

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	CHANGES IN RATES OF EARLY EXCLUSIVE BREASTFEEDING IN SOUTH AFRICA FROM 2010 TO 2013: DATA FROM THREE NATIONAL SURVEYS BEFORE AND DURING IMPLEMENTATION OF A CHANGE IN NATIONAL BREASTFEEDING POLICY
AUTHORS	Jackson, Debra; Swanevelder, Sonja; Doherty, Tanya; Lombard, Carl; Bhardwaj, Sanjana; Goga, Ameena

VERSION 1 – REVIEW

REVIEWER	Kingsley Agho Western Sydney University, Australia
REVIEW RETURNED	08-Jan-2019

GENERAL COMMENTS	<p>Title: the title of the paper did not reflect what the authors did. The paper did not examine the impact of policy rather changes in EBF between pre and post policy.</p> <p>What is the actual name of the national policy?</p> <p>Suggested title: changes in rates and factors associated with EBF among infant aged 4-8 weeks, 2010-2012-13</p> <p>Suggested title: changes in rates and factors associated with EBF among infant aged 4-8 weeks: pre and post (what policy ?)</p> <p>Abstract:</p> <p>Objective – this section should only focus on the objective. Lines 8-14 are not necessary</p> <p>The aim of the paper should reflect the title of the paper.</p> <p>Setting:</p> <p>Indicate sample size for each year of survey. On sampling procedure, the information on the abstract section is different from those reported in the method section of this paper – be consistent.</p> <p>Primary outcome</p> <p>What did the authors mean by infant feeding? Infant feeding included breastfeeding and complementary feeding –rather, my understanding was, the outcome is EBF as define by WHO 2008 IYCF guidelines (?).</p> <p>Results:</p> <p>Line 39-40 is not clear. What is prevalence doing here?</p> <p>The authors should re-write the result section and ONLY focus on results and nothing else but results.</p> <p>Conclusion:</p> <p>The authors should re-write the conclusion section and should focus on the conclusion based on their finding and the policy implications of those findings for future intervention(s).</p> <p>Introduction:</p> <p>Lines 5-9: the claim is not true – in Nigeria, the rate of EBF was 17% and there are other Africa countries whose rate of EBF is lower than South Africa.</p>
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	<p>Line 24: there are differences between breastfeeding and EBF – what are the benefit of EBF to child development? Not stated. Majority of the introduction is centred on HIV but that is not the motive of this paper.</p> <p>The aim of this paper is different from the title and the authors should be consistent and what are the policy implication of these findings? Over recommendation: the authors should re-write the whole introduction to reflect the real situation around EBF in Africa and South Africa</p> <p>Methods:</p> <p>Line 33-3: The authors indicated that “the sampling frame and selected facilities were identical between 2011-12 and 2012-13 except for four clinic.....”. Based on this limitation, did the authors recalculate the sampling weight (or normalised the sampling weight) for the three datasets? If not why?</p> <p>Line 52: the desired sample size was 12,200 infants aged 4-8 weeks and infant < 4 and >8 were excluded. This is a major limitation and it is no longer exclusive breastfeeding (EBF) because the definition of EBF. According to WHO, EBF is define as: The exclusive breastfeeding rate is the proportion of infants less than 6 months of age who were exclusively breastfed in the last 24 hours, i.e., the infant had received only breast milk from his/her mother or a wet nurse, or expressed breast milk, and no other liquids or solids with the exception of drops or syrups consisting of vitamins, mineral supplements or medicines.</p> <p>Justify why the authors excluded infant < 4 and >8 from their study?</p> <p>Line 29: the authors used the old WHO guidelines definition (my understanding is that mixed feeding and exclusive formula feeding are no longer in the new guidelines) – see link to the latest guideline: https://www.who.int/nutrition/publications/infantfeeding/9789241599290/en/</p> <p>Line 31: six-weeks postpartum is not correct if the authors examined infants aged 4-8 weeks</p> <p>Statistical section is not consistent. As stated earlier, The authors should re-analyse the datasets again because “the sampling frame and selected facilities were identical .. except for four clinic.....”.</p> <p>Discussion:</p> <p>I don't think this paper is about changes in national policy. If the authors are interested in doing changes in national policy paper, they should create a new dummy variable called "policychange" policychange is 0 for old policy and 1 for the new policy (choose 2012-13) and ran survey logistic regression model and then determine the slope (changes) between the old and new policy in relation to EBF among infant aged 4-8 weeks but that is not what the authors did and hence the discussion around change in policy should be removed or re-written.</p> <p>Limitation should include some of the limitations I pointed out earlier. The section on Public Health implications should be rewritten and should focus on how their findings is going to help shape future interventions to improve EBF in South Africa. I don't understand the motive for “mean = six week....”</p> <p>Tables:</p> <p>Table 1 indicate year of DHS</p> <p>Table 2: add changes in prevalence's in percentage between 2010-2011-12 and 2010-2012-13 and their rate and P-values</p> <p>The author should produce a table showing the frequency and number by each year of survey of all the potential variables examined.</p> <p>Table 3 – did the author conducted a multinomial logistic model ? this table is confusing? How did authors categorise the variable and</p>
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	<p>which one is the referent category? . for example infant age, it is continuous or categorical? Not clear?. Mother education, which one is the referent category? And so on...</p> <p>Table 4: the authors indicated “the sampling frame and selected facilities were identical between 2011-12 and 2012-13 except for four clinic.....” Based on this statement, I think pooling the surveys might lead to statistical bias and conducting a multinomial model may be the best approach, the authors should consider. I would like the authors to format this table properly and should also report unadjusted odd ratios</p> <p>Finally, I would like to see graph of EBF rates by 4,5,6,7 and 8 weeks by the three surveys and just reporting the overall rates may also lead to confusing sometimes.</p>
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REVIEWER	Jesse Anttila-Hughes University of San Francisco Dept. of Economics USA
REVIEW RETURNED	19-Feb-2019

