

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Determinants of immunisation coverage of children aged 12-59 months in Indonesia: a cross-sectional study.
AUTHORS	Herliana, Putri; Douiri, Abdel

VERSION 1 – REVIEW

REVIEWER	Helen Petousis-Harris University of Auckland New Zealand
REVIEW RETURNED	23-Feb-2017

GENERAL COMMENTS	<p>The Health survey has provided an opportunity to explore this question. Overall this is a well written manuscript and the findings will be directly useful to public health strategies.</p> <p>Abstract</p> <p>The abstract could be improved in several ways. The first sentence is redundant, the value of immunisation does not need to be stated. The second sentence is background not an objective but it serves well as the opening statement. Consider reordering the section on participants by putting the 'participants were randomly selected' before 'data from 14,401...' In the results suggest "The mean/median age of the children was [months]..." Avoid value statements such as 'only' 32%, simply state 32%. Last sentence in abstract suggest modifying to: 'To achieve the WHO target of protective coverage, public health....'</p> <p>Strengths and limitations Second point. Better estimates than what? Fourth point: Make a point how does this limit the study? Suggest reframing.</p> <p>Background. Suggest deleting the first paragraph as this is extraneous to the paper. All these things are taken as a given.</p> <p>Second paragraph. This is a better opening paragraph. Focus on coverage in low income countries followed by Indonesia. Third paragraph. This is great, it could go earlier.</p> <p>Methods Outcome variable: -Specify at what age the child was considered fully vaccinated. If this is age-appropriate vaccination then make it clear.</p>
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– vaccination status. The main issue I have with the paper is the rationale and potential importance of categorising the partially vaccinated as unvaccinated.

-Categorising the children with uncertain status to unimmunised seems arbitrary. These should be removed and categorised 'unknown'.

Independent variables

The 33 provinces in Indonesia were categorised into six island-based regions. Is this a national practice of the authors own categorisation?

Results

I have a major issue over the handling of the partially vaccinated. This group needs to be handled as a third group. There are many reasons why they may be different from unvaccinated. This is certainly the case in NZ where we see partially vaccinated as having different (in fact greater) risk factors than unvaccinated. At the very least there should be some sensitivity analysis performed to explore this.

I cannot see any convincing argument as to why these should be lumped together.

The text in the results section could be reduced by only describing the most significant findings, not everything in the tables.

Household Wealth Index – Add a foot not to explain how this is derived.

Antenatal care - quantify 'some care'. This could possibly be broken down further to an ideal level of care and perhaps just one or two contacts? I suspect there is a wide spectrum.

Be precise in reporting of variables. Report the age of the children in months not years.

"The mothers were 25-29 years old on average" Present this precisely. There are many examples of this throughout the results section. "Almost all", nearly half". Please change all of these to the exact numbers or proportions.

Discussion

The discussion is thorough and well considered.

Page 24. Did the authors find any evidence for Vaccine Hesitancy?

Page 27.

Strengths and limitations.

Remove the statement about the computations taking a huge amount of time.

REVIEWER	Dr. Christina Poethko-Müller Robert Koch-Institute, Germany
REVIEW RETURNED	27-Mar-2017

