

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Wealth-related inequality in early uptake of HIV testing among pregnant women: An analysis of data from a national cross-sectional survey, South Africa
<b>AUTHORS</b>	Ngandu, Nobubelo; Van Malderen, Carine; Goga, Ameena; Speybroeck, Niko

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Rosa Maria Soares Madeira Domingues Instituto Nacional de Infectologia Evandro Chagas/Fiocruz. Brazil
<b>REVIEW RETURNED</b>	03-Sep-2016

<b>GENERAL COMMENTS</b>	<p>Abstract</p> <p>Page 2, lines 30-32: The authors cannot conclude that HIV testing is improving among pregnant women from poorer backgrounds (the authors analyzed early HIV testing before pregnancy. Moreover, HIV testing was measured only once). The conclusion of high contribution of provinces to inequalities should be reviewed if the adequate sampling process was not used.</p> <p>Background</p> <p>A better description of the South Africa health system would help readers to understand the context of the study.</p> <p>Methods</p> <p>There is no information on the parameters used to calculate the sample size.</p> <p>Page 4, Lines 11-12: The authors state that health facilities were the primary sampling units selected proportionately to the size (small, medium, large). Later (line 14) the authors say that health facilities were randomly selected within each size stratum.</p> <p>Was the health facility size a stratum? If so, was the sample size calculated for each stratum? How many health facilities were selected in each stratum? How were they selected in each stratum? Why the mid-year maternal HIV prevalence was used to measure the size of health facilities? (Page 4, line 13).</p> <p>Page 4, Lines 15-16: the authors say that "caregiver-infant pairs were invited to enroll .....using either random or consecutive selection depending on facility size".</p> <p>There were three different facility sizes. What criterion was used for each of them? What method was used to random selection? How many children were enrolled at each health facility?</p> <p>Page 4, line 18: the inclusion criteria for infants were "aged 4-8 weeks and receiving 6 week immunization". How could infants with less than 6 weeks receive the 6-week immunization?</p> <p>Page 4, lines 35-42: the authors described the independent variables with potential to influence inequalities in the two outcomes but do not explain the reasons why these variables were chosen. For example, number of previous pregnancies could explain early</p>
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access to HIV testing if the woman had been tested during antenatal care in a previous pregnancy.

Page 4, lines 46-52: "The wealth scores to measure socio-economic status were generated from household living conditions and household assets". Why education and source of income were used as independent variables and not as part of the socio-economic status?

Page 6, line 4-5: the authors say that all analyses were adjusted using appropriate sampling weights but it is not clear how weights were calculated. The authors used a complex sampling design (selection of units and then of children within each health facility). There is no information about the use of design effect during the analysis.

#### Results

There is no table of the baseline characteristics of the women included in the study and only some of them are presented on the text (page 6, lines 15-19).

Page 8, line 35: values are different on table 3 and text (table 3 = 0,049, text = 0,049)

Page 8, lines 36-37= the authors state that "the strongest effect was from public transport users from which high pro-poor E(CI)s and regression coefficients were seen". However, the regression-decomposition coefficient of transport for the outcome "early HIV testing" was not significant.

Page 8, lines 43-53: The authors inform that they disaggregated the national data and recalculated the outcome prevalence by province. However, the authors do not inform if the sampling process allowed data to be representative of the nine South African provinces. The finding of a very high pro-rich inequality for infant HIV exposure in KwaZulu Natal, for example, could be the result of selection bias, if an appropriate sampling process had not been used.

#### Discussion

The authors used two different outcomes: "early HIV testing" (before antenatal care) and "infant exposure".

Infant exposure measures maternal infection as the results of the ELISA test (passively transferred maternal anti-HIV antibodies) were used to assess the infection. The results of higher burden of maternal infection in lower wealth groups are similar to other studies that have shown higher prevalence of HIV infection in the most social vulnerable women.