GENERAL COMMENTS	<p>This paper uses breastfeeding data from three national samples of South African 4-8-week old children to document a large increase in exclusive breast feeding coinciding with a major shift in SA national breastfeeding policy, the 2011 Tshwane Declaration. I believe that the paper is well written and the results significant and hence suitable for this journal, but have some concerns as follows:</p> <ul style="list-style-type: none"> • The main contribution of the paper is the novelty of the SAPMTCTE data, and so my primary concern is the mismatch between those estimates and other similar estimates from both the sample window 2010-2013, as well as since then; see eg the du Plessis et al. SAHR 2016 overview here: http://pmhp.za.org/wp-content/uploads/SAHR2016_chapter10_Breastfeeding.pdf . The authors touch on this in the discussion, but I think they need to address how unusually high their estimates are and be a bit more careful about couching their findings as unusual in both the discussion and in the conclusion within the abstract. The authors note that in some cases (eg the SANHANES) this seems to be driven by choice of 24 hour vs. longer recall methods. This is a meaningful difference with an established discussion in the literature, see e.g., Aarts et al. 2000 in Int J. Epidemiology or Fenta et al. 2017 in Int Breastfeed J., and the authors should (a) address the issues related to different measurements, their interpretation, and the functional definition of EBF and (b) present 7 day results to see if those better match contemporaneous estimates. They should also (c) at least address the possibility that these unusually high rates may be driven by survey design effects. It may very well be the case that some form of priming, respondent desire to please interviewer, or other behavioral design effect may have inflated estimates here (notably counseling, see below). The countervailing explanation would be that these data captured an increase where other surveys didn't, which would merit discussion in its own right. • It is worth noting that more recent estimates are also lower, e.g., the 2016 SA DHS 0-1 month rate of 44%. This implies that the Tshwane Declaration may have been locally effective but since elapsed. It's worth noting that there's no particular reason as far as I can see for the survey to not at least be internally comparable / for any possible design effects to intensify; this suggests that the Tshwane Declaration and associated shifts in policy do seem likely to have had an effect, subject to caveats over causal inference, but progress may have backslid in the interim. The authors should
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	<p>discuss these more recent estimates and note that that they imply either EBF rates have recently fallen or that they have remained the same and the SAPMTCTE data are simply estimating a much higher proportion.</p> <ul style="list-style-type: none"> • Unless I am mistaken, the national total percentage point breakdowns by feeding category don't add up, and imply that there is a fourth category of feeding that's not being listed in Text Box 1. Specifically, there is a 12.1 percentage point total reduction in MBF (24.7% to 16. 5%) and EFF (19.0% to 15.1%) but a net EBF gain of 36.2 percentage points. The authors should either explain and/or rectify the discrepancy, which seems large and meaningful. • The authors should provide more detail on the counseling variable. The results show a strong positive effect of breastfeeding counseling on the odds of reporting EBF; if the counseling session occurred at the same time / place as the EBF is being reported, which seems to be the case, then that is strong evidence that the survey here is suffering from a design effect, since there's no possible way for the counseling to change the previous 24 hours of feeding history. This may explain why these estimates are so high, and would put overall outcomes at the OR of non-counseled, much more closely in line with the rates we see in the 2016 SADHS. • It is not entirely clear what the authors mean on p9 by "ecologic", though I presume that designates a single observational time series / history where specific causal effects cannot be identified. It does seem likely that these data partially show the effect of the Tshawne Declaration, but the authors should probably be a bit more explicit in noting that many other contemporaneous factors are acting simultaneously and can't causally attribute specific things to specific aspects of it since there are so many changes. • On p9 para 2 and p10 top paragraph MBFHI is misspelled, and is perhaps a mistaken convolution of the BFHI with the local SA name "Mother-Baby Friendly Initiative (MBFI)"
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REVIEWER	Sara Jewett Nieuwoudt School of Public Health, University of the Witwatersrand, Johannesburg, South Africa
REVIEW RETURNED	15-Apr-2019

GENERAL COMMENTS	<p>This paper provides an excellent review of quantitative data trends in early EBF in the context of a national policy shift. The following suggestions relate to minor improvements that may contribute to overall clarity and context, particularly for non-SA readers.</p> <p>Title: As the analysis only looks at data for early EBF, consider adding the word "early" to the title.</p> <p>Abstract: Ensure all significant results are included, e.g. parity, planned pregnancy and older infant age.</p> <p>Background: Some statements are not substantiated by references. For instance, sentences 2 and 3 on the second paragraph of page 4 require citations. Please review this section carefully to ensure all statements about the literature are cited.</p> <p>Methods:</p> <ul style="list-style-type: none"> -When presenting variables, clarify which researchers refer to in making claims about SES in the abstract -p6, line 15: World Health Organization (z, as proper name) -p6, lines 45-48: Which method within STATA SE is being referred to? Consider breaking up this sentence so that you name the
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	<p>software and separately describe how the functions were used to account for SE.</p> <p>Results: -In31, p7.: Is there a reason the second survey period was excluded from this sub-analysis? As this paragraph isn't directly linked to the study question about EBF, a sentence or two more about why this analysis was included would strengthen this paragraph. -Table 3 results are not sufficiently discussed. Add 1-2 more sentences to address covariates and time periods before moving to Table 4. -Parity and planning of pregnancy are missing in results narrative for Table 4 (top of page 8)</p> <p>Discussion -Please reference changes in the policies and programmes (section beginning at the end of page 8 and second paragraph of p.9 though page 10. Even if you reference a few media reports, for instance, it would improve upon the current lack of references. -While some inconsistent findings are well discussed, some results are not discussed, such as parity and unplanned pregnancy. I suggest adding these.</p> <p>Limitations -Consider adding that these surveys only measure early EBF, with literature highlighting high drop-offs in latter periods of the first 6 months. [NB: This doesn't take away from the trend analysis, but factors that support early EBF and later EBF are likely to differ]</p> <p>Table 1: Recommend presenting all rates to 1 decimal place Table 3 Clarify in co-variates what is being measured for clearer interpretation of the odds ratios. For example, rather than HIV-Status, specify which status. For mother employment, specify if you were tracking unemployed or employed to better interpret the ORs.</p>
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REVIEWER	Dr Helen Muloi University of KwaZulu-Natal South Africa
REVIEW RETURNED	20-Apr-2019

