

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Factors associated with breastfeeding in England: an analysis by primary care trust
AUTHORS	Oakley, Laura; Renfrew, Mary; Kurinczuk, Jenny; Quigley, Maria

VERSION 1 - REVIEW

REVIEWER	Dr Deborah Shipton Public Health Research Specialist Glasgow Centre for Population Health Scotland No competing interests
REVIEW RETURNED	01-Mar-2013

THE STUDY	<p>Abstract: the abstract does not fully reflect what these findings add to the field. The last point of the key messages starts to get at this but i think this point needs to be brought into the abstract more, i.e. there are known individual level factors that are associated with BF, and this analysis quantifies how these individual level factors translate to area level associates. The "so what" around this - i.e. that this area-level information will be relevant to decisions around service provision, resource allocation, etc, - needs to be articulated in the abstract.</p> <p>Methods: most relevant things are covered in the methods.... (1) but given that data on smoking in pregnancy can be inconsistently recorded across hospitals it would be good to get some idea of the quality of these smoking data. For example, for what proportion of pregnant women is this information recorded for (nationally) and some indication of the variation by hospital. Thats not to say if these data are poorly recorded in some facilities that it wouldn't be worth including in the analysis but its useful for the reader to have this information to inform their interpretation. (2) lines 47-49, pg 6: it is true that the adjusted figures allow the BF rates across the PCTs to be compared after taking into account the the different distributions but that is not to say that these adjusted figures "can be considered to provide a more accurate comparison...". An unadjusted comparison is a valid comparison for some purposes but as mentioned later in the document is not the best measure when comparing local performance. The text just needs to be amended slightly to reflect/clarify this.</p> <p>I am not a statistician so will be limited in the degree to which i can fully review the random effects model procedure - although i have carried out such analysis. With that caveat the model methods seems good to me.</p>
RESULTS & CONCLUSIONS	By and large the results and conclusions reflect the findings - I have identified below a few area that need clarification or expanding to

	<p>make the messages more clear.....</p> <p>PCT size: the range of sizes of the PCTs is quite large. Is this largely because of a few very large/small PCTs? i.e. would probably be more informative to have the interquartile range.</p> <p>Would be useful to also have the estimate and CI for the % of young mothers (and other non-significant factors) in the adjusted models (e.g. Table 2, 3). You mention that these are non significant but seeing the estimate would allow the reader to see how far the estimate is from reaching significance. As Tables 2/3 are currently presented it is clear what is in and what is out of the adjusted model so might be better to put this extra information in the footnote or a supplementary table.</p> <p>Clarity: pg8 line 55 - misinterpreted this sentence first time round. Might need rewording.</p> <p>Interpretation: given the differences seen in the london and non-london PCTs are quite sticking I think that the discussion of this needs to be expanded. There are issues which may be relevant: (1) the issue of small sample in London is discussed in the limitations but probably also needs to be brought into the interpretation - for example, in discussing factors that failed to reach significance - do they represent a real effect or does it reflect a the small sample/underpowered is relevant? (2) is the BME pop in london significantly different to that outside london, e.g. high prop of 1st generation BME in london versus outside london? (3) in london is deprivation and BME too highly correlated to both exist in same model (These are some of the issues that occurred to me when reading the interpretation of the london/non london differences - they don't all necessarily need to be included but i do think this issue needs to be expanded on in the interpretation)</p>
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REVIEWER	Jennifer Baker Senior Research Associate Institute of Preventive Medicine Denmark
REVIEW RETURNED	07-Mar-2013

THE STUDY	All relevant information is in the manuscript.
REPORTING & ETHICS	The authors should include a statement of ethics so it is clear under what authority the work was reviewed and permitted (or mention that ethical approval was not needed as this study was at the ecological level).
GENERAL COMMENTS	In this study, the authors investigated what factors could be underlying ecological differences in breastfeeding (BF) initiation and duration rates across England. The study used routinely collected data. The paper is well-written and comprehensive. Given that the data were routinely collected, it's important that the authors add to the paper ways in which they checked the data quality. It's not enough to rely upon the Department of Health assessment of quality when using the data for a research purpose; a description of this should be added to the manuscript. The paper is very relevant for the UK audience. By providing a some contextualization such as describing what a primary care trust (PCT) is in greater detail, and