GENERAL COMMENTS	<p>This is a well-conducted data analysis, yielding interesting results. The scientific contribution is useful also for the practice and immunization policy in developing countries. The paper is well-written and concise. There are, however, some minor issues, which should be modified prior to publication.</p> <p>Background</p> <p>1.) Please check reference [8]. http://www.who.int/immunization/monitoring_surveillance/global_immunization_data.pdf The URL leads to a 2014 paper, most probably the statement derive from estimates based on data from 2008. Consequently the sentence “Despite this progress, vaccine preventable diseases are still responsible for 1.5 million child deaths each year.[8]” needs to be adapted.</p> <p>2.) Please specify the citation concerning the statement “... This highlights the fact that global coverage may hide variability between countries. It also suggests that the achievements are still fragile. Should this trend continue, the goals of providing universal immunisation for all children by 2020 and ending vaccine-preventable deaths by 2030 could not be achieved, and the cost of such failure would be close to 26 million child deaths.[5]” (p5 row12-14). Search of the reference [5] (World Health Organisation. Global Vaccine Action Plan. Geneva: WHO Press) did not reveal data supporting this statement.</p> <p>Methods Independent variables</p> <p>3.) Please specify the methodology that was applied to build the wealth index. Which amenities and assets were used for building the wealth index? How were the data aggregated? Please state a reference concerning the IDHS methodology/study protocol.</p> <p>Results</p> <p>4.) Please elaborate on the data source of the child’s immunization status: What is the proportion of health card/book based immunization information? Table 2 and table 3</p> <p>5.) Please describe the univariate analyses more clearly in the heading, i.e.: “...factors associated with being unimmunized” Table 2</p> <p>6.) In table 2 the proportion of immunized versus unimmunized children is expressed as column percentage. Transparency and information about determinants would be improved by stating row percentages instead.</p> <p>7.) The following statements are not fully correct as wording and the stated p-value falsely imply that results derive from a statistical trend-test. “The odds of being unimmunised increased as the child’s age and birth order increased (p<0.000).</p>
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“As the number of household members increased, the likelihood of a child to be unimmunised increased ($p < 0.000$).”
“As parents’ educational attainment increased, the likelihood of being unimmunised decreased ($p < 0.000$).”
“Regarding mother’s exposure to media, the child’s likelihood of being unimmunised increased as the frequency of media exposure decreased ($p < 0.000$).”
“We found that as the household wealth index increased, the likelihood of being unimmunised decreased ($p < 0.000$)”
Discussion

8.) The following paragraph is not fully comprehensive and is not convincing. The question whether poverty acts as a mediator-variable concerning the association between urban/rural areas and immunization should be investigated by step-wise inclusion of variables in the multivariable model (first step include area level (urban/rural), second step include wealth index in addition and observe whether OR for area level is getting smaller). Investigation of poverty as a potential moderator necessitates the inclusion of interaction terms in your model. Both approaches are feasible with your data without information to distinguish between urban areas with higher socioeconomic status and the slums.
“..., our results revealed that children who lived in rural areas were less likely to be unimmunised. Although health services are better and more easily accessible in urban areas compared to rural areas,[29] this fact likely masks the extent of urban poverty.[31] Estimates suggest that one third of urban populations in developing countries are actually living in slums.[40] With limited access to health services and poor quality of life, it is certainly likely that urban children had higher odds of being unimmunised. Unfortunately, we lacked information to distinguish between urban areas with higher socioeconomic status and the slums. ...”

9.) The statement of the following paragraph is plausible. However the citation of reference [41] is not fully sufficient as trends in Indonesia may differ from trends in Burkina. Please discuss health and immunization policy development in Indonesia which may support the given explanation for birth cohort effects.
“Our analysis revealed that children of older age groups were significantly more likely to be unimmunised compared to those in the youngest group. In other words, later birth years were associated with better immunisation coverage. It may indicate a positive trend of the immunisation programme performance over the years.[41] “

REVIEWER	Christiana Rialine Titaley Faculty of Medicine Pattimura University, Ambon Indonesia
REVIEW RETURNED	30-May-2017

GENERAL COMMENTS	<p>The research question posed by authors regarding the coverage of immunisation is important, particularly in developing countries including Indonesia. The manuscript is clear. There are few suggestions to improve the manuscript further.</p> <p>Title: After reviewing the manuscript, I think the analysis is more with children being “unimmunised” than “immunised”. Authors might want to reconsider the title. Please include the source of data in the title.</p> <p>Strengths and limitations:</p> <ul style="list-style-type: none"> • Again, after reviewing the manuscript, I think the analysis is more with children being “unimmunised” than “immunised”. • Authors mentioned that they could only look at the children nested within census block. How about nested into province? <p>Background:</p> <ul style="list-style-type: none"> • The background is too lengthy and could be made more concise. • As mentioned earlier, please reconsider the use “unimmunised” in the objective statement. <p>Methods:</p> <ul style="list-style-type: none"> • Please provide the in-text citation for IDHS 2012 data. • I suggest authors to state clearly that this is a secondary analysis rather than a primary data used. • I think, the final sample size used in the analysis should be stated clearly in the Methods section. By the end of “Data source” sub section, reader might think that information from 18,021 children was used in the analysis. • How if women had more than one children under five? Did authors involve all of the under 5 children? Please explain this clearly to avoid confusion. • Please explain more detail about the groups of variables in Andersen’s Behavioural Health Model. • For the household wealth index, how was this variable constructed? • For the definition of ANC authors mentioned “any care”. Any care provided by who? Similar to PNC. Any care provided by who? Does it include care provided by traditional birth attendants? • Under the statistical analysis authors mentioned about the sample size. If there were 13,745 cases completed why did authors use 14,041 children since authors mentioned they used “complete-case analysis”. If authors used 14,041 children, did the 656 included children died after their first birthday? Please explain clearly. • Did authors use complex sample survey analysis? Did the analysis weight the data for sampling probability? What are the command used by authors in Stata for the multilevel modelling? – this could be stated in the Methods Section • Did authors check for collinearity taking into account the large number of variables used in the analysis?
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- For the univariate analysis authors mentioned that they use univariate p value. How was this assessed for variables with more than two categories? Each category p value or combined p value?