GENERAL COMMENTS	<p>Comments to the Authors:</p> <p>General: Overall the article has been well written and researched.</p> <p>Specific: Please see comments, corrections below.</p> <p>Cover Page</p> <p>Keywords: Please change "Breastfeeding" to "Exclusive Breastfeeding" which is more appropriate for this article.</p> <p>Abstract (page 2):</p> <p>Line 10: Please see comment below for Introduction (page 4, line 8) regarding 4% reported exclusive breastfeeding rate.</p> <p>Lines 29-31: Repetition of "enrolled" a bit clumsy. I would suggest "The number of caregiver-infant pairs enrolled were 10,182, 10,106 and 9,120 in 2010, 2011-12, and 2012-13, respectively.</p> <p>Article Summary (page 3):</p>
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	<p>Lines 14-16: Repetition of the word “presenting”, suggest deleting the latter.</p> <p>Line 19: Include “a” before “private hospital”</p> <p>Introduction (page 4): Line 5: Write “exclusive breastfeeding” with lower case</p> <p>Line 8: You mention 4% but this figure does not appear in Table 1. Where does this figure 4% come from? (Same comment for Abstract page 2, line 10)</p> <p>Line 36: Why is exclusive written as ‘exclusive’? I think you can either mention breastfeeding and in particular EBF or just write EBF here...</p> <p>Methods (page 6): Line 3: “spot” should be plural (“spots”) for this sentence to read well.</p> <p>Line 10: Replace “mother” with “mother’s”.</p> <p>Line 48: You need to explain why 23 strata were used. It would make sense to have strata for each province since they have different populations and therefore weightings would be different but why 23?</p> <p>Did each of the 23 strata have 1 or more primary sampling units? Please elaborate a bit more on this.</p> <p>Methods (page 7): Line 5: insert “which” after “model” otherwise sentence doesn’t make sense.</p> <p>Line 9: Change to: significant at “the 5% level, which resulted in a” final regression.....”</p> <p>Line 14: Where are these p-values?</p> <p>Results (page 7): Line 26: Please be consistent with number of decimal places, here you say $p < 0.001$, in Table 2 you say $p < 0.0001$.</p> <p>Line 28: I think your Table 2 should show the p-values for all the Provinces, which will demonstrate this statement.</p> <p>Lines 31-34: Are these figures referring to the national rates? If so please specify this.</p> <p>Please check number of decimal places for p-value as per comment above (line 26). Also I am unsure about these figures, shouldn’t $EBF + EFF + MBF = 100\%$? For both year time periods you mention here for non-EBF categories the numbers do not add up to 100%. Please clarify if you are referring to the same time period of 4-8 weeks of age as you do with national EBF rates in lines 22-24.</p> <p>Discussion (page 8): Line 21: Ref 17 Please change from a magazine article to the journal article reference for this study. It should be noted that this study had</p>
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	<p>a strong emphasis on EBF counseling which could account for higher EBF rates.</p> <p>Line 34: Suggest entering the year here (as you did in line 26).</p> <p>Discussion (page 9): Line 5: Is “ecologic” the correct word here?</p> <p>Line 38: Change “free-formula” to “free formula”</p> <p>Line 43: Change “MFFHI” to “MBFHI” Discussion (page 10): Line 24: Insert “an” between “showed” and “even”</p> <p>Line 26: Insert “the” between “post” and “Tshwane”</p> <p>Line 36: Replace “breastfeeding” with “EBF” Discussion (page 11):</p> <p>Line 3: Insert “odds of” between lower and EBF.</p> <p>Line 5: Replace “breastfeeding” with “EBF”</p> <p>Public Health Implications (page 11): Line 36: Insert “a” between “with” and “major”</p> <p>Author’s Contributions (page 13):</p> <p>Line 19: Change “AE” to “AG”</p> <p>References (page 14): Line 31: Reference 5. Remove website address http://....., unnecessary as article is fully cited without this.</p> <p>References (page 15):</p> <p>Line 14: Reference 10. Same comment as for Reference 5.</p> <p>Line 38: Reference 14. Remove weblink & change to journal article reference.</p> <p>References (page 16): Line 22: Reference 21. I think this should read 2015;104,114-35 as per your other references?</p> <p>Line 41: Reference 25. You can remove “DOI.....” as unnecessary for citation.</p> <p>Text Box 1 (page 17): Line 7: I think it reads better if you put “not even water” in brackets rather than between commas.</p> <p>Table 1 (page 18):</p> <p>I think all your numbers should have the same number of decimal places eg you can change the 1998 figure of 7 to 6.8, the 2007 figure of 6 to 6.2. Please also change the Good Start 1 figures in 2003 to one decimal place.</p> <p>It would be nice to see the sample numbers for each of these</p>
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	<p>surveys / studies.</p> <p>Table 2 (page 19): I am not sure why you don't also include p-values for the trends for the year for each province too.</p> <p>Table 3 (page 20): I think it would look better to group these together as you do in the text eg all the mother's variables together etc. Line 15: Insert "Maternal" before HIV Status.</p> <p>Table 4 (page 21): You need to explain why some figures are bold and others aren't.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Kingsley Agho

Institution and Country: Western Sydney University, Australia

Title: the title of the paper did not reflect what the authors did. The paper did not examine the impact of policy rather changes in EBF between pre and post policy.

