Subject: The World Bank Economic Review - Decision on Manuscript ID WBER-2023-107
Date: Wednesday, August 9, 2023 at 13:47:23 Pacific Daylight Time
From: The World Bank Economic Review
To: Claus Portner
Attachments: WBER_2023-107_Ref.1.pdf

09-Aug-2023

Dear Professor Portner,

Thank you for submitting manuscript # WBER-2023-107 entitled "Impact of Twin Lockdowns on Hunger, Labor Market Outcomes, and Household Coping Mechanisms: Evidence from Uganda" to The World Bank Economic Review. There are a lot of nice elements in your manuscript, and I am eager to see a revision.

Your referees raise many constructive points for your revision. Your referees are leaders in this field, and I think their comments reflect how many will respond to your work. Please consider their feedback carefully. I think the following issues are especially important to address:

1 / The paper is just reporting 3 out of a possible 7 survey round effects and interpreting those as lockdown effects. I don't understand that. Why not just report all 7 survey round effects (graphically) and think of the paper as trying to understand why the survey round effects vary.

2/ What are the household fixed effects doing? In a world where the household's latent characteristics associated with food security are independent of what survey round they are interviewed in, they would have no impact on the survey round effects. Hence, the household fixed effects are meant to improve efficiency or address sample selection issues. That should be explained and justified. I don't see a "bias" argument unless you have a sample selection problem, and I cannot imagine that type of problem is time invariant.

3/ Beyond the survey effects, you have lockdown measures that depend on the household's survey date interacted with some disaggregated geography. These results (in Tables 6 and 7) could be more fully exploited to help explain your survey round effects. Namely, you could end up with "time to severe lockdown", "time from end of severe lockdown", "in severe lockdown" and control for survey round effects at the same time (depending on the distribution dates and geographic heterogeneity, of course). I think this type of mediation style analysis would be more informative than current Table 6 & 7 which I do not know how to compare to earlier tables.

4/ Attrition is large. Is it correlated with food security? It seems like it will be correlated with survey round. I think it might be useful to end with a bounding exercise where you assume attriters are at extremes of the food security distribution. Your end results would be insignificant, but the range of your survey year effects with different assumptions on attrition would provide the reader of a sense of how sensitive your analysis is to attrition. I feel comfortable pre-committing that your paper will not be rejected for attrition so long as you are clear on how

sensitive your results are to different assumptions about attriters, even if the answer is "super sensitive".

5/ Why isn't the 8th wave of the NPS used as the baseline? To me, that really influences the interpretation of your counterfactuals to your survey round effects. You don't need to add this additional data, but you should explain to your reader why you do not.

6/ Cases (i,t). This is the number of national cases in the 30 days before the survey date? What do we think of that as measure of local covid exposure?

7/ Given the clustered survey design of the NPS, you should be clustering by PSU throughout (probably only meaningful in the mediation analysis discussed above).

As you may know, the WBER is an all-purpose development journal. We currently have an acceptance rate below 5 percent. I would not ask for a revision if I did not think that you could resubmit a paper suitable for acceptance, but I am more doubtful on this paper than on the average accepted paper as I don't really understand why a paper that reports three survey round effects is informative of the impact of COVID lockdowns.
In your resubmission, please provide a brief explanation of how you have responded to the issues I highlighted as "especially important to address" in addition to any other substantive changes you have made in this resubmission. In your letter to me, please do not cut and paste text from the paper. I will reread the paper in your resubmission, and I want your cover letter to be a quick preview of how you have reacted to the issues I highlight.

I also expect to send your paper back to referees, so please provide a response to each referee.

Please resubmit your paper double-spaced with twelve-point Times New Roman or similar font and one-inch margins. These formatting guidelines apply to everything for consideration for print, including the text, tables, figures, references, and footnotes. Each table/figure should be on its own page and separate from the text. Everything for print cannot exceed 40 pages in total when formatted as above.

As a reminder, WBER requires that accepted papers include sufficient information on data sources and methods that work can be replicated by other researchers. This requirement is normally met through online posting on the journal website of a file that includes archival summary, data and relevant computational files. If data are proprietary or the requirement otherwise cannot be met, you should have already noted this in your submission, and I encourage you to let us know of any issues in this regard before your resubmission.

I look forward to seeing your resubmission in the near future.

Sincerely,
Dr Eric Edmonds
The World Bank Economic Review

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Because we are trying to facilitate timely publication of manuscripts submitted to the The World Bank Economic Review, your revised manuscript should be uploaded within 6 months from today. If it is not possible for you to submit your revision within that time frame, please contact us to request an extension.

Once again, thank you for submitting your manuscript to the The World Bank Economic Review and I look forward to receiving your revision.

Reviewers' Comments to Author:

Referee: 1

Comments to the Author
Thank you for the opportunity to read the paper. My comments are contained in the referee report provided.