Results:

- The results section is too lengthy. Could be made more concise.
- One of the variables used is the ownership of health insurance. What kind of health insurance? At that time, has a health insurance might imply children from poor family.
- Table 2 and 3 are more related to factors association with unimmunised status. Please revisit this.
- For the multilevel logistic regression, I recommend authors to carefully select the reference group so that the results could be interpreted similarly, i.e. all are risk factors (aOR greater than 1) rather than some were risk factors and some were protective factors (aOR smaller than 1). E.g. it might be clearer to make rural category as the reference group instead of urban category as the reference group.
- Authors has not mentioned the clustering level or the results of taking into account the role of environment.

Discussion:

- Too lengthy and could be made more concise
- Overall, authors could suggest some interventions that could be conducted to improve the condition. Previous literature could be used to support authors' claim. For example, what could be suggested for areas with low coverage of immunisation, e.g. Maluku and Papua or urban children? What could be suggested for children of older age groups or high birth ordered children or children from large families? Etc
- It is very important to address the issue of children not exposed to health system – not attending ANC, PNC or born at home.
- Line 34-36 needs a reference
- How about the role of cluster/environment from the multilevel analyses □ this has not been explained.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

Helen Petousis-Harris

University of Auckland, New Zealand

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

Please leave your comments for the authors below:

Thank you for the opportunity to review this manuscript on the determinants of immunisation coverage of children in Indonesia. The Health survey has provided an opportunity to explore this question. Overall this is a well written manuscript and the findings will be directly useful to public health strategies.

Abstract

Comment: The abstract could be improved in several ways. The first sentence is redundant, the value of immunisation does not need to be stated. The second sentence is background not an objective but it serves well as the opening statement.

Response (R): We have deleted the first sentence.

Comment: Consider reordering the section on participants by putting the 'participants were randomly selected' before 'data from 14,401...'

R: We have re-ordered the sentences accordingly.

Comment: In the results suggest "The mean/median age of the children was [months]..."

R: Amended into 'months'.

Comment: Avoid value statements such as 'only' 32%, simply state 32%.

R: Amended accordingly.

Comment: Last sentence in abstract suggest modifying to: 'To achieve the WHO target of protective coverage, public health....'

R: Amended accordingly.

Strengths and limitations

Comment: Second point. Better estimates than what?

R: Amended by replacing with 'reliable estimates'.

Fourth point: Make a point how does this limit the study? Suggest reframing.

R: Re-framed to better reflect the limitation of the study.

Background

Comment: Suggest deleting the first paragraph as this is extraneous to the paper. All these things are taken as a given. Second paragraph. This is a better opening paragraph. Focus on coverage in low income countries followed by Indonesia. Third paragraph. This is great, it could go earlier.

R: We have deleted the first paragraph.

Methods

Outcome variable:

Comment: Specify at what age the child was considered fully vaccinated. If this is age-appropriate vaccination then make it clear.

R: We have specified the age on Page 7 Line 17. Manuscript amended into '... scheduled to be received by the age of 12 months'.

Comment: Vaccination status. The main issue I have with the paper is the rationale and potential importance of categorising the partially vaccinated as unvaccinated.

R: Addressed below.

Comment: Categorising the children with uncertain status to unimmunised seems arbitrary. These should be removed and categorised 'unknown'.