What is the actual name of the national policy?

Suggested title: changes in rates and factors associated with EBF among infant aged 4-8 weeks, 2010-2012-13

Suggested title: changes in rates and factors associated with EBF among infant aged 4-8 weeks: pre and post (what policy ?)

We have revised title considering this and also comment from Reviewer 2. We do not include the 'name' of the policy (Tshwane Declaration of Support for Breastfeeding in South Africa, 2011) as the audience is global so the locally used name is not relevant for this audience, what is relevant is that it is a change to national breastfeeding policy.

Abstract:

Objective – this section should only focus on the objective. Lines 8-14 are not necessary

The aim of the paper should reflect the title of the paper.

Objective and title now more closely aligned. Respectfully disagree that the inclusion of the explanation of the policy in the abstract is not necessary as it is the core change being analyzed..

Setting:

Indicate sample size for each year of survey.

Sample size is already indicated in the next paragraph under participants

On sampling procedure, the information on the abstract section is different from those reported in the method section of this paper – be consistent.

Changed to 'stratified multi-stage' as used in methods section

Primary outcome

What did the authors mean by infant feeding? Infant feeding included breastfeeding and complementary feeding –rather, my understanding was, the outcome is EBF as define by WHO 2008 IYCF guidelines (?).

Corrected to only EBF

Results:

Line 39-40 is not clear. What is prevalence doing here?

The numbers cited are clearly defined as odds ratios which is the effect measure for the change in EBF prevalence.

The authors should re-write the result section and ONLY focus on results and nothing else but results.

Sentence moved to conclusion

Conclusion:

The authors should re-write the conclusion section and should focus on the conclusion based on their finding and the policy implications of those findings for future intervention(s).

Now states further improvements to EBF programmes are needed

Introduction:

Lines 5-9: the claim is not true – in Nigeria, the rate of EBF was 17% and there are other Africa countries whose rate of EBF is lower than South Africa.

The statement says 'one of the lowest' not lowest.

Line 24: there are differences between breastfeeding and EBF – what are the benefit of EBF to child development? Not stated.

The benefits of EBF are well established in the literature, word limits do not allow addition of this discussion to the paper, and they are not the focus of the paper.

Majority of the introduction is centred on HIV but that is not the motive of this paper.

The aim of this paper is different from the title and the authors should be consistent and what are the policy implication of these findings?

This paragraph is pointing out that HIV is one of many contributors to low EBF in South Africa, as stated in the paragraph other contributors include mixed-messaging from health providers, mixed feeding, urbanization, stigma, and mothers returning to work.

Over recommendation: the authors should re-write the whole introduction to reflect the real situation around EBF in Africa and South Africa

Respectfully, we do not understand what the reviewer means by 'real situation'.. We have provided a description of the history and current context relating to EBF in South Africa.

Methods:

Line 33-3: The authors indicated that “the sampling frame and selected facilities were identical between 2011-12 and 2012-13 except for four clinic.....”. Based on this limitation, did the authors recalculate the sampling weight (or normalised the sampling weight) for the three datasets?

If not why?

The sampling weight was recalculated for the later surveys taking the sample realization into account. The new clinics were used since they were now the clinics providing care to the underlying study population at the time of the survey.

Line 52: the desired sample size was 12,200 infants aged 4-8 weeks and infant < 4 and >8 were excluded. This is a major limitation and it is no longer exclusive breastfeeding (EBF) because the definition of EBF.

According to WHO, EBF is defined as:

The exclusive breastfeeding rate is the proportion of infants less than 6 months of age who were exclusively breastfed in the last 24 hours, i.e., the infant had received only breast milk from his/her mother or a wet nurse, or expressed breast milk, and no other liquids or solids with the exception of drops or syrups consisting of vitamins, mineral supplements or medicines.

Justify why the authors excluded infant < 4 and >8 from their study?

The WHO definition indicates less than 6 months, however EBF is often measured cross-sectionally at the time of visit to a health facility, or a population-based survey at any age between birth and 6 months. Longitudinal daily data of actual infant feeding is rare. We chose the 6 week vaccination visit as the contact point representative of early EBF. As infants rarely visit the clinic on exactly 6 weeks from their date of birth we allowed a window of 4-8 weeks.

Line 29: the authors used the old WHO guidelines definition (my understanding is that mixed feeding and exclusive formula feeding are no longer in the new guidelines) – see link to the latest guideline: <https://www.who.int/nutrition/publications/infantfeeding/9789241599290/en/>

Corrected to only EBF

Line 31: six-weeks postpartum is not correct if the authors examined infants aged 4-8 weeks
Statistical section is not consistent.

All descriptions changed to 4-8 weeks

As stated earlier, The authors should re-analyse the datasets again because “the sampling frame and selected facilities were identical .. except for four clinic.....”.