The early HIV testing is not so easy to interpret. Previous studies have shown lower rates of antenatal care and of HIV testing during pregnancy of women with lower socio-economic conditions which reveals inequalities in health care. The HIV testing before pregnancy may have different determinants. Are there differences in health care provision that could explain the greater contribution of the provinces in early HIV testing? Did women seek testing because of symptoms of Sexually Transmitted infection? Could these women have been tested in previous pregnancies or births? Women with high school education had less HIV testing and less infant exposure. Could less testing be the result of a perceived lower risk of infection? Important determinants, like age, could also explain a higher rate of previous testing and were not included in the analysis.

Page 11, lines 14-23 = the authors discuss the findings related to transport and suggest that the least poor live closer to health facilities with walkable distances and stresses the importance of community-outreach and mobile clinic programs especially in remote rural setting where accessibility of facilities is difficult. However, results of Table 1 show no difference on testing rates between those that used public transport or walked and the regression-

	<p>decomposition coefficient of the determinant “Transport” (Table 3) was also not significant.  Knowledge of PMTCT was significantly higher for the infant exposure outcome and the regression decomposition coefficient was also significant for this outcome. Although the contribution of this determinant variable was small, the authors should have discussed this finding.</p> <p>Conclusion  Page 11, lines 51-52: The conclusion that self-initiated uptake of HIV testing among the lower SES group is improving cannot be supported by the results of this study.</p> <p>Limitations  The inclusion of public health facility users only is not a bias but a limitation for the external validity of the results of the study.  The study was not population based; that is, only children that attended health facilities were eligible to the study. This criterion may have underestimated health inequalities if those really poor that live far from the health facilities and cannot afford to pay for public transport were not included.</p>
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<b>REVIEWER</b>	Anthony Simpson Dept of Social Anthropology, University of Manchester, Manchester, U.K.
<b>REVIEW RETURNED</b>	05-Sep-2016

<b>GENERAL COMMENTS</b>	This is an informative study. As an anthropologist, I am definitely not in a position to comment on the use of the statistics in the paper. What I do appreciate, however, is your recognition of the need for qualitative material to explore some of the puzzles that the paper brings to light.
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<b>REVIEWER</b>	Subhash Aryal University of North Texas Health Science Center, USA
<b>REVIEW RETURNED</b>	29-Oct-2016

<b>GENERAL COMMENTS</b>	The method used even after correction is not robust. Alternative statistical methods which are more appropriate for binary data are available in the literature. The calculated indexes are close to zero. Even though one of the index was statistically significant, the practical relevance may not be very high. The significant findings may be due to large sample size. To compare the provinces, how the coding was performed has not been defined. It is stated one categorical variable of the nine South African provinces was included. Was the comparison one province versus the other eight or were they included using eight dummy codes for the nine provinces. Since the most important findings are for the provinces, this needs to be clearly stated. If the comparison was each province versus the other eight, multiple comparison may be an issue. The conclusions may be too optimistic given the parameter estimates and the indexes reported.
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## VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

### Abstract

Page 2, lines 30-32: The authors cannot conclude that HIV testing is improving among pregnant women from poorer backgrounds (the authors analyzed early HIV testing before pregnancy. Moreover, HIV testing was measured only once).

Response: We appreciate the reviewer highlighting that the conclusion in the ABSTRACT could be misleading. We have revised the conclusion to remove the implication that ‘early HIV testing’ was done during pregnancy and that a trends analysis was done (i.e. using the word ‘improving’). The ABSTRACT-conclusion now reads as follows:

“Our results on better early uptake of HIV testing amongst the poorer sub-population compared with the richer highlights inequity in uptake of HIV testing in South Africa. The higher burden of infant HIV exposure in the poorer sub-population may reflect this differential uptake or could illustrate increased maternal HIV prevalence amongst poor people. This socio-economic difference could delay EMTCT in South Africa. The high contribution of provinces to inequality highlights a need to shift from reliance on national-level estimates alone. Future interventions need to be context specific and tailored for different socio-economic sub-populations and sub-regional settings.”

The conclusion of high contribution of provinces to inequalities should be reviewed if the adequate sampling process was not used.

Response: We have given more explicit detail in the first paragraph of the METHODS section of the manuscript about how the sampling process was done. The survey sampling approach was done such that the results of the survey were representative of provincial-level and national-level reporting of estimates. We provide details of this revision under the reviewer comments of the METHODS section below.

