Author's response to reviews

Title: Migration and Child Immunization in Nigeria: Individual- and community-level contexts

Authors:

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Author's response to reviews: see over
Response to reviewer’s comments

Reviewer’s report 1:

Title: Migration and Child Immunization in Nigeria: Individual- and community-level contexts

Version: 1 Date: 11 May 2009

Reviewer: Ari Mwachofi

Revisions

Discretionary Revisions

Please reorganize the paper focus statement so that when you make references to immunizations in the world, third world, Africa, Nigeria it is still clear that your focus is Nigeria. The way it is now gets a little confusing to the reader. Some statements are confusing – “risk of full immunization” do you mean “likelihood” of full immunization?

The paper has been re-written/re-organized as the reviewer indicates.

“Risk of full immunization” has been changed to “likelihood” of full immunization” throughout

the manuscript as the reviewer indicates.

Minor Essential Revisions

Please include a table that lists variables in the study, their definitions and summary statistics. Please re-organize the manuscript: Introduction that includes a short description of migration patterns in Nigeria to give the reader a proper context; background with statement of the problem and consequent research questions and hypotheses; Methods with Data sources; Theoretical framework with a description of variable selected for this study with and why they are considered to be appropriate.

Please state the study objectives more clearly.

A table that lists variables is given (see Table 2: Exposure variables used in modelling the association between migration status and the likelihood of full immunization) as the reviewer indicates.

The manuscript has been re-written/re-organized. The Introduction gives the reader a short description of the Nigerian context. The Background includes a statement of the problem and and consequent research questions and theoretical framework. A description of variables
selected for this study and why they are considered to be appropriate is highlighted in the Data and Methods. This is as the reviewer indicates.

The study objectives have been stated more clearly as the reviewer indicates (see page 5).

Major Compulsory Revisions

Discretionary Revisions

Consider simplifying your analysis and including at most two analysis models. I am not sure that five models add information that you could not provide with only two models.

The analyses and interpretation have been simplified. However the 5 models are needed to clearly test the theoretical framework and draw inferences as the reviewer indicates. Model 1 tests
the perspective of migrant disruption; models 2 & 3 test migrant selectivity; model 4 tests migrant selectivity; and model 5 assesses the effect of community-level characteristics.

1. Is the question posed by the authors well defined?
The question could have been better defined - i.e. a clearer statement of the problem, a clearer statement of the research question and researcher's hypothesis

   A clearer statement of the problem has been included in the Background as the reviewer indicates.

2. Are the methods appropriate and well described?
The methods are appropriate but not well described.

   The methods are well described.

3. Are the data sound?
Yes – except – it is not clear how the variable representing wealth/income was extrapolated

   An adequate description of how the variable representing wealth/income was extrapolated has been included under socio-economic characteristics (see “wealth index” on pages 6 & 7) as the reviewer indicates.
4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
A table of variables used in the study that includes definitions and summary statistics would be very useful and would make it easier for the reader to follow the methods description.

   A table that lists variables and their definitions is given (see Table 2: Exposure variables used in modelling the association between migration status and the likelihood of full immunization) as the reviewer indicates.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
Yes

6. Are limitations of the work clearly stated?
No

   The strengths and limitations of the study have been included as the reviewer indicates (see 3rd paragraph on page 14).

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
Not clear

   References have been made to publications related to the findings in this study as the reviewer indicates. Only one other study (See reference 13. Kirosa G, White MJ: Migration, community context, and child immunization in Ethiopia. Soc Sci Med 2004, 59:2603–2616) has focused on migration and child immunization, and that was in Ethiopia. This study is therefore the second such study and the first in Nigeria.
8. Do the title and abstract accurately convey what has been found?
Yes

9. Is the writing acceptable?
There are several incomplete or complex sentences making reading a bit of a challenge.
The paper has been re-written/re-organized.

**Level of interest:** An article of importance in its field
**Quality of written English:** Needs some language corrections before being published
**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Reviewer's report 2**
**Title:** Migration and Child Immunization in Nigeria: Individual- and community-level contexts
**Version:** 1  **Date:** 14 May 2009  
**Reviewer:** Jean-Christophe Fotso

The paper explores a subject of great interest using state-of-the-art methodologies and nationally representative DHS data. The analytical strategy and the interpretation of results are commendable. The author needs to address the following issues for the manuscript to be accepted for publication.

**Major Compulsory Revisions**
Background/Introduction: There is poor linkage or even confusion between the background and the introduction sections. First, a manuscript will typically start from the general to the particular (in this case, Immunization in Nigeria). Second there are some inconsistencies in the background, one example being the 6th line in the background section (global immunization coverage exceeding 70% in the 1990s). Third, the “high political will of the government” is mentioned (last but one line on page 1) without any illustration or evidence. Fourth, the last paragraph on page 2 seems problematic: What does the first sentence (Rapid urbanization … could be
explained) mean? The author mentions the need to assess changes in urban-rural differentials, yet the paper does not address the issue.

The theoretical framework, though relevant, does not seem to be the basis of the analysis: What variables will capture selectivity, disruption and adaptation.