Referee: 2

Comments to the Author
Summary

The paper uses a household fixed effects model on a national sample to estimate the effects of Uganda's two strict lockdowns in response to COVID-19. They compare the end of the first lockdown and the period of the second to the interim period between the two lockdowns, almost "holding fixed" the COVID-19 crisis to narrow focus to the lockdowns. The authors show significant increases in food insecurity from the stringency of the lockdowns, and document

potential changes faced by households likely to contribute.

Contributions

The use of panel data allows the authors to further develop the literature on the effects of COVID-19 on households in developing countries.

The authors document significant increases in food insecurity that seem very plausibly to be due to Uganda's (unusually strict) lockdowns; the comparison to between lockdown periods (though done based on data availability) strengthens this attribution.

The rich, high-frequency data allows the authors to look at a variety of potential coping mechanisms, and to document a broad range of (potential) responses to shocks.

The authors compellingly ameliorate concerns that about seasonality driving their results.

Essential Points

1. The authors are actually comparing lockdowns to other periods during COVID-19, as the data is first gathered in June 2020 (well after the first lockdown initiated). This could be more clearly articulated earlier so that it is not a surprise to the reader.

I would also argue it is potentially a strength of the paper – the authors are holding COVID-19 (relatively) constant, and indeed cases are actually higher during some non-lockdown periods – allowing them to look more precisely at lockdowns, as opposed to the disease. I would recommend the authors make this distinction clearer, as well as to discuss more explicitly which parts of the relevant literature are specifically able to focus on lockdowns vs. merely the aggregate effect of the pandemic and lockdowns.

2. It is not clear to me why reductions in paid work seems to be in the authors' preferred channel. Based on Table 3, paid work is certainly decreasing, but so is farm income and income from assets, remittances from within country, assistance from non-family individuals, and assistance from NGOs. I would like to see the authors better justify why they believe paid work specifically to be the main pathway.

In particular, Table 5 suggests that agricultural households saw no decrease in market work at all (comparing the coefficient on "First lockdown: short run" and "Ag household x First lockdown: short run") yet still had roughly a 14pp increase in facing any food insecurity in that same period. This seems at odds with the authors' primary explanation.

3. One point which is severely underdiscussed are the changes in household size. There are increases in both adult and child members; how much of the results on food insecurity could be explained by these changes? As well, even among those not living at home, children would have been home considerably more during this time period due to school closures from March 2020 through January 2022; since many Ugandan children have lunch at school or attend boarding schools, these changes may lead to significant increases in food security. It is unclear why this channel is not considered a primary one.

Suggestions

4. How do the specific numeric estimations of food insecurity in this paper – which seem to be based on fairly standard measures – compare to other papers in the literature? The authors make specific comparisons to other household fixed effects models, but not to the broader literature, or even to the subset of literature that focuses on Uganda (Kansiime et al. focuses on Uganda; as does Agamile (2022), who actually uses much of the same dataset). Is the 25 pp estimate found by the authors large, small, or average relative to these papers?

5. How much should we expect the household fixed effects to deal with in this estimation? My main concern about omitted variable bias would be something like "vulnerability." Yet given the wide-ranging effects of the shock, many aspects of vulnerability would likely be affected by COVID-19 and the lockdowns (income diversification, availability of remittances, health, etc.). Others (land quality, education of household head, etc.) would likely be absorbed by the fixed effect. How different would the results be if the authors used an index of economic vulnerability from the 2019/20 UNPS data instead of household fixed effects? The authors argue in the set up that the use of household fixed effects represents a meaningful contribution to the literature but are fairly mild in their description of what the fixed effects achieve on page 9. More consistency would be helpful.

6. On page 21, the authors note that they interact lockdown variables with "whether the household was engaged in agricultural production in the prior round." This makes it very difficult to interpret Table 5; why not use engagement in agricultural production from the 2019/20 UNPS data? At the least, I'd like to see the authors justify making this variable dynamic in more specific detail.

7. The authors' choices about how to define short-run, medium-run and non-lockdown are unclear. Based on Figure 1, it's hard to understand why period 2 (August 2020) is medium-run, but period 3 (September 2020, one month later, with the same stringency index and similar time spent at residence) is considered non-lockdown. It would be helpful to see a clearer articulation of these choices.

8. How good is Google Mobility data in Uganda? How disaggregated is it? Is there good reason to think this data is accurate in rural areas? How many time periods are there in the data, and how is the measure constructed (e.g., last thirty days)? Based on Figure 1, it seems like the data (and much of the COVID-19) data is national in scope; is this correct? A little more detail would be helpful.

9. To the extent it is possible, it would be helpful to see more detail on whether enforcement of lockdowns might have varied subnationally. I would expect that it did (and in particular, that it might have been much less strictly enforced in rural parts of Uganda).