See response above. We weighted for sample realization. Having the same sites was not critical as this was not a before-after design. It was multiple cross-sectional surveys

Discussion:

I don't think this paper is about changes in national policy. If the authors are interested in doing changes in national policy paper, they should create a new dummy variable called "policychange" policychange is 0 for old policy and 1 for the new policy (choose 2012-13) and ran survey logistic regression model and then determine the slope (changes) between the old and new policy in relation to EBF among infant aged 4-8 weeks but that is not what the authors did and hence the discussion around change in policy should be removed or re-written.

As indicated this is an ecologic analysis of the years around the policy change – the specific lack of causal inference is now clearly stated in that sentence. Because this was national policy our assumption is that 2012-13 = new policy and thus year is a correlate for new versus old policy.

Limitation should include some of the limitations I pointed out earlier.

Additional limitations have been added

The section on Public Health implications should be rewritten and should focus on how their findings is going to help shape future interventions to improve EBF in South Africa. I don't understand the motive for “mean = six week....”

Deleted

Tables:

Table 1 indicate year of DHS

Already indicated in column headings in the table

Table 2: add changes in prevalence's in percentage between 2010-2011-12 and 2010-2012-13 and their rate and P-values

Respectfully we feel adding the changes in prevalence percentage will clutter the table, also the 95% confidence intervals do not overlap except in one instance, so adding p-values are unnecessary as the reader can use the 95%CI.

The author should produce a table showing the frequency and number by each year of survey of all the potential variables examined.

This would be a massive table so it was not included. The paper by Goga et.al. reference 13 Table 1 gives an example of what such a table would look like for just 1 of the 3 years. We defer to the editor regarding the request for an additional table. We did however add sample size for each year to Table 2 and Total Observations added for Tables 3 and 4.

Table 3 – did the author conducted a multinomial logistic model ? this table is confusing? How did authors categorise the variable and which one is the referent category? . for example infant age, it is continuous or categorical? Not clear?. Mother education, which one is the referent category? And so on...

Additional clarity added to text, and a substantial footnote added to table

Table 4: the authors indicated “

The sampling frame and selected facilities were identical between 2011-12 and 2012-13 except for four clinic.....” Based on this statement, I think pooling the surveys might lead to statistical bias and conducting a multinomial model may be the best approach, the authors should consider. I would like the authors to format this table properly and should also report unadjusted odd ratios

Our analysis is as suggested by the reviewer. We have a binary indicator for 12/13 versus 2010 and this allows us to estimate an effect measure – the odds ratio of women EBF at 6 weeks in 12/13 compared to 2010 adjusted for the other variables in the model. We took account for the survey design in the analysis.

Finally, I would like to see graph of EBF rates by 4,5,6,7 and 8 weeks by the three surveys and just reporting the overall rates may also lead to confusing sometimes.

Added along with text in results and discussion

Reviewer: 2

Reviewer Name: Jesse Anttila-Hughes

Institution and Country: University of San Francisco - Dept. of Economics

USA

This paper uses breastfeeding data from three national samples of South African 4-8-week old children to document a large increase in exclusive breast feeding coinciding with a major shift in SA national breastfeeding policy, the 2011 Tshwane Declaration. I believe that the paper is well written and the results significant and hence suitable for this journal,

Thank you!

but have some concerns as follows:

- The main contribution of the paper is the novelty of the SAPMTCTE data, and so my primary concern is the mismatch between those estimates and other similar estimates from both the sample window 2010-2013, as well as since then; see eg the du Plessis et al. SAHR 2016 overview here: http://pmhp.za.org/wp-content/uploads/SAHR2016_chapter10_Breastfeeding.pdf . The authors touch on this in the discussion, but I think they need to address how unusually high their estimates are and be a bit more careful about couching their findings as unusual in both the discussion and in the conclusion within the abstract. The authors note that in some cases (eg the SANHANES) this seems to be driven by choice of 24 hour vs. longer recall methods. This is a meaningful difference with an established discussion in the literature, see e.g., Aarts et al. 2000 in Int J. Epidemiology or Fenta et al. 2017 in Int Breastfeed J., and the authors should

(a) address the issues related to different measurements, their interpretation, and the functional definition of EBF and

Corrected we used 8 day recall as described in methods but mis-stated in discussion. Our EBF calculation stops at 6 weeks whereas SADHS look at infants under the age of 6 months; Thus our EBF estimates are going to be higher. This has been added to text.

(b) present 7 day results to see if those better match contemporaneous estimates.

Majority of the comparisons were to studies using 24 hour recall, the SAHANES used 'current' and 'since birth' we do not have any comparable data to this definition. We used 8 day recall which is more robust than 24 hour recall. This has been clarified in the text.

They should also (c) at least address the possibility that these unusually high rates may be driven by survey design effects. It may very well be the case that some form of priming, respondent desire to please interviewer, or other behavioral design effect may have inflated estimates here (notably counseling, see below). The countervailing explanation would be that these data captured an increase where other surveys didn't, which would merit discussion in its own right.

Potential impact of household vs facility sampling added in discussion. The majority of studies measuring EBF use recall – so all studies are subject to recall bias – we sought to compare feeding patterns over years, using the same methodology.

- It is worth noting that more recent estimates are also lower, e.g., the 2016 SA DHS 0-1 month rate of 44%. This implies that the Tshwane Declaration may have been locally effective but since elapsed. It's worth noting that there's no particular reason as far as I can see for the survey to not at least be internally comparable / for any possible design effects to intensify; this suggests that the Tshwane Declaration and associated shifts in policy do seem likely to have had an effect, subject to caveats over causal inference, but progress may have backslid in the interim. The authors should discuss

these more recent estimates and note that that they imply either EBF rates have recently fallen or that they have remained the same and the SAPMTCTE data are simply estimating a much higher proportion.