Overall, the paper will gain a great deal of clarity and focus if this section is re-written in a more coherent and articulated way.

1a. The paper has been re-written/re-organized. In the Introduction, a general description of immunization globally and in Nigeria has been included as the reviewer indicates.

1b. Inconsistencies in the background have been corrected. The sentence in question has been deleted as the reviewer indicates.

1c. The sentence “high political will of the government” mentioned (last but one line on page 1) without any illustration or evidence has been deleted.

1d. The paper has been re-written/re-organized. The last paragraph on page 2 “Rapid urbanization ……” As mentioned by the reviewer has been deleted.

The theoretical framework is the basis of the analysis! The variables capturing selectivity, disruption and adaptation have now been clearly specified (See “theoretical framework on page 4) as the reviewer indicates.

Data/Methods:

Too much detail given on data collection of the Nigeria DHS, most of this information is not necessary as it is already in the public domain. Without any inclusion of the length of stay of migrants, the results of the analysis may not be convincing. A mother who migrated 9 years ago to a city has probably had chance to rebuild her network, while a recent migrant mother would face the problems of adaptation and disruption highlighted in the theoretical framework.

The author fails to justify the selection of the community-level covariates: Why are the percentages of hospital delivery, prenatal care by doctor important as determinant of child immunization?

The data collection has been significantly shortened. Referral has been made to where a detailed
description of the data collection methodology could be found (See “Data and Methods” on page 5) as the reviewer indicates.

Justification of the selection of the community-level covariates has been provided (See last paragraph under “Community-level explanatory factors” on page 7 for justification for selecting

the community-level covariates e.g. Prenatal care directly increases the chances that the mother

accesses subsequent health care services for her child, such as institutional delivery and immunization [25, 26]. Therefore, the proportion of mothers that delivered in a hospital setting is

a predictor of child immunization uptake. Hospital delivery is one of the most important preventive measures against maternal and child health outcomes, and an important determinant

of full immunization [27] [28]; and iii) community mother’s education, defined as the percentage

of mothers with secondary or higher education within the PSU, and categorized as: low and high.

Higher levels of maternal education are associated with better child health outcomes, such as child immunization rates [29, 30]).

Results:

Referring to 58.6% for urban non-migrant (in table 1) as “higher proportion” while 73.1% of rural non-migrant’s children had BCG may not be correct. More generally, the fact that author does not describe and comment the higher likelihood of rural non-migrant’s children to be immunized, but only focus on comparing the urban non-migrant with the rural-urban migrant seems to be a

major flaw in the interpretation of the results. Table 2 should just be a footnote of Table 3.

In the full sentence, which reads “A higher proportion of urban non-migrant children had received BCG (58.6%), and OPV 1 (62.3%)”. The author was referring only to urban non-
migrants. Reference has been made to the higher likelihood of rural non-migrant’s children to be
immunized as the reviewer indicates.

Minor Essential Revisions
The writing style may be improved; there a number of repetitions which may be avoided without
loss of clarity.

The paper has been re-written/re-organized as the reviewer indicates.

Discretionary Revisions
None

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests

Reviewer's report 3
Reviewer: Stella Babalola
Title: Migration and Child Immunization in Nigeria: Individual- and community-level contexts
Version: 1 Date: 6 June 2009
Reviewer: Stella Babalola
The manuscript addresses an important health topic with potentially relevant implications for improving community health. However, there are some points that should be addressed to make the paper publishable.

1. Some of the references cited to support claims about the current state of immunization are older than ten years old (e.g. references 1 and 2). Things have not remained static and newer evidence is available in literature.

   The paper has been re-written/re-organized. The references in question have been deleted and more current ones cited as the reviewer indicates.

2. The text needs thorough editing: some sentences are incomplete and there are significant typographical errors;

   The text in this manuscript has been re-written/re-organized as the reviewer indicates.

3. It is not clear how the authors arrived at eight full complement of vaccinations. Counting the various occurrences of each type of vaccine gives 9 (1 time for BCG, 3 times for DPT, 4 times for OPV and 1 time for measles);

   The author begs to disagree! According to the World Health organization (WHO), full immunization is made up of 8 vaccinations! (Please see references 23 & 24 below).


4. Theoretical framework:
a. Description of the migrant disruption hypothesis is not clear. The authors mentioned disruption of demographic processes and then give mother’s network of social and financial support as an example.

Theoretical framework is the basis of the analysis. The variables capturing selectivity, disruption and adaptation have now been clearly specified as the reviewer indicates. (See “theoretical framework on page 4).

b. It is not clear how the three concepts (disruption, selection and adaptation will be operationalized.

The theoretical framework is the basis of the analysis! The variables capturing selectivity, disruption and adaptation have now been clearly specified as the reviewer indicates (See “theoretical framework on page 4).

5. Data and methods:

a. One wonders what motivated the choice of various explanatory factors. Based on literature? Determined through evidence from preliminary analyses?

The choice of various explanatory factors was motivated by the theoretical framework, which focuses on the different migration perspectives: migrant disruption, selectivity, and adaptation (please see “Theoretical framework”).

b. It is not clear how the authors combine birth order and birth interval. The authors mentioned birth order 2 – 4. What exactly does this mean?

