PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Prevalence and risk factors of depression symptoms among rural and urban populations affected by Ebola virus disease in the Democratic Republic of the Congo: A representative cross-sectional study
AUTHORS	Cénat, Jude Mary; Noorishad, Pari-Gole; Dalexis, Rose Darly; Rousseau, Cécile; Derivois, Daniel; Kokou-Kpolou, Cyrille Kossigan; Bukaka, Jacqueline; Balayulu-Makila, Oléa; Guerrier, Mireille

VERSION 1 – REVIEW

REVIEWER	Mateen, F Massachusetts General Hospital
REVIEW RETURNED	09-Aug-2021

GENERAL COMMENTS	This is an overall intriguing paper by Cenat et al on depressive symptom burden approximately 7 months after an Ebola Virus Disease outbreak in a province (Equateur) of the Democratic Republic of Congo. The authors identify a high burden of depressive symptom burden in the severe range and find associations with rural residence and unemployment with higher depressive symptom burden. I have some comments and questions for the authors on their interesting work. My first comment is to strengthen the methods for the reader, including for readers who are less familiar with the background, location, and study population. In particular the Ebola Virus Disease epidemic in DRC is known but could be better depicted in terms of dates, mortality, and related details, particularly in Equateur. Sociodemographic features of this population (household income, age structure, and known features of the health of this group in general) would be helpful to characterize the setting. The authors mention twice that they are traveling in insecure transport to rural communities but this is the only way to concretely understand the level of difficulty of the study, poverty of the population, etc. Other parts of the methods need further development to be fully clear. For example, I was not sure if children were included until I read the table. I was not sure what instrument for depressive symptoms was used in the abstract or until fairly long into the paper (or why that instrument was considered to be the best and only one used).

	There were certain parts of the statistics that were not mentioned in the methods (Cronbach alpha, goodness of fit, etc.) that came up and were not described. One statement (page 8, lines 30-31) seemed unreferenced and perhaps bold: "The psychometric properties of all the measures were found to be reliable, valid, and accurate." The stated objectives were very clear in the introduction and the authors' comments on scare research on the level of EVD exposure, stigma, and depression were very accurate. Very little is done on this difficult to study topic. As a minor point, I questioned whether participation in a funeral in this setting ("Have you participated in the funeral of a person deceased because of the Ebola virus?") was appropriate to determine exposure if funerals were banned or minimized. In some countries, these regulations occurred. I am unsure if this is a valid measurement in this population or not. Another minor point is the use of "exposition . . . to HIV" and whether "exposure" was meant. In terms of the assessment of variables, the two main findings of relationship to rural vs. urban and employment were interesting but the variables were treated coarsely (dichotomizing). What is the meaning of rural or urban in this setting? What is the meaning of employed and what is the main source of work? Were sensitivity analyses done using these variables differently. Finally, I questioned the impact of poverty as a confounding variable between the EVD exposure (often in poorest settings) and the variables stated of rural setting (often poorer) and unemployed (similarly poorer). Because no measurement of poverty or economic status is given, I wonder if the authors are measuring poverty rather than any other variable and how EVD and poverty may be collinear. I think the wording of communities impacted by EVD having more depressive symptoms is valuable (versus communities who experienced EVD having more depressive symptoms as a result of EVD). The role of gender here seemed inconsistent in the analyses, and although it is interesting, the results did not consistently find a major impact of gender. A lot of the discussion is on gender in spite of this. Did the authors have a hypothesis on gender that was or wasn't supported? Thank you for allowing me to review this exciting work and congratulations to the authors for this accomplishment.