R: As discussed under Outcome Variable sub section on Page 7-8 of the manuscript, studies have shown that mothers responding 'don't know' is likely to reflect that the child was not fully immunised. Combining 'don't know' with 'unimmunised' had therefore been done by other researchers in the past, and case-wise deletion provided similar results. The small size of the 'don't know' (1.51%) group indicates that there was little likelihood of bias in combining this group with 'unimmunised'.

Independent variables

Comment: The 33 provinces in Indonesia were categorised into six island-based regions. Is this a national practice of the authors own categorisation?

R: We used IDHS 2012 categorisation. We have put in-text citation to avoid confusion. It is relatively common to group the 17,000 islands and 33 provinces across Indonesia under five or six major island-based regions. The rationale is discussed on Page 21 Line 21-24 of the manuscript.

Results

Comment: I have a major issue over the handling of the partially vaccinated. This group needs to be handled as a third group. There are many reasons why they may be different from unvaccinated. This is certainly the case in NZ where we see partially vaccinated as having different (in fact greater) risk factors than unvaccinated. At the very least there should be some sensitivity analysis performed to explore this. I cannot see any convincing argument as to why these should be lumped together.

R: Our study focused on factors associated with coverage, which is the complete uptake of recommended vaccination. For this reason, the participants were dichotomised into 'fully immunised', which represents complete uptake and build towards coverage, and otherwise 'unimmunised'. Under the Strengths and Limitations section on Page 28 Line 5-13, we have acknowledged that those within 'unimmunised' could be either partly immunised or completely unimmunised. However, exploring the factors associated with these two is beyond the scope of this study. We share the sentiment that it is equally important, and therefore suggest for future research to address the question.

Comment: The text in the results section could be reduced by only describing the most significant findings, not everything in the tables.

R: We have tried but were unable to reduce any more words without affecting its transparency or clarity.

Comment: Household Wealth Index – Add a foot not to explain how this is derived.

R: Amended by explaining how it was derived on Page 8 Line 23.

Comment: Antenatal care - quantify 'some care'. This could possibly be broken down further to an ideal level of care and perhaps just one or two contacts? I suspect there is a wide spectrum.

R: Amended by quantifying 'some care' on Page 9 Line 7-9. In this study, antenatal care represented any pregnancy-related care provided by skilled health personnel or traditional birth attendants during the pregnancy. The participants were grouped into two: those whose mother received antenatal care during their pregnancy, irrespective of the type of provider and the number of contacts, and those who did not.

Comment: Be precise in reporting of variables. Report the age of the children in months not years.

R: Amended to report age of children in months not years on Page 13 Line 2.

Comment: "The mothers were 25-29 years old on average" Present this precisely. There are many examples of this throughout the results section. "Almost all", nearly half". Please change all of these to the exact numbers or proportions.

R: Kindly note that the exact numbers or proportions are provided in the table. The text were purposely made to be reader-friendly.

Discussion

Comment: The discussion is thorough and well considered.

Page 24. Did the authors find any evidence for Vaccine Hesitancy?

R: We believe we did not have enough information to conclude this.

Comment: Page 27. Strengths and limitations. Remove the statement about the computations taking a huge amount of time.

R: Statement removed.

Reviewer 2

Dr. Christina Poethko-Müller

Robert Koch-Institute, Germany

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

Please leave your comments for the authors below:

This is a well-conducted data analysis, yielding interesting results. The scientific contribution is useful also for the practice and immunization policy in developing countries. The paper is well-written and concise. There are, however, some minor issues, which should be modified prior to publication.

Comment 1) Please check reference [8].

http://www.who.int/immunization/monitoring_surveillance/global_immunization_data.pdf

The URL leads to a 2014 paper, most probably the statement derive from estimates based on data from 2008. Consequently the sentence "Despite this progress, vaccine preventable diseases are still responsible for 1.5 million child deaths each year.[8]" needs to be adapted.

R: We have checked the reference. The figure was derived not only from WHO/IVB 2008 data, but also from WHO 2012 estimates for measles and WHO cause-specific mortality 2000-2011 for the rest of the diseases. The reference was published just after these dates in June 2014.

Comment 2) Please specify the citation concerning the statement "... This highlights the fact that global coverage may hide variability between countries. It also suggests that the achievements are still fragile. Should this trend continue, the goals of providing universal immunisation for all children by 2020 and ending vaccine-preventable deaths by 2030 could not be achieved, and the cost of such failure would be close to 26 million child deaths.[5]" (p5 row12-14).