Explanations about the merging of birth order and birth interval are given in the 2nd paragraph under “Individual-level explanatory factors” on page 6 i.e. “The variable ‘preceding birth interval’ is the interval before the birth of the child in question. As such, the effect of the preceding birth interval is considered in relation to the younger of the two children. Ideally, first births are left
out of the analysis of preceding birth interval and survival of the preceding child because they are
not preceded by another birth. In order to enable the inclusion of first births in the analysis,
first
births in this study have been merged with those with a preceding birth interval of 24 months or
longer”.
- “birth order 2-4 with short birth interval (<24 months)” means 2nd to 4th birth order with a
short birth interval of less than 24 months.

Which birth interval is relevant here?
All the birth intervals are relevant here.

How about birth order 1 – 2?
“Birth order 1” is of course First birth.

c. The inclusion of birth interval as a predictor implies that women with only one live birth are
excluded from the analyses. Is that correct? Not sure why the authors decided to adopt this
approach;
Not correct! Women with only one live birth are NOT excluded from the analyses. This is the
exact reason for merging birth order and birth interval! (Please see explanations about the
merging of birth order and birth interval given either above or in the 2nd paragraph under
“Individual-level explanatory factors” on page 6).

d. The age groupings are not equal. It is not clear why the authors decide to use these groupings
as opposed to the more conventional ones;
Mother’s age was grouped as: 15-18, 19-23, 24-28, 29-33, and 34 years and older. The DHS
survey only includes women 15 – 49 years. Apart from the age groups 15-18, which is a 4-
year
interval, and 34 years and older, the age groups 19-23, 24-28, and 29-33 are made up of 5-
year
intervals. This grouping has been used in hundreds of studies.

e. The community contextual variables are actually compositional variables aggregated upwards. There are two potential problems with this approach:

i. The same variables that were used to derive the “contextual” variables were also included as individual variables. This could result in multi-collinearity. Using the non-self mean might lessen this problem;

ii. The approach is subject to atomistic fallacy or the problem of making inference at a higher level with data collected at a lower level;

iii. At a minimum, the authors should mention these problems as limitations;

   The author couldn’t disagree more! The simultaneous inclusion of both individual- and neighbourhood-level predictors in regression equations with individuals as the units of analysis,

   permits: i) the examination of neighbourhood or area effects after individual-level confounders

   have been controlled; ii) the examination of individual-level characteristics as modifiers of the area effect (and vice versa); and iii) the simultaneous examination of within- and between neighbourhood variability in outcomes, and of the extent to which between-neighbourhood variation is “explained” by individual- and neighbourhood-level characteristics. (Please see last paragraph under “Community-level explanatory factors” on page 8, and references 31 & 32 below).


6. Statistical analysis:

a. One wonders why the authors did not consider using incremental models (for models 3 to 5).

   The analyses and interpretation have been simplified as the reviewer indicates.
However the 5 models are needed to clearly test the theoretical framework and draw inferences as the reviewer indicates. Model 1 tests the perspective of migrant disruption; models 2 & 3 test migrant selectivity; model 4 tests migrant selectivity; and model 5 assesses the effect of community-level characteristics. Incremental models would be testing the association between each individual variable and the likelihood of full immunization. This is not the aim of the study. The study aimed to examine the effects of individual- and community-level characteristics of migrant groups (i.e. migrant disruption, selectivity, adaptation perspectives, and community characteristics) on the likelihood of their full immunization uptake.

7. Results:

a. It is somewhat unusual to see full immunization described as a risk; “Risk of full immunization” has been changed to “likelihood” of full immunization” throughout the manuscript as the reviewer indicates.

b. The way the authors explain mother and community-level heterogeneity in child immunization is awkward. For example, a significant variance of 13.2% at the mother’s level (model 2) indicates that the variance in immunization rate across mothers remain significant even after controlling for the variables included in the model. If the authors had calculated the residual intra-class correlation (ICC), it would have should what proportion of the variance in childhood immunization was due to (unmeasured) factors operating at the mother’s level;

The analyses and interpretation have been simplified as the reviewer indicates. (Please see “Measures of association (fixed effects)” and “Measures of variation (random effects)” on page 9, as well as the results section.)
8. It is not clear how the data support some of the points made in the discussion. For example, in some models, the study found that the children of rural non-migrants were more likely to be immunized than children born to rural non-migrant mothers. The authors then concluded that migrant disruption explains the findings. This is not convincing. Similarly, the fact that demographic and socio-economic variables were significant predictors of childhood immunization was interpreted as pointing to migrant selectivity as an explanation although it is not clear how many of measured socio-economic and demographic variables actually preceded the migration experience.

   Please see the theoretical framework, which is the basis of the analysis. The variables capturing selectivity, disruption and adaptation have now been clearly specified (See “theoretical framework on page 4).

9. The conclusion does not appear to derive from the findings. These revisions are essential and should be made before the manuscript can be published.

   The conclusion has been revised as the reviewer indicates

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I declare that I have no competing interests