### Background

A better description of the South Africa health system would help readers to understand the context of the study.

Response: We thank the reviewer for bringing this to our attention. More detail describing the South African health system has been provided in paragraph 4 of the BACKGROUND section. The revised section now reads as follows;

“The other challenge is the unequal health care system which is dualistically divided into public and private sectors. The majority (~68%) of the population use the public health care system which however is serviced by only 30% of the country’s doctors and specialists<sup>17</sup>. The public sector has a three tier service provision system; the primary health care clinics and community health centres which serve as the first contact at no cost, for basic health and maternity care; these make referrals of complicated cases to the secondary level care – the district hospitals. Academic hospitals form the highest level and mostly serve more complicated healthcare needs. Reports of 2015, indicate a doctor-patient ratio of 1:>4000 in the public sector with still ~4% of the population living at least 5km away from the nearest health facility<sup>18</sup>. The private sector, smaller, comprises of private-practising healthcare professionals and private hospitals whose services are mainly remunerated through the medical aid schemes. Comparatively, the primary level of the public sector is mostly over-burdened and does experience sub-standard service provision while the private sector mainly offers high quality service. The government and some non-governmental organisations have expanded the number of primary health care clinics in an effort to decongest and improve the quality of public healthcare. Improvements for maternal and child health care have been at the forefront of attempts to improve public health care, such as the recent revisions of the PMTCT consolidated guidelines<sup>19</sup>.”

## Methods

There is no information on the parameters used to calculate the sample size.

Page 4, Lines 11-12: The authors state that health facilities were the primary sampling units selected proportionately to the size (small, medium, large). Later (line 14) the authors say that health facilities were randomly selected within each size stratum.

Was the health facility size a stratum? If so, was the sample size calculated for each stratum? How many health facilities were selected in each stratum? How were they selected in each stratum?

Why the mid-year maternal HIV prevalence was used to measure the size of health facilities? (Page 4, line 13).

Page 4, Lines 15-16: the authors say that “caregiver-infant pairs were invited to enroll .....using either random or consecutive selection depending on facility size”.

There were three different facility sizes. What criterion was used for each of them? What method was used to random selection? How many children were enrolled at each health facility?

Response: The reviewer’s comments indicate that the description of the sampling process for the survey was not given adequately, and we are thankful for this insight. We have revised this section by adding more detail for clarity. The first paragraph of the METHODS section now reads as follows:

“A secondary analysis of data from a national cross-sectional survey, conducted in 2012 to evaluate the South African PMTCT program, was conducted<sup>20</sup>. The methods have been explained in detail elsewhere<sup>21</sup>. In summary, the survey was conducted at public primary health care clinics and community health centres offering immunisation services countrywide. The primary aim was to measure national and provincial-levels MTCT amongst infants attending public health facilities for their 6 week immunisation. Infants with known and unknown HIV exposure were eligible for inclusion. The 6-weeks postpartum point was chosen because it has a 99% infant coverage for immunisation<sup>22</sup>. Antenatal HIV prevalence and presumed PMTCT coverage were used to estimate the sample size needed for each province at precisions of 30% to 50% and a design effect of 2. The national target sample size was 12 200, ranging between 700 and 1800 per province, proportional to provincial six week immunisation coverage. A two-stage probability proportional to size sampling approach was used. The first stage was at provincial level. In each province health facilities were stratified into medium (130-300 immunisations per year) and large (300 immunisations or more per year) facilities. Large facilities were further stratified into two groups - facilities in districts with antenatal HIV prevalence <29% or ≥29%, which was the 2009 national average antenatal HIV prevalence. Therefore facilities were grouped into three strata. The second stage was at health facility level: 580 facilities selected proportional to target facility sample size, were needed to achieve the desired provincial and national sample sizes. The target number of infants per facility was taken as the median number of infants expected in each facility within each stratum over a three week data collection period. Finally, caregiver-infant pairs were invited to enrol into the study during the 6-weeks immunization visit using either random or consecutive selection depending on facility size. Ultimately, 10533 infants were screened and 9120 provided both interview and infant blood data to measure MTCT. With respect to the data analysis for the primary outcome (6-week MTCT), sampling weights were calculated as the inverse of the realised sample size, accounting for South African live births, relative to the target sample size for each facility.”