REVIEWER	Maurice, A de St University of California Los Angeles
REVIEW RETURNED	22-Aug-2021

GENERAL COMMENTS	This is a well-written manuscript that seeks to understand the impact of Ebola virus disease (EVD) outbreaks on communities' mental health. The authors look at the amount of exposure to Ebola virus and risk of depression. They also examine any sociodemographic risk factors for depression among individuals affected by EVD. The major limitation of the study is that there is no control group and the survey was not administered before the

	outbreak to determine the specific impact of the outbreak on depressive symptoms. Specific comments: Strengths and limitations: -would review point 3, should be "results have". Suggest revising point 3 as anthropological and cultural questions were not necessarily addressed in this manuscript. Suggest revising point 4 to phrase this as a limitation (e.g. A limitation of the study is that we did not have information about the history of depression in these individuals...). -One limitation that was not mentioned in this study is that the authors did not account for whether or no these participants had personally recovered from EVD or not, this could affect their own mental health and stigma. Suggest if possible accounting for survivor status Introduction -page 4 line 23, should be spelled as "Guinea' -page 5, line 5, this is awkwardly phrased. suggest revising. Methods: -the authors should include inclusion and exclusion criteria for survey participants. Was there any monetary incentive for participating in the survey? -lines 41-51: it is unclear what the authors mean by "two stage stratified". The authors should provide more details. Was the weighting used to determine the number of participants who they would recruit or was this used to identify certain populations? What do they mean by stratified and random? -page 6, lines 3 to 8, what do they mean by "selected randomly". how was this done? -page 6, line 12, am not sure that the detail about the unsecured boats is necessary -page 6, lines 17-19 the comments about "they led a survey..." seems redundant -page 6, lines 30-33, how were the psychometric properties of all measures were explored and found to be reliable -suggest moving the participants section to results -suggest including a map of the areas Results -page 9, line 10 are the percentages flipped around for men and women in the brackets (39 vs 48%)? -page 9, lines 54-55- it is interesting that those with higher education/professional training had more symptoms, is it possible that some of these individuals worked in the healthcare sector? could this be explored in the data? -page 10, lines 14 to 16, is it possible that those in relationships had children or had to support their families financially which might account for more depressive symptoms? It would be interesting if the data exist to explore this in the analysis or if not, to comment on this finding in the discussion. Discussion: -the authors should comment on relationship status and depressive symptoms -the authors should also comment on professional degree and higher education with depressive symptoms

	-the authors should comment on accessibility of mental health services in both rural and urban settings Research and clinical implications: -could the authors comment on suggestions for improving access to mental health services in rural settings? Table 1: -why were these age groups selected? suggest breaking the age groups down into fewer categories that may make more sense from a risk factor perspective (e.g. 18-39; 40-64, 65 and older). This may also provide more insight into risk factors and significance -suggest simplifying marital status as well, divorced and separated could be combined -were individuals asked if they were not religious at all? -was survivor status accounted for? -were individuals asked about children? if so would include this as a variable Table 2: -see comments above Table 3: -suggest specifying what the comparison group was in the chart and not in the footnote -see comments for Table 1 -how was age studied? was it a continuous variable?

VERSION 1 – AUTHOR RESPONSE

Reviewer 1.	
Dr. F Mateen, Massachusetts General Hospital Comments to the Author: This is an overall intriguing paper by Cenat et al on depressive symptom burden approximately 7 months after an Ebola Virus Disease outbreak in a province (Equateur) of the Democratic Republic of Congo. The authors identify a high burden of depressive symptom burden in the severe range and find associations with rural residence and unemployment with higher depressive symptom burden.	Thank you, Dr. Mateen, for reviewing our paper and for your comments that helped improve the present version of the paper.
I have some comments and questions for the authors on their interesting work. My first comment is to strengthen the methods for the reader, including for readers who are less familiar with the	At the beginning of the method section, we added considerations about the Equateur province. However, we tried to stay factual to avoid any discrimination based on the poverty of the province.

