



SDI Review Form 1.6

Journal Name:	International Journal of TROPICAL DISEASE & Health
Manuscript Number:	Ms_IJTDH_23855
Title of the Manuscript:	Domestic violence against women and its consequence on child mortality: analysis from five African countries
Type of the Article	Original Research Article

General guideline for Peer Review process:

This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty**', provided the manuscript is scientifically robust and technically sound.

To know the complete guideline for Peer Review process, reviewers are requested to visit this link:

(<http://www.sciencedomain.org/page.php?id=sdi-general-editorial-policy#Peer-Review-Guideline>)



SDI Review Form 1.6

PART 1: Review Comments

	Reviewer's comment	Author's comment (if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)
Compulsory REVISION comments	No revision expected by me (see the General comments below)	
Minor REVISION comments	No minor comments expected by me (see the General comments below)	
Optional/General comments	I have a number of problems with the paper. The general comment is that the paper needed a number of editorial work to make it easy to understand. That said, i have the following comments: <u>Abstract (16-19)</u>: It is not clear if in line 17 at the end it means urgent efforts are needed to eradicate violence against women in Africa in countries ravaged by war or that Africa is ravaged by war. The authors should read again that part. <u>Methods (64-68)</u>: the description that “..population based survey used data from the ... NDHS...” is misleading. How can a population based survey use data from a Demographic and Health Survey? How many surveys are we talking about in this paper? It is one survey only, the DHS. The paper uses data from DHS conducted in the mid-2000s which renders to me the idea of comparability. That is, surveys conducted almost the same years and comparing their results. But this point is silent as an additional justification from the authors apart from mentioning the application of the conflict tactics scale (CTS). The question is whether the CTS is not applicable to the more recent surveys in Egypt (2014) and Nigeria (2013)? What would be the challenges if they used more recent data from Egypt and Nigeria? Would it take a long time to replicate the analyses on the new survey data?	Sentence rephrased Sentence rephrased Two reasons. The Conflicts tactical scale is missing in more recent DHS surveys. Secondly, at the time of the study analysis, the most recent country



SDI Review Form 1.6

	The CTS is not described fully? How is it applied to the “exposed” and “non-exposed” variables for measuring domestic violence? The provision of the dimensions of DV on CTS in Table 1 is not sufficient. The reader needs to understand what CTS is? Is it the categorization of DV questions as yes/no? How were these “yes/no” questions combined to form a DV index? It is not clear. Wealth index (130-135): Was the index constructed by the authors as described in the paper or they used the already computed indices provided with the public data files for DHS? Or they just regrouped the poorest/poor etc categories for the analysis? I doubt if the authors recreated the wealth index. If they did they should provide more information on the variables used apart from mentioning principal components analysis. Table titles should be self-explanatory. Just writing “Characteristics of respondents” (191) is not sufficient. Some inconsistencies in Table 2: I tried to understand why there are many women with primary or secondary school yet the proportion of those literate was small. I would assume those with no education are illiterate. For example, in Nigeria 38% had primary and secondary+ education but literacy shows 18%. In Kenya 77% had combined primary, secondary and higher yet 47% were literate. While we know that some people can go to school and still become illiterate, these differences are still huge. Table 3 is listed in the paper but not discussed anywhere. Discussion: This is flawed by stating “the study further revealed that experience of DV by mothers significantly increased the probability of under-five death.” (214). This is only applicable in Nigeria out of all the 5 countries. That said, as the authors noted in line 212, I have problems with this argument based on the following: 1/ Exposure to DV is the main predictor of child mortality yet in Table 3 (which	data with the CTS were employed in the analysis. It should also be noted that the Domestic violence data is done not done all countries and usually repeated on 10 years basis. Earlier reviewers have advised deleting any details on CTS and instead provide reference for interested readers. Description of the CTS scales fully would take 5-10 pages. An attempt at summary is at least one and half pages. This would increase the pages and word counts which most publishers recommend against. This was why references 18-22 where the CTS in each country were provided for readers that needs for details. Authors did not create the WI variable. We just re-categorized the given data into 3 groups. Noted and correction effected This is data from DHS not authors primary data. DHS categorized literacy based on ability to read and write not on educational level attained. This was explained in lines 148-151 Type error. Table 3 mentioned in lines 198
--	---	---



SDI Review Form 1.6

	is not discussed in the paper), there is no significant relationship between the two variables ($p=0.141$). How can a non-significant relationship in a bivariate level turn out significant in a full model? Usually it is the opposite. Variables that are insignificant at the bivariate level often maintain their non significant effect after controlling for other variables. That's why a number of researchers often include variables that are significant at the bivariate level in full models. 2/ And there is no relationship for the other countries. I wonder why the authors continued to run the full models? The authors mention that this may be related to small sample sizes (212-213)! That is the very reason why the analyses were supposed to be stopped. The main effects variable are not significant. None of the variables are significant in Table 4 for Egypt, Kena, and Congo DR and almost all the variables in Zimbabwe model are not significant. There is nothing to write about this paper.	and 204 under result. It should noted that regression was done in two models as stated in the methods. After confounders considered in this study were adjusted for, exposure to DV became significant even though it wasn't in the first. Also note that because of space, the first model (crude OR) was not included in figure 4. For the same reasons given above, sometimes correction for confounders and effect modifiers (not applicable here) could render an insignificant relationship significant. Number of children of mothers was significant under Zimbabwe and media access under Nigeria. Thank you! Conclusion added
--	--	--