Search of the reference [5] (World Health Organisation. Global Vaccine Action Plan. Geneva: WHO Press) did not reveal data supporting this statement.

R: Please refer to Page 28 of the reference document. We have also amended our manuscript Page 4 Line 21 to better reflect the original statement.

Methods

Independent variables

Comment 3) Please specify the methodology that was applied to build the wealth index. Which amenities and assets were used for building the wealth index? How were the data aggregated? Please state a reference concerning the IDHS methodology/study protocol.

R: Household wealth index was constructed based on household amenities and assets (radio, television, refrigerator, bicycle, motorcycle, or car) and dwelling characteristics (electricity, flooring, roofing, water source, toilet facilities, and sleeping arrangements). Afterwards, the results were categorised into quintiles from poorest to richest. Manuscript amended by explaining how it was derived and aggregated starting from Page 8 Line 23. In-text citation added as well.

Results

Comment 4) Please elaborate on the data source of the child's immunization status: What is the proportion of health card/book based immunization information?

R: 86%. Amended accordingly on Page 7 Line 12.

Table 2 and table 3

Comment 5) Please describe the univariate analyses more clearly in the heading, i.e.: "...factors associated with being unimmunized"

R: We have amended the table headings and updated our Statistical Analysis sub section on Page 10 Line 7-8 to describe our analysis more clearly. However, we did not change our headings into 'unimmunised' because our unimmunised category consisted of not only children who were completely unimmunised but also those who were partly immunised. Changing the headings into 'unimmunised' might be misleading for readers who skim through our work.

Table 2

Comment 6) In table 2 the proportion of immunized versus unimmunized children is expressed as column percentage. Transparency and information about determinants would be improved by stating row percentages instead.

R: We appreciate the input but prefer to let it remain as it is. We referred to many similar studies that have used column percentage, such as recent works from India (Devasenapathy et al, 2016) and Mozambique (Lanaspa et al, 2015).

Comment 7) The following statements are not fully correct as wording and the stated p-value falsely imply that results derive from a statistical trend-test.

"The odds of being unimmunised increased as the child's age and birth order increased ($p < 0.000$).

"As the number of household members increased, the likelihood of a child to be unimmunised increased ($p < 0.000$)."

R: As stated on Page 10 Line 10, we did test of trends.

Comment 8) The following paragraph is not fully comprehensive and is not convincing. The question whether poverty acts as a mediator-variable concerning the association between urban/rural areas and immunization should be investigated by step-wise inclusion of variables in the multivariable model (first step include area level (urban/rural), second step include wealth index in addition and observe whether OR for area level is getting smaller). Investigation of poverty as a potential moderator necessitates the inclusion of interaction terms in your model. Both approaches are feasible with your data without information to distinguish between urban areas with higher socioeconomic status and the slums.

"..., our results revealed that children who lived in rural areas were less likely to be unimmunised. Although health services are better and more easily accessible in urban areas compared to rural areas,[29] this fact likely masks the extent of urban poverty.[31] Estimates suggest that one third of urban populations in developing countries are actually living in slums.[40] With limited access to health services and poor quality of life, it is certainly likely that urban children had higher odds of being unimmunised. Unfortunately, we lacked information to distinguish between urban areas with higher socioeconomic status and the slums. ..."

R: We understand that mediation is possible, but place of residence (urban-rural) was an independent factor. The fact that both place of residence and wealth index came out significant in our multivariate analysis confirmed this.

9) The statement of the following paragraph is plausible. However the citation of reference [41] is not fully sufficient as trends in Indonesia may differ from trends in Burkina. Please discuss health and immunization policy development in Indonesia which may support the given explanation for birth cohort effects.

“Our analysis revealed that children of older age groups were significantly more likely to be unimmunised compared to those in the youngest group. In other words, later birth years were associated with better immunisation coverage. It may indicate a positive trend of the immunisation programme performance over the years.[41] “

R: We have added discussion to better support our explanation, starting from Page 22 Line 24.