Added in discussion, however it should also be noted that In the age group 0-1 month the number of children included was 115 – see page 119 of the detailed SADHS report. With this small sample inferences made from the DHS data on early EBF need to be cautious – this has been added to text.

- Unless I am mistaken, the national total percentage point breakdowns by feeding category don't add up, and imply that there is a fourth category of feeding that's not being listed in Text Box 1. Specifically, there is a 12.1 percentage point total reduction in MBF (24.7% to 16. 5%) and EFF (19.0% to 15.1%) but a net EBF gain of 36.2 percentage points. The authors should either explain and/or rectify the discrepancy, which seems large and meaningful.

This paragraph has been deleted

- The authors should provide more detail on the counseling variable. The results show a strong positive effect of breastfeeding counseling on the odds of reporting EBF; if the counseling session occurred at the same time / place as the EBF is being reported, which seems to be the case, then that is strong evidence that the survey here is suffering from a design effect, since there's no possible way for the counseling to change the previous 24 hours of feeding history. This may explain why these estimates are so high, and would put overall outcomes at the OR of non-counseled, much more closely in line with the rates we see in the 2016 SADHS.

Added specific question to text. This variable comes from the answer to the question worded: "During pregnancy did you ever discuss with anyone at the clinic what the best way for you to feed your baby" which means that any counseling session was at least 4-8 weeks or more prior to the interview. Also in many cases in South Africa the antenatal clinic and child clinics are not the same, as the former are centralized into Midwifery Units and the latter are held in primary health care centers. Therefore we do not see a possible proximal influence for this analysis. We have added the qualifier to all mention of counseling that it was antenatal counseling.

- It is not entirely clear what the authors mean on p9 by "ecologic", though I presume that designates a single observational time series / history where specific causal effects cannot be identified. It does seem likely that these data partially show the effect of the Tshawne Declaration, but the authors should probably be a bit more explicit in noting that many other contemporaneous factors are acting simultaneously and can't causally attribute specific things to specific aspects of it since there are so many changes.

Lack of causal inference due to ecologic design is now added in discussion and limitations.

- On p9 para 2 and p10 top paragraph MBFHI is misspelled, and is perhaps a mistaken convolution of the BFHI with the local SA name "Mother-Baby Friendly Initiative (MBFI)"

Corrected

Reviewer: 3

Reviewer Name: Sara Jewett Nieuwoudt

Institution and Country: School of Public Health, University of the Witwatersrand, Johannesburg, South Africa

This paper provides an excellent review of quantitative data trends in early EBF in the context of a national policy shift. The following suggestions relate to minor improvements that may contribute to overall clarity and context, particularly for non-SA readers.

Thank you!

Title: As the analysis only looks at data for early EBF, consider adding the word "early" to the title.

Added

Abstract: Ensure all significant results are included, e.g. parity, planned pregnancy and older infant age.

Added

Background: Some statements are not substantiated by references. For instance, sentences 2 and 3 on the second paragraph of page 4 require citations. Please review this section carefully to ensure all statements about the literature are cited.

Added

Methods:

-When presenting variables, clarify which researchers refer to in making claims about SES in the abstract

This had been clarified in results section and qualified in abstract

-p6, line 15: World Health Organization (z, as proper name)

Reference deleted.

-p6, lines 45-48: Which method within STATA SE is being referred to? Consider breaking up this sentence so that you name the software and separately describe how the functions were used to account for SE.

We used survey statistics in STATA SE (for big data sets with excessive variables), v15 as simple random sampling was not employed for this project. This linearized survey method which was used for the univariate and multiple logistic regressions ensured that the standard errors of the estimates were calculated correctly, accounting for 23 strata (one province only had 2 strata due to no large clinics below national average), weighting, and identifying the primary sampling units.

Results:

-In31, p7.: Is there a reason the second survey period was excluded from this sub-analysis? As this paragraph isn't directly linked to the study question about EBF, a sentence or two more about why this analysis was included would strengthen this paragraph.

Paragraph deleted

-Table 3 results are not sufficiently discussed. Add 1-2 more sentences to address covariates and time periods before moving to Table 4.

Additional detail added.

-Parity and planning of pregnancy are missing in results narrative for Table 4 (top of page 8)

All variables now included in text.

Discussion

-Please reference changes in the policies and programmes (section beginning at the end of page 8 and second paragraph of p.9 through page 10. Even if you reference a few media reports, for instance, it would improve upon the current lack of references.

Added

-While some inconsistent findings are well discussed, some results are not discussed, such as parity and unplanned pregnancy. I suggest adding these.

Added

Limitations

-Consider adding that these surveys only measure early EBF, with literature highlighting high drop-offs in latter periods of the first 6 months. [NB: This doesn't take away from the trend analysis, but factors that support early EBF and later EBF are likely to differ]

Added

Table 1: Recommend presenting all rates to 1 decimal place

Added

Table 3 Clarify in co-variates what is being measured for clearer interpretation of the odds ratios. For example, rather than HIV-Status, specify which status. For mother employment, specify if you were tracking unemployed or employed to better interpret the ORs.

Clarified Maternal HIV status, categories of employment and other variables are found in Table 4. Substantial note to reading the table has been added.

Reviewer: 4

Reviewer Name: Dr Helen Mulol

Institution and Country: University of KwaZulu-Natal - South Africa

General: Overall the article has been well written and researched.

Thank you!

Specific: Please see comments, corrections below.

Cover Page

Keywords: Please change "Breastfeeding" to "Exclusive Breastfeeding" which is more appropriate for this article.