	<p>why the London/non-London divide is so important, it would become of greater relevance for the non-UK audience.</p> <p>Abstract -Please state who the comparison group is for the women of a “Black and Minority Ethnic background” is in lines 23-25.</p> <p>Strengths and limitations It’s subjective to state that the data are of “good quality” without further information to back this up. Please rephrase.</p> <p>Introduction Lines 36+: I was surprised to see that maternal overweight and obesity were not mentioned as being associated with BF initiation and duration as this has been shown in several countries. Please consider including this factor as well.</p> <p>Methods Line 6: Please contextualize a PCT more. From a non-UK perspective, it is not easy to understand what geographic area this encompasses, how many clinics it includes, whether it’s an administrative designation or if it follows city limits, etc. Line 28+: Did the authors investigate if there were any differences in PCTs with and without data that met the Department of Health quality standards?</p> <p>Results P8, lines 4+: It would be useful if the directions of the associations were given.</p> <p>Discussion P9, Limitations: Although an exhaustive list of all variables unavailable for this study cannot be described here, it’s surprising that maternal education was not listed; it seems as though it could explain more of what’s going on rather than speculating about levels of service provision. P10, line 9+: The DH method of dealing with missing data must be mentioned earlier in the methods section as it has great importance for the interpretation of the results.</p> <p>Interpretation of results Parts of this section are repetitive; please consider revising.</p> <p>Table 1: Please provide text for the abbreviations in the footnotes or spell out the abbreviations in the table themselves.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer report 1
 Reviewer: Dr Deborah Shipton
 Public Health Research Specialist
 Glasgow Centre for Population Health
 Scotland

Abstract: the abstract does not fully reflect what these findings add to the field. The last point of the key messages starts to get at this but i think this

point needs to be brought into the abstract more, i.e. there are known individual level factors that are associated with BF, and this analysis quantifies how these individual level factors translate to area level associates. The "so what" around this - i.e. that this area-level information will be relevant to decisions around service provision, resource allocation, etc, - needs to be articulated in the abstract. Authors' response: The abstract has been revised to reflect the key findings of the study.

Methods: most relevant things are covered in the methods....

(1) but given that data on smoking in pregnancy can be inconsistently recorded across hospitals it would be good to get some idea of the quality of these smoking data. For example, for what proportion of pregnant women is this information recorded for (nationally) and some indication of the variation by hospital. That's not to say if these data are poorly recorded in some facilities that it wouldn't be worth including in the analysis but it's useful for the reader to have this information to inform their interpretation.

Authors' response: We have added a sentence to the methods section further explaining the DH quality checks imposed on these data. In the discussion section we have stated how many PCTs were excluded for this reason (this number was previously only reported as a footnote to Table 1), along with the overall proportion of maternities for which smoking status was available (99%). We were not able to find information on quality variation between hospitals, but we have reported the range for PCTs.

(2) lines 47-49, pg 6: it is true that the adjusted figures allow the BF rates across the PCTs to be compared after taking into account the the different distributions but that is not to say that these adjusted figures "can be considered to provide a more accurate comparison...". An unadjusted comparison is a valid comparison for some purposes but as mentioned later in the document is not the best measure when comparing local performance. The text just needs to be amended slightly to reflect/clarify this.

Authors' response: We have amended the text to address this comment.

I am not a statistician so will be limited in the degree to which i can fully review the random effects model procedure - although i have carried out such analysis. With that caveat the model methods seems good to me.

By and large the results and conclusions reflect the findings - I have identified below a few areas that need clarification or expanding to make the messages more clear.....

PCT size: the range of sizes of the PCTs is quite large. Is this largely because of a few very large/small PCTs? i.e. would probably be more informative to have the interquartile range.

Authors' response: We have added the median and IQ range to provide a better estimate of the size of included PCTs. This descriptive information on PCTs has been moved to the methods section to address a suggestion from Reviewer 2 that further contextual information (relating to PCTs) is provided.

Would be useful to also have the estimate and CI for the % of young mothers (and other non-significant factors) in the adjusted models (e.g. Table 2, 3). You mention that these are non significant but seeing the estimate would allow the reader to see how far the estimate is from reaching significance. As Tables 2/3 are currently presented it is clear what is in and what is out of the adjusted model so might be better to put this extra information in the footnote or a supplementary table.

Authors' response: We have added the p values for non-significant factors in multivariable models (taken from the last model before the variable was removed) to the main text although if the Editor thinks that this information is not necessary then we would be happy to omit it. We are reluctant to add the estimates and CIs to the tables as these are taken from different models than the final adjusted models currently presented in Tables 2 and 3. However, we are happy to add these to the

text (alongside the p values) if the Editor thinks this would be useful.

Clarity: pg8 line 55 - misinterpreted this sentence first time round. Might need rewording.

Authors' response: We have amended this to clarify the interpretation.

Interpretation: given the differences seen in the london and non-london PCTs are quite sticking I think that the discussion of this needs to be expanded. There are issues which may be relevant:

(1) the issue of small sample in London is discussed in the limitations but probably also needs to be brought into the interpretation - for example, in discussing factors that failed to reach significance - do they represent a real effect or does it reflect a the small sample/underpowered is relevant?

(2) is the BME pop in london significantly different to that outside london, e.g. high prop of 1st generation BME in london versus outside london?

(3) in london is deprivation and BME too highly correlated to both exist in same model

(These are some of the issues that occurred to me when reading the interpretation of the london/non london differences - they don't all necessarily need to be included but i do think this issue needs to be expanded on in the interpretation)

Authors' response: The Reviewer is correct to emphasise the small sample size in the London analysis. We have amended the text to ensure the lack of power in the London analysis is covered as a possible explanation for our findings (section: limitations), and we have clarified in the results section the different sample sizes. We have also mentioned that differering characteristics of the BME population within and outside London are a possible explanation for the inconsistency in the observed association between BME and breastfeeding (section: Interpretation of results). BME was fitted as a continuous variable. We observed some correlation between deprivation and BME in the London analysis, but this correlation was not high and did not prevent the estimation of independent effects in the multivariable modelling.

