

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Food insecurity and the risk of HIV acquisition: Findings from population-based surveys in six sub-Saharan African countries (2016-2017)
AUTHORS	Low, Andrea; Gummerson, Elizabeth; Schwitters, Ameer; Bonifacio, Rogerio; Teferi, Mekleet; Mutenda, Nicholas; Ayton, Sarah; Juma, James; Ahpoe, Claudia; Ginindza, Choice; Patel, Hetal; Biraro, Samuel; Sachathep, Karam; Hakim, Avi J; Barradas, Danielle; Hassani, Ahmed Saadani; Kirungi, Willford; Jackson, Keisha; Goeke, Leah; Philips, Neena; Mulenga, Lloyd; Ward, Jennifer; Hong, Steven; Rutherford, George; Findley, Sally

VERSION 1 – REVIEW

REVIEWER	Frongillo, EA University of South Carolina, Department of Health Promotion, Education, and Behavior
REVIEW RETURNED	18-Dec-2021

GENERAL COMMENTS	<p>1. The Introduction is well-written, but the statement of the aim of the study is weak. No scientific aims are stated. Exploring relationships is an activity, not a scientific aim. Please articulate clear scientific aims (at least one) that reflects the intention of the authors in doing the study. The authors are selling themselves short by not articulating aims that are linked to the issues raised earlier in the Introduction and then tested by the data; apparently there were such aims so please state them.</p> <p>2. Regarding the assessment of food insecurity, several issues need resolution. 1) Provide a citation for the HFIAS. 2) The scale measures access to food, not the indicator guide. 3) Was the whole scale assessed or was it just item 7 in the guide? If the whole scale, then why was only one item used? 4) Either way, what were the response options? Food insecurity is the exposure variable for this study, and the information provided about how it was assessed is highly inadequate.</p> <p>3. When were the data collected? That information is not provided other than it was before 2018.</p> <p>4. The information in the first paragraph of the Statistical Analysis section is inadequate to understand how the complex sampling was handled. Given that two-stage sampling was done, how was the clustering handled? What were the primary sampling units? Given that data from multiple countries were combined apparently, nothing explicit is said about how that was done and especially how the weights were combined. Were the weights in each country normed so that each country contributed equally? If not, what was</p>
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	<p>done to ensure that the weights being thrown together were meaningful? The statement “with Taylor series weighting for variance estimation” does not make sense. Taylor series approximation is a way to estimate the variability due to all aspects of the complex sampling; there is no such thing as “Taylor series weighting.”</p> <p>5. Please clarify in the title whether Table 2 reports bivariate relationships or multivariable relationships.</p> <p>6. On p. 15, lines 14-17, the statement refers to univariate analyses (whatever that means—should be bivariate) but then links to Table 3 which presents multivariable analyses.</p> <p>7. The Conclusions section says nothing about this study. Everything said there could have been written before the study was done and so is not a conclusion of this study.</p>
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REVIEWER	Bekun, Festus Eastern Mediterranean University, Economics and finance
REVIEW RETURNED	09-Jan-2022

GENERAL COMMENTS	<p>The topic is interesting, and the author has done a great job in realizing the subject. However, there are few areas on the paper that is still lagging and should be addressed properly.</p> <p>Abstract</p> <ol style="list-style-type: none"> 1. The authors should motivate the choice of variables 2. Keywords should be revised to match key element of title not title in itself <p>Introduction</p> <ol style="list-style-type: none"> 1. The objective of the paper presented need more clarifications to suit reader to understand the main idea of the paper. <p>Literature review</p> <p>The literature is well written. However, there is need for more recent studies ranging from 2018-2021 to motivate the study properly. The entire study is too scanty and the related literature is not exhausted</p> <p>See suggestions although not limited to these</p> <p>Poverty and agriculture in Southern Africa revisited: a panel causality perspective. SAGE Open, 9(1), 2158244019828853.</p> <p>Does life expectancy, death rate and public health expenditure matter in sustaining economic growth under COVID-19: Empirical evidence from Nigeria?. Journal of Public Affairs, 21(4), e2302</p> <p>Methodology</p> <ol style="list-style-type: none"> 1. The variables used in the model should be justified 2. More benefit of the various techniques utilized should be stated. And if possible, their equations should be added to the revised manuscript to enrich the quality. <p>Discussion</p> <ol style="list-style-type: none"> 1. The discussion is well written, but the authors should like their findings to the previous studies in the literature. <p>Conclusion</p> <ol style="list-style-type: none"> 1. The sub-title should be conclusion and policy recommendation but not only conclusion 2. The policy which is the engine of the study is weak and small. I therefore encourage the authors to elaborate more on the policy recommendations to policy makers for the investigated bloc 3. The authors should add limitation of the study and future recommendation
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REVIEWER	Akintunde, Tosin Hohai University, Sociology
REVIEW RETURNED	20-Jan-2022
GENERAL COMMENTS	The discussion ended abruptly without research implications for intervention and future research direction. While the study identified vulnerabilities, the authors should provide a robust and tentative solution to support their findings.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Prof. EA Frongillo, University of South Carolina Comments to the Author:

1. The Introduction is well-written, but the statement of the aim of the study is weak. No scientific aims are stated. Exploring relationships is an activity, not a scientific aim. Please articulate clear scientific aims (at least one) that reflects the intention of the authors in doing the study. The authors are selling themselves short by not articulating aims that are linked to the issues raised earlier in the Introduction and then tested by the data; apparently there were such aims so please state them.

Response: We thank the reviewer for the excellent suggestion. We recognize that the we didn't flesh out the gaps in the literature enough to justify our aim and therefore have made two changes to the introduction, with the additions in italics:

"The HIV pandemic *has been posited* to have a bidirectional link to food insecurity, as the associated health consequences can drive lower productivity and decreased labor mobility, whereas food insecurity can increase HIV risk behaviors, *and has been linked to prevalent infection*. It also drives disruptions in care and higher mortality."

"These surveys provide a unique opportunity to assess the relationship between food insecurity and HIV incidence in a large representative cohort of individuals, *and thus address the gap in the literature directly linking food shortages to HIV acquisition*. "

2. Regarding the assessment of food insecurity, several issues need resolution. 1) Provide a citation for the HFIAS. 2) The scale measures access to food, not the indicator guide. 3) Was the whole scale assessed or was it just item 7 in the guide? If the whole scale, then why was only one item used?

Response: 1. We provide the reference for the HFIAS in the main text: Citation: Ballard T, Coates J, Swindale A, et al. Household Hunger Scale: indicator definition and measurement guide. In: Food and Nutrition Technical Assistance II Project, ed. Washington, DC,: FHI360, 2011.

2. As per the above guide on pages 3 and 4, the questions that we used, including the question about food in the house, capture insufficient food supply and intake, and hence act as indicators of food insecurity.

3. Please see the response below for further details on the use of the scale vs the individual questions.

4) Either way, what were the response options? Food insecurity is the exposure variable for this study, and the information provided about how it was assessed is highly inadequate.

Thank you for your suggestions- we are grateful for your expertise in this subject matter. We have added more detail in the text and, in particular, in the appendix on how we constructed our hunger measurement and why we chose the indicator rather than the scale measure. This is something we discussed at length during the analysis stage and ultimately decided that the indicator

made more sense for our analysis. The primary reasons we preferred it is that our analysis of HIV risk is at the individual level rather than the household level and we ultimately concluded that having no food in the house was a more reliable reflection of individual experience of hunger versus having someone (maybe you- or maybe someone else) in your household experience hunger. We also had a slight preference for the directly observable indicator when comparing across multiple country contexts.

We provide the questions used, the response options and further information on how we calculated our exposure variable in the appendix. We have added a reference to the appendix to the methods: "Further variable descriptions, including the questions used and the construction of our exposure, are provided in the Appendix, p.6."

We also provide information on how we calculated our specific exposure variable in the text of the methods: "We defined severe food insecurity as a household having no food in the house at least three times in the past four weeks."

Finally, as there are many ways to measure and define food insecurity, we also constructed the Household Hunger Scale and ran the model analyzing HIV risk using it as the exposure variable (appendix p.9). In the appendix we discuss similarities and differences in the results between the use of the scale vs the use of the question about food availability in the house: "The trends in recent infection in women that were observed using the food in the house indicator variable persisted, but, interestingly, the power was reduced, in large part because the number of participants classified as severely hungry was considerably smaller with the scale measure. This might reflect that the question on whether there was no food in the house is relatively objective, whereas the questions on hunger and going a whole day and night without eating are answered by the head of household for all members of the household, and therefore might not accurately reflect individual household members' experiences with hunger."

We reference this analysis in the results: "The use of a scale of food insecurity incorporating the three questions on food availability and access, did not produce substantially different results than our measure [(Supplementary Table 1)], although fewer people were classified as severely food insufficient. This is further discussed in the appendix [(p 9)]."