Done

Abstract (page 2):

Line 10: Please see comment below for Introduction (page 4, line 8) regarding 4% reported exclusive breastfeeding rate.

Corrected

Lines 29-31: Repetition of "enrolled" a bit clumsy. I would suggest "The number of caregiver-infant pairs enrolled were 10,182, 10,106 and 9,120 in 2010, 2011-12, and 2012-13, respectively.

Done

Article Summary (page 3):

Lines 14-16: Repetition of the word "presenting", suggest deleting the latter.

Done

Line 19: Include "a" before "private hospital"

Done

Introduction (page 4):

Line 5: Write "exclusive breastfeeding" with lower case

Done

Line 8: You mention 4% but this figure does not appear in Table 1. Where does this figure 4% come from? (Same comment for Abstract page 2, line 10)

Corrected

Line 36: Why is exclusive written as 'exclusive'? I think you can either mention breastfeeding and in particular EBF or just write EBF here...

Done

Methods (page 6):

Line 3: "spot" should be plural ("spots") for this sentence to read well.

Done

Line 10: Replace "mother" with "mother's".

Done

Line 48: You need to explain why 23 strata were used. It would make sense to have strata for each province since they have different populations and therefore weightings would be different but why 23?

Revised for clarity, specific number of strata deleted

Did each of the 23 strata have 1 or more primary sampling units?
Please elaborate a bit more on this.

Revised for clarity

Methods (page 7):

Line 5: insert "which" after "model" otherwise sentence doesn't make sense.

Done

Line 9: Change to: significant at "the 5% level, which resulted in a" final regression....."

Done

Line 14: Where are these p-values?

Corrected

Results (page 7):

Line 26: Please be consistent with number of decimal places, here you say $p < 0.001$, in Table 2 you say $p < 0.0001$.

Corrected

Line 28: I think your Table 2 should show the p-values for all the Provinces, which will demonstrate this statement.

Non-overlapping 95%CI tell the same story that all trends are significant but with added information of precision of estimate. P-value footnote includes provincial adjusted estimates for overall weighted average rate.

Lines 31-34: Are these figures referring to the national rates? If so please specify this.

Added

Please check number of decimal places for p-value as per comment above (line 26).

Done

Also I am unsure about these figures, shouldn't $EBF + EFF + MBF = 100\%$? For both year time periods you mention here for non-EBF categories the numbers do not add up to 100%. Please clarify if you are referring to the same time period of 4-8 weeks of age as you do with national EBF rates in lines 22-24.

Paragraph deleted

Discussion (page 8):

Line 21: Ref 17 Please change from a magazine article to the journal article reference for this study.

Done

It should be noted that this study had a strong emphasis on EBF counseling which could account for higher EBF rates.

Added

Line 34: Suggest entering the year here (as you did in line 26).

Done

Discussion (page 9):

Line 5: Is "ecologic" the correct word here?

Yes as it refers to policy change so comparison is essentially ecologic

Line 38: Change "free-formula" to "free formula"

Done

Line 43: Change "MFFHI" to "MBFHI"

Corrected

Discussion (page 10):

Line 24: Insert "an" between "showed" and "even"

Done

Line 26: Insert "the" between "post" and "Tshwane"

Done

Line 36: Replace "breastfeeding" with "EBF"

Done

Discussion (page 11):

Line 3: Insert "odds of" between lower and EBF.

Done

Line 5: Replace "breastfeeding" with "EBF"

Done

Public Health Implications (page 11):

Line 36: Insert "a" between "with" and "major"

Done

Author's Contributions (page 13):

Line 19: Change "AE" to "AG"

Corrected

References (page 14):

Line 31: Reference 5. Remove website address <http://.....>, unnecessary as article is fully cited without this.

Done

References (page 15):

Line 14: Reference 10. Same comment as for Reference 5.

Done

Line 38: Reference 14. Remove weblink & change to journal article reference.

Done

References (page 16):

Line 22: Reference 21. I think this should read 2015;104,114-35 as per your other references?

Corrected

Line 41: Reference 25. You can remove "DOI....." as unnecessary for citation.

Done

Text Box 1 (page 17):

Line 7: I think it reads better if you put "not even water" in brackets rather than between commas.

Text Box Deleted

Table 1 (page 18):

I think all your numbers should have the same number of decimal places eg you can change the 1998 figure of 7 to 6.8, the 2007 figure of 6 to 6.2.

Done

Please also change the Good Start 1 figures in 2003 to one decimal place.

It would be nice to see the sample numbers for each of these surveys / studies.

More detail added, data reordered so now sequential by year and added additional Good Start 1 data.

Table 2 (page 19): I am not sure why you don't also include p-values for the trends for the year for each province too.

Discussed above

Table 3 (page 20): I think it would look better to group these together as you do in the text eg all the mother's variables together etc.

Done

Line 15: Insert "Maternal" before HIV Status.

Done

Table 4 (page 21): You need to explain why some figures are bold and others aren't.