Page 4, line 18: the inclusion criteria for infants were “aged 4-8 weeks and receiving 6 week immunization”. How could infants with less than 6 weeks receive the 6-week immunization?

Response: Although immunization is strictly meant to be at 6 weeks, it was observed that mothers brought in infants when they were aged between 4 and 8 weeks for the 6 weeks immunization. The

reasons could be related to logistics of visiting the facility. This study did not investigate these reasons.

Page 4, lines 35-42: the authors described the independent variables with potential to influence inequalities in the two outcomes but do not explain the reasons why these variables were chosen. For example, number of previous pregnancies could explain early access to HIV testing if the woman had been tested during antenatal care in a previous pregnancy.

Response: The rationale for adding each independent variable has been included in paragraph 3 of METHODS. The section now reads as follows:

“Independent variables with potential to influence inequalities in the two outcomes were chosen, i.e., variables which can influence or be influenced by socio-economic background and at the same time can influence at least one of the outcomes: Educational level, dichotomised as primary school and lower or high school and above was selected as education could influence attitudes towards the importance of healthcare; Marital status, dichotomised into single women (i.e., not married, not in a relationship, widows, divorced) and married (or cohabiting) women, was included as spousal support is likely to encourage uptake of healthcare; Transport to a health facility categorised into own car, public transport and walking was included as a marker of ease of healthcare access, affecting the frequency and timing of uptake; Prior knowledge about PMTCT as either ‘yes’ or ‘no’ was included as prior knowledge can influence timing of HIV testing in relation to pregnancy; A categorical variable of the nine South African provinces was included as provincial differences in healthcare management and in cultural behavioural norms has been observed; lastly, source of income with four categories of women namely employed, dependent on extended family, dependent on spouse or partner and fourthly those with irregular sources of income such as government grants were included. The latter is not a good measure of household income but is a common structural division in South Africa, and it will be important to know whether and how it impacts on the primary outcome variables.”

Page 4, lines 46-52: “The wealth scores to measure socio-economic status were generated from household living conditions and household assets”. Why education and source of income were used as independent variables and not as part of the socio-economic status?

Response: In the South African population group included in this survey, education is not a good wealth ranking measure. Due to the history of the country, there are people with less education who have grown to be wealthier than those with much higher education. Source of income as opposed to the amount of income is not a good measure of wealth status. Being employed in this particular survey did not take into account the level of trade for the employment. Nor does depending on a spouse for example reflect the amount of spouse’s income. This was also tested in the analyses, and the socio-economic status measurements were not changed by including source of income. Nevertheless, these groups are common in the country and hence important to know how they impact on HIV healthcare uptake and outcomes.

Page 6, line 4-5: the authors say that all analyses were adjusted using appropriate sampling weights but it is not clear how weights were calculated. The authors used a complex sampling design (selection of units and then of children within each health facility). There is no information about the use of design effect during the analysis.

Response: The sampling design has been explained with more clarity in the first paragraph of the METHODS section. The precisions and design effect used have been provided.

## Results

There is no table of the baseline characteristics of the women included in the study and only some of them are presented on the text (page 6, lines 15-19).

Response: All the baseline characteristics available and important to this study are the ones referred to by the reviewer, age, education, source of income and marital status. A table would be too small for these hence it makes sense to present them in the text only. Based on the focus of this paper and with two outcome variables, it makes more sense to present the independent variables, for which some are baseline characteristics, by each outcome variable as we do in Table 1, otherwise the information will be repetitive.

Page 8, line 35: values are different on table 3 and text (table 3 = 0,049, text = 0,049)

Response: It is not clear what the reviewer means and for which variable specifically. The values are the same. In the text we used the word "reduced" which logically refers to a negative regression coefficient. If this is what the review was intending to mean.