We believe that this is a thorough description of the calculation of our exposure and how the use of a scale produces a slightly different result, but that the highly specialized nature of this discussion is better suited for the appendix rather than the main text. However, we are willing to move it into the methods and results if the editors think that it is more appropriate.

3. When were the data collected? That information is not provided other than it was before 2018.

Response: We have clarified the timing of data collection in the methods:

"We used data from all PHIA surveys collecting data on household food availability between 2015 and 2018 (Eswatini, Lesotho, Namibia, Tanzania, Uganda, and Zambia, see Supplementary Figure 5 for precise timing of data collection, appendix p.5)"

4. The information in the first paragraph of the Statistical Analysis section is inadequate to understand how the complex sampling was handled. Given that two-stage sampling was done, how was the clustering handled? What were the primary sampling units? Given that data from multiple countries were combined apparently, nothing explicit is said about how that was done and especially how the weights were combined. Were the weights in each county normed so that each country contributed equally? If not, what was done to ensure that the weights being thrown together were meaningful? The statement "with Taylor series weighting for variance estimation" does not make

sense. Taylor series approximation is a way to estimate the variability due to all aspects of the complex sampling; there is no such thing as “Taylor series weighting.”

Response: We have added additional detail on the calculation of the sampling weights, as well as how they were used in the pooled data analyses. The weights are designed to adjust for survey design, non-response and selection probability at the country level (to make each country’s sample representative of the country population). In the pooled data analyses, the use of the underlying study sampling weights therefore allowed each country to be self-representing for its population size.

5. Please clarify in the title whether Table 2 reports bivariate relationships or multivariable relationships.

Response: Table 2 refers to both univariable and multivariable logistic regression analyses. We have changed the title to reflect the style of Table 3.

6. On p. 15, lines 14-17, the statement refers to univariate analyses (whatever that means—should be bivariate) but then links to Table 3 which presents multivariable analyses.

Response: We have changed the title of table 3 to remove the multivariable term as it presents both univariable and multivariable analyses. Univariable analyses refers to the analysis of one dependent and one independent variable, whereas multivariable refers to one dependent variable and multiple independent variables.

7. The Conclusions section says nothing about this study. Everything said there could have been written before the study was done and so is not a conclusion of this study.

Response: The relationship between acute food insecurity and HIV acquisition is in fact a new finding of this study, as is the scale of our evidence for the bidirectional nature of HIV infection and food insecurity. In addition, the finding that direct food support can have a mediating impact on the relationship whereas more general economic support does not is also a new finding which we feel is important for setting policy. These findings guided the writing of our conclusions. However, in light of this suggestion and other reviewers’ comments, we have added further recommendations specific tHIV prevention and food insecurity, and the potential impact of climate change.

Reviewer: 2

Dr. Festus Bekun, Eastern Mediterranean University Comments to the Author:

The topic is interesting, and the author has done a great job in realizing the subject. However, there are few areas on the paper that is still lagging and should be addressed properly.

Abstract

1. The authors should motivate the choice of variables

Response: Thank you. In response to your and other reviewer’s comments, we have added further details to the methods on the selection of the variables, referring to the theoretical framework guiding this analysis, which we included in the appendix:

“Exposure variables were selected on the basis of our theoretical framework (appendix p.2) either as potential causes of severe food insecurity, or as potential drivers or mediators of the relationship between severe food insecurity and HIV acquisition.”

2. Keywords should be revised to match key element of title not title in itself

Response: We have changed the key word HIV acquisition to HIV risk. Food insecurity is a MeSH term with a clear definition and is important for people to identify this paper. The other terms are not in the title.

Introduction

1. The objective of the paper presented need more clarifications to suit reader to understand the main idea of the paper.

Response: We acknowledge that the paper needed clarification of our primary aim or objective. Please see the response to the similar point made by Reviewer 1.

Literature review

The literature is well written. However, there is need for more recent studies ranging from 2018-2021 to motivate the study properly. The entire study is too scanty and the related literature is not exhausted See suggestions although not limited to these Poverty and agriculture in Southern Africa revisited: a panel causality perspective. SAGE Open, 9(1), 2158244019828853.