Reviewer 3

Christiana Rialine Titaley

Faculty of Medicine, Pattimura University, Ambon, Indonesia

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

Please leave your comments for the authors below

Title

Comment :After reviewing the manuscript, I think the analysis is more with children being “unimmunised” than “immunised”. Authors might want to reconsider the title.

R: Our main findings and discussion focused on immunisation coverage and its determinants. Furthermore, ‘unimmunised’ could refer to both partly immunised and completely unimmunised. The determinants of these two weren’t address in our analysis and they might be different. It might therefore be misleading to change the title into ‘unimmunised’.

Comment : Please include the source of data in the title.

R: Following the STROBE Statement, the title indicates the study design using commonly used term, while the data source is included in the abstract.

Strengths and limitations:

Comment: Again, after reviewing the manuscript, I think the analysis is more with children being “unimmunised” than “immunised”.

R: Addressed.

Comment : Authors mentioned that they could only look at the children nested within census block. How about nested into province?

R: We have reframed this point to better reflect the limitation. As discussed on Page 28 Line 1-12, the 2012 IDHS selected participants through a two-stage stratified sampling design. The primary sampling unit was the census block (CB) and the complete list of households in each CB became the basis for second-stage sampling. That being said, we should build a three-level model (i.e. children within households nested within CBs). However, there was no household identifier in the dataset as it may compromise the participants’ anonymity. Therefore, we could only build a two-level model (i.e. children nested within CBs), and it becomes a limitation of this study.

Background:

Comment: The background is too lengthy and could be made more concise.

R: We have deleted the first paragraph to make it more concise.

Comment : As mentioned earlier, please reconsider the use “unimmunised” in the objective statement.

R: Addressed.

Methods:

Comment : Please provide the in-text citation for IDHS 2012 data.

R: Amended accordingly starting from Page 6 Line 18.

Comment : I suggest authors to state clearly that this is a secondary analysis rather than a primary data used.

R: Amended accordingly on Page 6 Line 15.

Comment: I think, the final sample size used in the analysis should be stated clearly in the Methods section. By the end of “Data source” sub section, reader might think that information from 18,021 children was used in the analysis.

R: We have stated the final sample size in the Methods section, not under Data Source but under Statistical Analysis sub section on Page 9 Line 24.

Comment: How if women had more than one children under five? Did authors involve all of the under 5 children? Please explain this clearly to avoid confusion.

R: All of her children would have been included. As stated on Page 7 Line 5 of the manuscript, every child of each woman was included.

Comment: Please explain more detail about the groups of variables in Andersen’s Behavioural Health Model.

R: Amended accordingly on Page 8 Line 10-16.

Comment: For the household wealth index, how was this variable constructed?

R: Household wealth index was constructed based on household amenities and assets (radio, television, refrigerator, bicycle, motorcycle, or car) and dwelling characteristics (electricity, flooring, roofing, water source, toilet facilities, and sleeping arrangements). Afterwards the results were categorised into quintiles from poorest to richest. Manuscript amended accordingly on Page 8 Line 23.

Comment: For the definition of ANC authors mentioned “any care”. Any care provided by who? Similar to PNC. Any care provided by who? Does it include care provided by traditional birth attendants?

R: In this study, both ANC and PNC represented any care provided by skilled health personnel or traditional birth attendants, irrespective of the type of provider and the number of visits. Manuscript amended accordingly starting from Page 9 Line 7.

Comment : Under the statistical analysis authors mentioned about the sample size. If there were 13,745 cases completed why did authors use 14,041 children since authors mentioned they used “complete-case analysis”? If authors used 14,041 children, did the 656 included children died after their first birthday? Please explain clearly.

R: Yes, the 656 children had died prior to the survey. Following IDHS 2012 protocol, their immunisation status were not recorded in the dataset and treated as missing. Given the small amount of missing values (4.6%) we used complete-case analysis and no sensitivity analysis was required.

Comment: Did authors use complex sample survey analysis? Did the analysis weight the data for sampling probability? What are the command used by authors in Stata for the multilevel modelling? – this could be stated in the Methods Section

R: The IDHS 2012 used complex sampling and very robust, we have cited previously published literature on its quality. Further reading shows weighting adjustment was done to make sure estimates were reliable. For the analysis, we used multilevel modelling to take into account its two-stage stratified sampling design. Lastly, we have amended the manuscript to include the STATA command on Page 10 Line 16.