Page 8, lines 36-37= the authors state that "the strongest effect was from public transport users from which high pro-poor E(CI)s and regression coefficients were seen". However, the regression-decomposition coefficient of transport for the outcome "early HIV testing" was not significant.

Response: We agree with the reviewer. The overall contribution was very high but the regression coefficient was not statistically significant. We have now acknowledged this in the same sentence and it now reads as follows;

"Within means of transport, the strongest effect was from public transport users from which high pro-poor E(CI)s and regression coefficients were seen, although the regression coefficient for early HIV testing was not statistically significant."

Page 8, lines 43-53: The authors inform that they disaggregated the national data and recalculated the outcome prevalence by province. However, the authors do not inform if the sampling process allowed data to be representative of the nine South African provinces. The finding of a very high pro-rich inequality for infant HIV exposure in KwaZulu Natal, for example, could be the result of selection bias, if an appropriate sampling process had not been used.

Response: The sampling has been explained with more clarity in the first paragraph of the METHODS section, as mentioned earlier. Sampling was done such that estimates from the data can be valid for both provincial and national levels.

## Discussion

The authors used two different outcomes: "early HIV testing" (before antenatal care) and "infant exposure".

Infant exposure measures maternal infection as the results of the ELISA test (passively transferred maternal anti-HIV antibodies) were used to assess the infection. The results of higher burden of maternal infection in lower wealth groups are similar to other studies that have shown higher prevalence of HIV infection in the most social vulnerable women.

The early HIV testing is not so easy to interpret. Previous studies have shown lower rates of antenatal

care and of HIV testing during pregnancy of women with lower socio-economic conditions which reveals inequalities in health care. The HIV testing before pregnancy may have different determinants.

Response: We agree with the reviewer that HIV testing before pregnancy could be difficult to interpret unless data were collected for pre-pregnancy lifestyles. However, it is informative to a good extent as it reflects averaged differences in perceptions about the importance of knowing one's HIV status before conception and general uptake of HIV testing among the lower wealth groups in South Africa. We begin the DISCUSSION section by highlighting this as the message from our study.

Are there differences in health care provision that could explain the greater contribution of the provinces in early HIV testing?

Response: The policy for HIV testing and for PMTCT in South Africa are managed at a national level. Theoretically, there are a number of factors which can promote differences in health outcomes between provinces including provincial management cultures in the health sector as well as cultural norms of the majority tribes or ethnic groups in the province, among others. However, this survey did not focus on collecting such secondary data to confidently relate the findings to specific reasons, hence we conclude by stating the need for sub-geographical (provincial-level) investigations in future.

Did women seek testing because of symptoms of Sexually Transmitted infection? Could these women have been tested in previous pregnancies or births?

Response: The reviewer's questions are beyond the information gathered during the survey. What is important is that women were aware of their HIV status before getting pregnant, which is highly recommended in order to reduce vertical transmission.

Women with high school education had less HIV testing and less infant exposure. Could less testing be the result of a perceived lower risk of infection?

Response: It is possible that less testing among the women with higher education could be a result of perceived lower risk of infection. However, the survey did not gather information to relate to this possibility hence it cannot be included as part of the discussion.

Important determinants, like age, could also explain a higher rate of previous testing and were not included in the analysis.

Response: We agree with the reviewer that age could have a relationship with rate of previous HIV testing. We did not include it because we chose independent variables which can also be independently related to socio-economic background. In South Africa, age cannot be expected to correlate with socio-economic background because of the high unemployment rate. It could be a true predictor in high income countries. As stated earlier, we have specified the criteria for selecting independent variables in the METHODS section.

Page 11, lines 14-23 = the authors discuss the findings related to transport and suggest that the least poor live closer to health facilities with walkable distances and stresses the importance of community-outreach and mobile clinic programs especially in remote rural setting where accessibility of facilities is difficult. However, results of Table 1 show no difference on testing rates between those that used



public transport or walked and the regression-decomposition coefficient of the determinant “Transport” (Table 3) was also not significant.