Does life expectancy, death rate and public health expenditure matter in sustaining economic growth under COVID-19: Empirical evidence from Nigeria?. Journal of Public Affairs, 21(4), e2302

Response: Thank you for the suggestion. We conducted an extensive literature review on the relationship between HIV and food insecurity while developing the analytic plan for this study. We have also updated it during this revision, including with the suggested reference on poverty and agriculture in Southern Africa. However, we did want to note that, while the relationship between poverty and food insecurity has been well documented, we focus here on the bi-directionality of the relationship between severe food insecurity and HIV infection, particularly on food insecurity and risk of HIV acquisition. This focus informed our literature review and our selection of references.

Methodology

1. The variables used in the model should be justified

Response: We have made the suggested change. Please see our response above.

2. More benefit of the various techniques utilized should be stated. And if possible, their equations should be added to the revised manuscript to enrich the quality.

Response: We agree that a more detailed discussion of our methods was called for and have added more on this in the paper in the design and analysis sections. We have also reorganized the methods section to make the analysis section easier to follow.

As this paper is aimed towards a general audience, and the models are standard and commonly used, we do not think that the addition of equations describing the logistic or Poisson models would be of much benefit.

Discussion

1. The discussion is well written, but the authors should like their findings to the previous studies in the literature.

Response: Thank you. We have cited the relevant literature here or in the introduction, as well as in the theoretical framework presented in the Appendix. Based on your suggestion, we have also added a few newer references from our recent literature review.

Response:

Conclusion

1. The sub-title should be conclusion and policy recommendation but not only conclusion

Response: While it is true that our conclusion includes policy recommendations as well, in accordance with the style guidelines of BMJ, we believe that it is more appropriate to use Conclusions as the subheading.

2. The policy which is the engine of the study is weak and small. I therefore encourage the authors to elaborate more on the policy recommendations to policy makers for the investigated bloc

Response: We have reorganized the conclusions section to highlight the importance of different policy/programmatic responses that can reduce both severe food insecurity and the risk of HIV infection among women experiencing SFI. This revision more clearly highlights the bi-directionality of the food insecurity-HIV association, the behavioral patterns underpinning the association and the role of food supports to women to reduce HIV risk.

3. The authors should add limitation of the study and future recommendation

Response: We had already provided a paragraph on study limitations but have added the subheading to make this easier to recognize. In terms of future recommendations, we have enhanced the conclusion as detailed below:

“In conclusion, in this time of global economic disruption and stark increases in food insecurity,^{8 39} it is critical to track the emergency of food shortages in communities the communities most vulnerable to the devastating effects of climate change. Our study suggests that in addition to population-level emergency food assistance, women, particularly those heading their own households, need to be specifically targeted with food assistance to alleviate their severe food insecurity. At the same time, these women can be linked to support services that will help them reduce their risk of HIV infection. Beyond these acute responses, programs need to follow-up with support for economic empowerment of unmarried women, so that they can support themselves and their children without relying on high-risk sexual activity in their times of greatest need. Thus, investments in community resilience need to ensure that these women are fully integrated into the programs, whether in the agricultural sector or in the market or service economy. “

AND

“The international recognition that food support prevents conflict is heartening, but global donors must also consider other consequences of hunger, including the risks to HIV epidemic control in communities with ongoing high incidence compounded by poverty and food shortages. Future research should further evaluate how current biomedical prevention modalities such as pre-exposure prophylaxis (PrEP), and structural interventions such as educational support targeted towards young women might disrupt this pathway. In light of evidence suggesting that the age at acquisition is shifting upwards,⁴⁰ re-evaluation of prevention programmatic age targets is recommended in order to support women throughout their life span. Understanding that food insecurity has both short and long-term consequences, including HIV transmission, should spur further investments in preparedness, including in crop and system resilience, and environmental justice.”

Reviewer: 3

Dr. Tosin Akintunde, Hohai University

Comments to the Author:

The discussion ended abruptly without research implications for intervention and future research direction. While the study identified vulnerabilities, the authors should provide a robust and tentative solution to support their findings.

Response: Thank you. In response to similar comments from the other two reviewers, we have considerably expanded the discussion section. Please see the additional text in the conclusions and future research sections of the paper for a fuller discussion of implications of our findings.

VERSION 2 – REVIEW

REVIEWER	Frongillo, EA University of South Carolina, Department of Health Promotion, Education, and Behavior
REVIEW RETURNED	20-Apr-2022

GENERAL COMMENTS	No further comments.
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REVIEWER	Bekun, Festus Eastern Mediterranean University, Economics and finance
REVIEW RETURNED	24-Mar-2022

GENERAL COMMENTS	current version reads well and suitable for journal audience
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