background, location, and study population. In particular the Ebola Virus Disease epidemic in DRC is known but could be better depicted in terms of dates, mortality, and related details, particularly in Equateur. Sociodemographic features of this population (household income, age structure, and known features of the health of this group in general) would be helpful to characterize the setting. The authors mention twice that they are traveling in insecure transport to rural communities but this is the only way to concretely understand the level of difficulty of the study, poverty of the population, etc.	Please, see the first paragraph of the methods section.
Other parts of the methods need further development to be fully clear. For example, I was not sure if children were included until I read the table. I was not sure what instrument for depressive symptoms was used in the abstract or until fairly long into the paper (or why that instrument was considered to be the best and only one used).	Thank you for this comment. We made it clearer in the participants' subsection: The total sample's mean age was of 34.1 (SD = 12.6), with ages ranging between 18–85.
There were certain parts of the statistics that were not mentioned in the methods (Cronbach alpha, goodness of fit, etc.) that came up and were not described. One statement (page 8, lines 30-31) seemed unreferenced and perhaps bold: "The psychometric properties of all the measures were found to be reliable, valid, and accurate."	Thank you for this comment. We agree that the sentence in question can be confusing. The methods section already gives so many details on the psychometric properties of the measures used.
The stated objectives were very clear in the introduction and the authors' comments on scare research on the level of EVD exposure, stigma, and depression were very accurate. Very little is done on this difficult to study topic.	This is indeed a limitation of this study. The fact that few studies have been conducted in the province of Equateur on mental health issues limits the comparability of our results. This also demonstrates the importance of the study.
As a minor point, I questioned whether participation in a funeral in this setting ("Have you participated in the funeral of a person deceased because of the Ebola virus?") was appropriate to determine exposure if funerals were banned or minimized. In some countries, these regulations occurred. I am unsure if this is a valid measurement in this population or not.	Yes, initially funerals were banned during the 2013-2016 epidemic. But since then, they have been allowed under strict conditions. Even funerals have been banned, studies have shown that there is little adherence to these measures. In many neighborhoods, even when someone is dead with EVD symptoms, public health authorities are not called. The families take care of the funeral themselves without any health measures.

	This is why the question is relevant in the EVD outbreak context.
Another minor point is the use of "exposition . . . to HIV" and whether "exposure" was meant.	Thank you for this comment. We corrected it.
In terms of the assessment of variables, the two main findings of relationship to rural vs. urban and employment were interesting but the variables were treated coarsely (dichotomizing). What is the meaning of rural or urban in this setting? What is the meaning of employed and what is the main source of work? Were sensitivity analyses done using these variables differently.	Thank you for this comment. For rural vs. urban, analyses were first performed for the 18 cities (urban areas) and villages (rural areas) affected. There was a consistency for urban and rural areas on the results presented. There was no point in presenting the results by the 18 affected areas since they were the same than those presented. It would have fragmented the sample for similar results. With respect to employment, the first question asked was whether they had a job or an income-generating activity (yes, no). Then, the employment category was asked. Non-conclusive analyses were done for the employment category, so we simply presented whether they had a job, i.e., an income-generating activity.
Finally, I questioned the impact of poverty as a confounding variable between the EVD exposure (often in poorest settings) and the variables stated of rural setting (often poorer) and unemployed (similarly poorer). Because no measurement of poverty or economic status is given, I wonder if the authors are measuring poverty rather than any other variable and how EVD and poverty may be collinear. I think the wording of communities impacted by EVD having more depressive symptoms is valuable (versus communities who experienced EVD having more depressive symptoms as a result of EVD).	Thank you for this interesting comment. The concern is that in the province of Equateur, the level of poverty is so high, and the wages are so low that the comparison of wage levels did not yield anything significant. The reality is more complex. We have just conducted interviews and this is one of the issues that we have been exploring. Some people having a close relative abroad (Europe, North America) live with more money than a teacher who earns very little. These are complex factors that should be presented with caution.