Response: The discussion is very accurate for infant HIV exposure which as shown in Table 1 was higher among those who used public transport compared to those who walked or owned cars, and higher among those who walked compared to those who owned cars. However, for early HIV testing, differences were still there but not as strong. In Table 1, the 95% CI show clear differences between those who used public transport and those who owned cars only. The contribution to inequality is high and equal between the two outcomes. Certainly for HIV testing it is not due to the regression (association between transport and the outcome) but mainly due to the wealth-related inequality as reflected. The latter is obviously driven by the difference between public transport users and those who own cars, which is an indirect reflection of wealth status.

We have included a detailed motivation for the recommendation in paragraph 3 of the DISCUSSION, which now reads as;

“This could indirectly reflect the distance from facilities which need to be travelled, wherein poorer communities live further away from health facilities while the least poor live closer to health facilities with walkable distances or choose to live far from services if they can afford private transport. The insignificant regression coefficients for HIV testing are due to a weak difference between those who walked and the rest. There were clear differences in the rate of both outcomes between those who owned cars and those who used public transport, leading to high contributions to inequality in both cases.”

Knowledge of PMTCT was significantly higher for the infant exposure outcome and the regression decomposition coefficient was also significant for this outcome. Although the contribution of this determinant variable was small, the authors should have discussed this finding.

Response: We have added a discussion on this observation at the end of paragraph 2 of the DISCUSSION section. The following was added;

“Even though knowledge about MTCT was significantly associated with infant HIV exposure, it had no influence on wealth-related inequality implying that national efforts on HIV education have not prioritised certain socioeconomic groups over others.”

#### Conclusion

Page 11, lines 51-52: The conclusion that self-initiated uptake of HIV testing among the lower SES group is improving cannot be supported by the results of this study.

Response: As highlighted by the reviewer in the ABSTRACT, we have also revised this sentence in the CONCLUSION to remove the implication that a trends analysis was done. The sentence now reads as follows;

“However, self-initiated uptake of HIV testing among the lower SES group before pregnancy indicates good awareness of HIV among the economically disadvantaged and at the same time reveals inequity between the richer and poor. The unequally high infant HIV exposure amongst the poorer could be reflecting differences in risk behaviour choices or/and be a result of better uptake of HIV testing among the poorer.”

#### Limitations

The inclusion of public health facility users only is not a bias but a limitation for the external validity of

the results of the study.

Response: We have revised the first sentence of the LIMITATIONS to remove the implication of 'bias' and inform readers accordingly. It now reads as follows;

“One limitation of this study is that its findings are only valid for the South African population using public health facilities.”

The study was not population based; that is, only children that attended health facilities were eligible to the study. This criterion may have underestimated health inequalities if those really poor that live far from the health facilities and cannot afford to pay for public transport were not included.

Response: Thank you to the reviewer for highlighting this. We have added this limitation at the end of the 'Limitations' section and it reads as follows:

“Lastly the study was facility-based, but enrolled a nationally representative sample across all nine provinces of South Africa; we did not include mother and infants who were too poor to access health facility care. Thus we could have under-estimated the impact of poor socio-economic status amongst the poorest group. However, given that routine data estimate that 99% of livebirths attend health facilities for their 6 week immunisation we do not believe that this under-estimate significantly changes our overall estimate.”

Reviewer: 2

This is an informative study. As an anthropologist, I am definitely not in a position to comment on the use of the statistics in the paper. What I do appreciate, however, is your recognition of the need for qualitative material to explore some of the puzzles that the paper brings to light.

Response: We appreciate that the reviewer understands the important messages presented in the manuscript.

Reviewer: 3

The method used even after correction is not robust. Alternative statistical methods which are more appropriate for binary data are available in the literature. The calculated indexes are close to zero. Even though one of the index was statistically significant, the practical relevance may not be very high. The significant findings may be due to large sample size. To compare the provinces, how the coding was performed has not been defined. It is stated one categorical variable of the nine South African provinces was included. Was the comparison one province versus the other eight or were they included using eight dummy codes for the nine provinces. Since the most important findings are for the provinces, this needs to be clearly stated. If the comparison was each province versus the other eight, multiple comparison may be an issue. The conclusions may be too optimistic given the parameter estimates and the indexes reported.

