-sectional assessment. Variables were gathered into the analysis without reference to when it was measured. The assessment of all covariates was irrespective of time. The statistical assessment did not use any resource of longitudinal data analysis. The multivariable models have not classified the factors by time.

Neither this study was properly longitudinal, nor is a cross-sectional study on this issue less valuable. Mother's caries experience is progressive in time; their current condition may be an effective proxy of their previous status. The same can be thought to socioeconomic status. Income and education would not differ substantially from childbearing to celebrating the second birthday.

Response:- We acknowledge that dental examinations of mothers and children were concurrent. However, as the reviewer rightly noted, the mothers' caries experience has accumulated over time to that point; thus, their current condition is an effective indication of their previous disease incidence (because of the chronic, cumulative nature of dental caries as a disease). Some of our covariates were collected at birth of the children (maternal socio-demographic characteristics, maternal behavioural factors and maternal time-restricting conditions). Such use of maternal baseline data lends some aspects of longitudinal analysis. For example, mothers' brushing

behaviour preceded children's brushing behaviour and caries. It also allows for interpretation and dissemination of the findings as providing evidence to support early interventions for women giving birth. A discussion about it has been added in the strengths and limitations section of this paper (page 9-10 line 272-279)

Comment 3:- Correspondence between Methods and Results

Why is it necessary to assess goodness of fit twice by almost the same statistic? Are the AIC and the DIC so different that both of them have to be assessed? If they do, why is this information missing in Results? The models observed a conceptual framework of analysis; the authors have not considered the goodness of fit in statistical modelling. Why, then, have they announced the two information criteria?

Response:- We acknowledge that AIC and DIC were used only to check goodness-of-fit during the model building process. Our models were based on the conceptual framework; hence, those indicators were not important. That was the reason we did not include those indicators in the tables. We have decided to remove it.

Comment 4:- The same can be said to the interaction term between maternal and child brushing. Multivariable models already consider additive interaction between factors included in the analysis. When considering a multiplicative interaction, it is desired an explanation of its conceptual meaning. However, the interaction term was not considered in the conceptual DAG; it was not reported in any table, nor was its effect on the goodness of fit.

Response:- Thank you for pointing this out. We have added explanation of the conceptual meaning to the text (page 7 line 188-191). We also added details of the interaction term in the table 3 (page 16).

Comment 5:- Study limitations

Less than half a sentence to acknowledge study limitations is not enough.

Response:- The discussion about the strength and limitation of this study has been expanded (page 9-10 line 272-279)

Comment 6:- Naturalizing socioeconomic inequalities in health

Maternal country of birth is a critical covariate in this assessment. Having visited Adelaide once, I would dare to hypothesize that, in that city, women born in Australia, New Zealand, and the UK might have experienced better socioeconomic conditions during the lifetime than women born elsewhere. Immigration is a matter of serious concern in the promotion of health and social justice.

However, so few were said to explore the link between maternal country of birth and the ECC status of children. I found it horrible (anecdotal evidence not supported by empirical data or literature) to state that this link might be due to cooking methods or preference for certain types of food.

Response:- Thank you for the suggestions. This point of view has been added in the discussion (page 9 line 260-263 and 265-268) and in the reference section (page 12 line 374-382).