Added

VERSION 2 – REVIEW

REVIEWER	Jesse Anttila-Hughes University of San Francisco Department of Economics
REVIEW RETURNED	16-Jul-2019

GENERAL COMMENTS	This paper is much improved with the revision and I am pleased to see all of my major concerns addressed, notably better comparisons with other datasets' estimates, more explicit discussion of EBF measure choice, and clarification on the counseling variable. I do think that the authors need to slightly expand their discussion of causal inference, and explicitly note that other co-occurring factors such as urbanization or shifting gender norms are certainly also driving at least some of the changes in EBF, and hence it is not appropriate to infer that the entirety of the change was caused by govt action. The authors also say "... no causal inference can be confirmed" and "no causal inference is implied," neither of which are quite correct. One could say "... we cannot say that our estimated changes are causally attributable to the Tshwane Declaration," or simply say many factors are co-occurring in an ecologic analysis and thus it's impossible to perfectly estimate the average causal effect of this single policy change.
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REVIEWER	Sara Jewett Nieuwoudt School of Public Health, University of the Witwatersrand, Johannesburg, South Africa
REVIEW RETURNED	10-Jun-2019

GENERAL COMMENTS	Thank you for addressing all of my prior comments. I remain impressed by this manuscript and believe it adds value to our understanding of how policy may influence behaviour. The following comments are minor and mostly address typos identified in the revised (marked) version submitted as a supplement: p.8, final sentence: "...indoor flush toilet, AND employed mothers..."
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	<p>(add 'and')</p> <p>p.9, final sentence: Break up this sentence. After references 22 & 23 I suggest ended the sentence. From "nevertheless" this is a separate point linked to comparing apples with apples in terms of EBF duration.</p> <p>p.10, first full sentence: Consider rewording this sentence for clarity. I have made notes in the attached document.</p> <p>p.10, first sentence after bold sub-heading: It is more the analytic framework that is different rather than when data were collected, as SADHS also measured before and after the shift.</p> <p>p.10, last paragraph: Reference the review you talk about in the second sentence.</p> <p>p.11, second paragraph: References are needed. See details in attached document.</p> <p>Table 2: There seems to be an extra bracket in the third and fourth columns describing CIs (row directly above Eastern Cape)</p> <p>Table 3: Given the complexity of interpreting this table, as evident in the very long notes, consider how important the table is to the paper (given that 10% differences were not identified). I would be more inclined to retain it if differences were observed, but it is less compelling as it stands and could be addressed in narrative. Of course, this is up to you. If retained, check the title for a missing word or punctuation between "Increase" and "2010"</p> <p>Table 4: Check the use of capitals and lowercase in the title. It looks random as it stands.</p> <p>Table 4: For the last rows, I see you are comparing by year AND whether or not they reported ANC counselling. Forgive my ignorance on this, but shouldn't the reference be Yes if the comparison is by year or is it possible to compare both simultaneously? I'm struggling to interpret the odds ratios here.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 3

Reviewer Name: Sara Jewett Nieuwoudt

Institution and Country: School of Public Health, University of the Witwatersrand, Johannesburg, South Africa

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Thank you for addressing all of my prior comments. I remain impressed by this manuscript and believe it adds value to our understanding of how policy may influence behaviour. The following comments are minor and mostly address typos identified in the revised (marked) version submitted as a supplement:

Thank you!

p.8, final sentence: "...indoor flush toilet, AND employed mothers..." (add 'and')

added

p.9, final sentence: Break up this sentence. After references 22 & 23 I suggest ended the sentence. From "nevertheless" this is a separate point linked to comparing apples with apples in terms of EBF duration.

corrected

p.10, first full sentence: Consider rewording this sentence for clarity. I have made notes in the attached document.

Reworded as suggested

p.10, first sentence after bold sub-heading: It is more the analytic framework that is different rather than when data were collected, as SADHS also measured before and after the shift.

Added analytic framework

p.10, last paragraph: Reference the review you talk about in the second sentence.

p.11, second paragraph: References are needed. See details in attached document.

a. Reference example added for media coverage

b. sentence revised to reflect this statement is a hypothesis

c. Clarified in reference to review as per query on review above

Table 2: There seems to be an extra bracket in the third and fourth columns describing CIs (row directly above Eastern Cape)

deleted

Table 3: Given the complexity of interpreting this table, as evident in the very long notes, consider how important the table is to the paper (given that 10% differences were not identified). I would be more inclined to retain it if differences were observed, but it is less compelling as it stands and could be addressed in narrative. Of course, this is up to you. If retained, check the title for a missing word or punctuation between "Increase" and "2010"

We would prefer to include this table. Title has been revised.

Table 4: Check the use of capitals and lowercase in the title. It looks random as it stands.

This has been corrected for all tables titles

Table 4: For the last rows, I see you are comparing by year AND whether or not they reported ANC counselling. Forgive my ignorance on this, but shouldn't the reference be Yes if the comparison is by year or is it possible to compare both simultaneously? I'm struggling to interpret the odds ratios here.

Clarification and explanation of the last row has been added as a footnote. This row measures multiplicative interaction so the variable is a combination of ANC breastfeeding counseling and Year.

Reviewer: 2

Reviewer Name: Jesse Anttila-Hughes

Institution and Country: University of San Francisco

Department of Economics

Please state any competing interests or state 'None declared': none declared

Please leave your comments for the authors below

This paper is much improved with the revision and I am pleased to see all of my major concerns addressed, notably better comparisons with other datasets' estimates, more explicit discussion of EBF measure choice, and clarification on the counseling variable.

Thank you!

I do think that the authors need to slightly expand their discussion of causal inference, and explicitly note that other co-occurring factors such as urbanization or shifting gender norms are certainly also driving at least some of the changes in EBF, and hence it is not appropriate to infer that the entirety of the change was caused by govt action. The authors also say "... no causal inference can be confirmed" and "no causal inference is implied," neither of which are quite correct. One could say "... we cannot say that our estimated changes are causally attributable to the Tshwane Declaration," or simply say many factors are co-occurring in an ecologic analysis and thus it's impossible to perfectly estimate the average causal effect of this single policy change.

Wording has been adjusted as suggested in last paragraph on page 9 and in Limitations paragraph.