Response: We thank the reviewer for mentioning this, indicating that we may not have been fully clear in our text. The comments seem to indicate that we should not have used a basic regression analyses

model and we could not agree more. Regression analyses on their own are indeed not appropriate for reporting wealth-related inequalities. However, we used methods that have an outcome that includes two variables, health and wealth. The methods we used are, to the best of our knowledge, the most robust, as also seen from the references used for the methodological approach. If however the reviewer is aware of a recent method which can be used to accurately measure and analyse wealth-related inequality, we will of course appreciate it being referred to us.

Regarding the magnitude of the indices, the CI and E(CI) scores can range from negative 1 to positive 1 and yes the range gives very strong and less strong inequalities but inequality exists as long as the index is not zero. We do not claim anywhere in the manuscript that the indexes are very strong, but we show that wealth-related inequality is present.

For analyses of data at provincial level, no direct statistical comparisons were done between provinces. Instead, wealth-related inequalities were calculated separately for each province alongside outcome variable estimates. That is, only a descriptive presentation of the provincial level measurements was performed. We have added clarity regarding provincial level analyses in the METHODS section, last paragraph, and it now reads as follows.

“Data analyses at provincial level

Given that the survey data are also valid for provincial level estimates and given the observed differences in the primary outcomes of the survey between provinces, we presented descriptive summaries of the outcome variables and wealth-related inequalities by province. We calculated wealth-related inequalities for each province separately using the same approach presented above. The same socio-economic ranking scores obtained from the combined national data were applied in the provincial-level calculation of wealth-related inequality. The estimates for each outcome were also calculated separately for each province.”

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Subhash Aryal Unt Health Science Center USA
<b>REVIEW RETURNED</b>	14-Dec-2016

<b>GENERAL COMMENTS</b>	The authors have not addressed my concern significantly, only provided minor clarification. I am not enthusiastic about the method used.
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<b>REVIEWER</b>	Sungwook Kim Warwick medical school, UK
<b>REVIEW RETURNED</b>	01-Mar-2017

<b>GENERAL COMMENTS</b>	<ol style="list-style-type: none"> <li>1. Many equity papers on binary variables use concentration index without Erreygers correction. So could you please make this point more clear?</li> <li>2. Were there any need variables in the data? It is common to decompose the concentration index using both non-need (SES variables) and need variables. In fact this method will be better to present horizontal equity based on equal treatment for equal needs (ETEN).</li> <li>3. It's not clear why CI was used on Infant HIV exposure. This can be simply done with a descriptive table. Chi 2 test will be sufficient to</li> </ol>
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	<p>examine whether there are differences across SES groups. If you want to measure wealth related inequality, CI should be used for 'access' or 'utilisation' for health care. This also affect table 3. What's the association between infant exposure and public transport?</p> <p>4. It's not clear what are the findings from your analysis at 'province' level. If you have found 'urban' areas use more, it can be meaningful but at the moment, the implication of this analysis is not clear. Why we need to do this analysis needs to be mentioned in introduction more clearly.</p> <p>5. Main conclusion is not clear. This shows pro-poor CI in both of outcome measures. However, in the context of South Africa, this result would be compared with the existing literature in South Africa. For example, mother's education may or may not affect HIV testing. Rather, father's economic power can be more significant factor affecting HIV testing as many researches exist.</p> <p>Based on these questions, it seems to be revised significantly. This paper needs to set up more clear research question and the result should be delivered in the discussion with the existing research.</p>
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## VERSION 2 – AUTHOR RESPONSE

Reviewer 4 (sungwook kim):

1. Many equity papers on binary variables use concentration index without Erreygers correction. So could you please make this point more clear?

Response: We thank the reviewer for this comment. We have added a paragraph at the end of the discussion (before 'Limitations') briefly explaining why we chose to use the Erreygers' corrected concentration index. Here we indeed also mention that "not all studies with binary outcomes use this correction".

2. Were there any need variables in the data? It is common to decompose the concentration index using both non-need (SES variables) and need variables. In fact this method will be better to present horizontal equity based on equal treatment for equal needs (ETEN).