Comment: Did authors check for collinearity taking into account the large number of variables used in the analysis?

R: Yes, this was part of the univariate analysis and variable selection.

Comment: For the univariate analysis authors mentioned that they use univariate p value. How was this assessed for variables with more than two categories? Each category p value or combined p value?

R: We used each category P-value, presented in Table 2.

Results:

Comment: The results section is too lengthy. Could be made more concise.

R: We have tried but were unable to reduce any more words without affecting its transparency or clarity.

Comment: One of the variables used is the ownership of health insurance. What kind of health insurance? At that time, has a health insurance might imply children from poor family.

R: Insurance coverage represented any health insurance provided through social security or local government, by employer, privately-purchased, or other insurance. We have amended the manuscript accordingly on Page 9 Line 4-6.

Comment: Table 2 and 3 are more related to factors association with unimmunised status. Please revisit this.

R: We have amended the headings to better reflect our analysis.

Comment: For the multilevel logistic regression, I recommend authors to carefully select the reference group so that the results could be interpreted similarly, i.e. all are risk factors (aOR greater than 1) rather than some were risk factors and some were protective factors (aOR smaller than 1). E.g. it might be clearer to make rural category as the reference group instead of urban category as the reference group.

R: We appreciate the input, but changing the reference would not affect the conclusion as the results of the analysis would remain the same.

Comment: Authors has not mentioned the clustering level or the results of taking into account the role of environment.

R: The model adjust for this and we were focusing on the interpretation. These were covered in Statistical Analysis sub section.

Discussion:

Comment: Too lengthy and could be made more concise

R: Similar with Result section, we have tried but were unable to reduce any more words without affecting its transparency or clarity.

Comment: Overall, authors could suggest some interventions that could be conducted to improve the condition. Previous literature could be used to support authors' claim. For example, what could be suggested for areas with low coverage of immunisation, e.g. Maluku and Papua or urban children? What could be suggested for children of older age groups or high birth ordered children or children from large families? It is very important to address the issue of children not exposed to health system – not attending ANC, PNC or born at home.

R: Within Discussion section, we have addressed in detail what each of our finding indicated, and we have concluded possible interventions to help improve coverage in Indonesia. It should be noted, however, that the survey was cross-sectional, which was the reason why we have been conservative with suggesting interventions in the first place.

Comment: Line 34-36 needs a reference.

R: It was what our analysis indicated. We have cited references on the previous sentences to support the explanation of our findings.

How about the role of cluster/environment from the multilevel analyses- this has not been explained.

R: Addressed.

VERSION 2 – REVIEW

REVIEWER	Christiana Rialine Titaley Faculty of Medicine, Pattimura University, Ambon, Indonesia
REVIEW RETURNED	08-Aug-2017

GENERAL COMMENTS	<p>1. About the definition of ANC and PNC: ANC and PNC usually refer to health care provided by trained birth attendants, rather than traditional birth attendants. Or else authors might want to use a different term for what they have used. If they could restrict them to only care provide by trained personnel, the information could be easily found in the DHS dataset.</p> <p>2. About the reference group for the analysis, yes, I expect that it will not change any results or conclusions, however, it will be clearer to readers and might avoid any misinterpretation.</p>
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VERSION 2 – AUTHOR RESPONSE

1. About the definition of ANC and PNC: ANC and PNC usually refer to health care provided by trained birth attendants, rather than traditional birth attendants. Or else authors might want to use a different term for what they have used. If they could restrict them to only care provide by trained personnel, the information could be easily found in the DHS dataset.

Response (R): On Page 9 Line 6-9, we have stated that in this study "antenatal care represented any pregnancy-related care provided by skilled health personnel or traditional birth attendants during the pregnancy, irrespective of the type of provider and the number of visits". We have carefully defined each of our independent variables prior to the analysis and clearly presented them so readers would know exactly what they mean. We would therefore like to keep our working definition of antenatal care as it is.

2. About the reference group for the analysis, yes, I expect that it will not change any results or conclusions, however, it will be clearer to readers and might avoid any misinterpretation.

R: Similarly, we have clearly outlined our reference group for the readers. Since it will not change any results or conclusions, we would like to keep it as it is. We appreciate both comments and will leave the final decision to the editors.