Reviewer report 2

Reviewer: Jennifer Baker

Senior Research Associate

Institute of Preventive Medicine

Denmark

The authors should include a statement of ethics so it is clear under what authority the work was reviwed and permitted (or mention that ethical approval was not needed as this study was at the ecological level).

Authors' response: We have added a sentence at the beginning of the 'Methods' section to confirm ethical approval was not required (this was previously stated at the end of the text).

In this study, the authors investigated what factors could be underlying ecological differences in breastfeeding (BF) initiation and duration rates across England. The study used routinely collected data. The paper is well-written and comprehensive. Given that the data were routinely collected, it's important that the authors add to the paper ways in which they checked the data quality. It's not enough to rely upon the Department of Health assessment of quality when using the data for a research purpose; a description of this should be added to the manuscript. The paper is very relevant for the UK audience. By providing a some contextualization such as describing what a primary care trust (PCT) is in greater detail, and why the London/non-London divide is so important, it would become of greater relevance for the non-UK audience.

Authors' response:

We were unable to independently check the quality of the routine data used. However, we have provided an appropriate discussion of their limitations and have expanded our description of the DH data quality assurance process (Methods).

We have added a brief description of PCTs at the beginning of the Methods section, where PCTs are

first mentioned, and also moved the description of PCT size here from the Results section. We feel that the issue of the London/non-London divide is addressed in the third paragraph of the 'Statistical analysis' (page 6, lines 23-27 of the original submitted version).

Abstract

-Please state who the comparison group is for the women of a "Black and Minority Ethnic background" is in lines 23-25.

Authors' response: We have amended the text to clarify.

Strengths and limitations

It's subjective to state that the data are of "good quality" without further information to back this up. Please rephrase.

Authors' response: We have removed this phrase.

Introduction

Lines 36+: I was surprised to see that maternal overweight and obesity were not mentioned as being associated with BF initiation and duration as this has been shown in several countries. Please consider including this factor as well.

Authors' response: Our analysis was restricted to socio-demographic factors for which data were routinely available, although we also included smoking due to availability and evidence of association with breastfeeding. We did not consider looking at maternal obesity as there is no available routine data on this indicator. We have added a reference to the association between maternal obesity and breastfeeding in the literature section of the Introduction.

Methods

Line 6: Please contextualize a PCT more. From a non-UK perspective, it is not easy to understand what geographic area this encompasses, how many clinics it includes, whether it's an administrative designation or if it follows city limits, etc.

Authors' response: We have provided a brief description of PCTs at the beginning of the Methods section, and also moved the description of PCT size here from the Results section.

Line 28+: Did the authors investigate if there were any differences in PCTs with and without data that met the Department of Health quality standards?

Authors' response: Ten PCTs were excluded from analysis due to missing 6-8 week data, no PCTs had missing data on initiation. We did not investigate differences between missing and non-missing PCTs, but we are satisfied that the number of PCTs with missing data was small (7% for 6-8 week, 0% for initiation). The ten PCTs with missing data on breastfeeding at 6-8 weeks came from a variety of areas: three were in London, three were in South East Coast SHA, two in Eastern England SHA, one in West Midlands SHA and one in Yorkshire and the Humber SHA (these figures are available from the supplementary tables).

Results

P8, lines 4+: It would be useful if the directions of the associations were given.

Authors' reply: We have amended the text to indicate directions of observed associations.

Discussion

P9, Limitations: Although an exhaustive list of all variables unavailable for this study cannot be described here, it's surprising that maternal education was not listed; it seems as though it could explain more of what's going on rather than speculating about levels of service provision.

Authors' response: We agree that it would be useful to have information on levels of maternal education and have added a statement to this effect.

P10, line 9+: The DH method of dealing with missing data must be mentioned earlier in the methods section as it has great importance for the interpretation of the results.

Authors' response: We have added a statement to the methods section clarifying this approach to missing data.

Interpretation of results

Parts of this section are repetitive; please consider revising.

Authors' response: We have revised this section to minimise repetition.

Table 1: Please provide text for the abbreviations in the footnotes or spell out the abbreviations in the table themselves.

Authors' response: We have added lists of abbreviations as a footnote for all tables.

VERSION 2 – REVIEW

REVIEWER	Dr Deborah Shipton, Public Health Research Specialist Glasgow Centre for Population Health Scotland I have no competing interests in reviewing this manuscript
REVIEW RETURNED	26-Apr-2013
RESULTS & CONCLUSIONS	minor piont: Pg 9 line 40 “maternal smoking associated with increased exclusive breastfeeding”. The OR is less than one so an increase in the % who smoke is negatively associated exclusive BF.
GENERAL COMMENTS	I have only one minor revision which would not need to be re-reviewed.