Response: The reviewer's suggestion is appreciated and has been used to improve the analysis. We have added 3 need variables which were available in the data (age, illnesses during pregnancy – TB and syphilis). We therefore now present need-standardised concentration indexes in two ways; (i) using a concentration index from a decomposition which includes the need-variables in addition to other non-need variables and (iii) subtracting the concentration index predicted for the need-based uptake from the standard concentration index. We have also added example references (in the last paragraph of the 'Discussion', before 'Limitations') of three studies who have used this approach.

3. It's not clear why CI was used on Infant HIV exposure. This can be simply done with a descriptive table. Chi 2 test will be sufficient to examine whether there are differences across SES groups. If you want to measure wealth related inequality, CI should be used for 'access' or 'utilisation' for health care. This also affect table 3. What's the association between infant exposure and public transport?

Response: The reviewer's insights are accepted. We have excluded 'infant HIV exposure' as an outcome from the manuscript and now present focus only on the access indicator – uptake of HIV testing. As the reviewer suggested, and because infant HIV exposure is important in understanding the characteristics of the study sample, we have included a chi-squared test result reporting its distribution between the three wealth groups. We have added this result in the first paragraph of the

'Results' section where we describe the study sample characteristics.

4. It's not clear what are the findings from your analysis at 'province' level. If you have found 'urban' areas use more, it can be meaningful but at the moment, the implication of this analysis is not clear. Why we need to do this analysis needs to be mentioned in introduction more clearly.

Response: We understand the reviewer's concern. Unfortunately the data do not have rural-urban information. We had included province based on different prevalence of HIV outcomes usually observed at provincial level but we agree that this may not be a wealth-related effect as population wealth may not be systematically disproportionate between provinces. Therefore we have excluded the detailed provincial level analyses. Further we articulate under 'Limitations' the lack of rural-urban allocation information in our study data. Having this kind of information would have added more granularity to our results to help understand the geographic-related inequalities better.

5. Main conclusion is not clear. This shows pro-poor CI in both of outcome measures. However, in the context of South Africa, this result would be compared with the existing literature in South Africa. For example, mother's education may or may not affect HIV testing. Rather, father's economic power can be more significant factor affecting HIV testing as many researches exist. Based on these questions, it seems to be revised significantly. This paper needs to set up more clear research question and the result should be delivered in the discussion with the existing research.

Response:

The manuscript is now only focussed on uptake of HIV testing. We have added literature which supports and helps the understanding of the findings (2014). Our discussion and conclusions now show clearly that age, province, transport and wealth-group are the main contributors of inequality in early uptake of HIV testing. Age poses a strong 'need' effect on inequality. We also raise it to the readers that the 'age' results echo what South Africa is currently experiencing and attempting to solve – the challenge of uptake of health care among adolescent and young mothers (examples added are Fatti et al (2013), Horwood et al (2014), see paragraph 2 of discussion). We also mention that the high contribution of province likely reflects underlying rural-urban disparities. Since our data is limited to perform this kind of evaluation, we recommend it to be a preferred approach for future studies in light of already existing reports of rural-urban inequality in service delivery (examples referenced are; Booysen (2003), van der Hoeven (2012)). The results from wealth-group and transport support the pro-poor inequality observed in uptake of HIV testing. We find the results on transport worth to note as distance has challenged the poor in accessing health care, not only in South Africa in the past (example added is McLaren (2014)) but also in other African countries too (examples from Geng et al (2010), Wachira et al (2014)), yet here, public transport users, whom most are the poor, show better uptake of HIV testing. We further highlight that the general distribution of HIV testing across education backgrounds is similar to reports from other studies (example Maughan-Brown (2016)) but in our study we further confirm that the differences in the antenatal population are not driven by wealth-related inequality.

### VERSION 3 – REVIEW

<b>REVIEWER</b>	Sungwook Kim Warwick medical school, U.K
<b>REVIEW RETURNED</b>	08-Apr-2017
<b>GENERAL COMMENTS</b>	Authors worked on my previous comments well.