The role of gender here seemed inconsistent in the analyses, and although it is interesting, the results did not consistently find a major impact of gender. A lot of the discussion is on gender in spite of this. Did the authors have a hypothesis on gender that was or wasn't supported?	We do not have any particular hypothesis. Otherwise, as we have noted, we have made the same observation in different studies conducted in Africa (DRC, Togo and Rwanda). We hope to learn more from the qualitative studies.
Thank you for allowing me to review this exciting work and congratulations to the authors for this accomplishment.	Thank you again for your detailed comments, we appreciated the value and clarity they have added to the paper.
Reviewer: 2	
Dr. A de St Maurice, University of California Los Angeles Comments to the Author: This is a well-written manuscript that seeks to understand the impact of Ebola virus disease (EVD) outbreaks on communities' mental health. The authors look at the amount of exposure to Ebola virus and risk of depression. They also examine any sociodemographic risk factors for depression among individuals affected by EVD. The major limitation of the study is that there is no control group and the survey was not administered before the outbreak to determine the specific impact of the outbreak on depressive symptoms.	Thank you, Dr. St Maurice for your comments that helped us improve this new version of our manuscript. Indeed, as we have noted, we agree that this is the greatest limitation of this study. This study was funded under a call for proposals on Ebola Virus Disease with projects limited to two years in duration. We do not always have the opportunity to eliminate such limitations. But we accept, as we have noted, that it would have been so interesting to be able to evaluate findings before and after EVD.
Specific comments: Strengths and limitations: -would review point 3, should be "results have". Suggest revising point 3 as anthropological and cultural questions were not necessarily addressed in this manuscript. Suggest revising point 4 to phrase this as a limitation (e.g. A limitation of the study is that we did not have information about the history of depression in these individuals...).	Thank you for this comment. We have revised point 3. - Results have important implications suggesting a particular need to implement mental health programs that integrate the rural and gender specificities of communities affected by EVD. - A limitation of the study is that we did not have information about the

	history of depression in these individuals.
-One limitation that was not mentioned in this study is that the authors did not account for whether or no these participants had personally recovered from EVD or not, this could affect their own mental health and stigma. Suggest if possible accounting for survivor status	This is a longstanding consideration for us. We don't think it's appropriate to add survivor status as a limitation because there were few survivors. Even including the 23 survivors would not have allowed us to do comparison analyses.
Introduction -page 4 line 23, should be spelled as "Guinea'	Thank you. We have corrected the word.
-page 5, line 5, this is awkwardly phrased. suggest revising.	Thank you. We revised the sentence and divided it in two: "In addition, in the last three years, the Democratic Republic of the Congo (RDC) has been facing recurrent outbreaks of EVD and the COVID-19 pandemic [8]. Studies are important to document the prevalence of mental health problems among populations affected by infectious disease epidemics, as well as associated risk factors."
Methods: -the authors should include inclusion and exclusion criteria for survey participants. Was there any monetary incentive for participating in the survey?	Thank you for this comment. We added the inclusion criteria: "The inclusion criteria were: (1) be at least 18 years of age; (2) live in one of the 18 villages and cities affected by EVD since the beginning of the outbreak; (3) speak French, Lingala, Tshiluba, Kikongo, or Swahili; (4) have no mental health disorder that interferes with functioning. Participants in the study did not receive any monetary compensation." However, there was not monetary incentive for participating in the survey.
-lines 41-51: it is unclear what the authors mean by "two stage stratified". The authors should provide more details.	Thank you for this comment. We highlighted the information in the

Was the weighting used to determine the number of participants who they would recruit or was this used to identify certain populations?	text (p. 6). As it is written, the two-stage stratified sampling was used to ensure adequate representation across gender and urban and rural areas in terms of proportion.
What do they mean by stratified and random? -page 6, lines 3 to 8, what do they mean by "selected randomly". how was this done?	Thank you for this comment. We provided more information on page 6. We hope it is clearer now that: - The Two-stage stratified approach is used to ensure an accurate representation of gender and residence areas (urban vs rural) in our sample. - The households in the 18 affected urban and rural areas were selected randomly. When a selected household was vacant and when any person wanted to participate but did not meet inclusion criteria, the next household was selected.
-page 6, line 12, am not sure that the detail about the unsecured boats is necessary	Thank you for this comment. I would like to remove it, but the first reviewer thought it was helpful to give such details to help the readers know more about the context of the study.
-page 6, lines 17-19 the comments about "they led a survey..." seems redundant	Thank you for this comment. We removed it and integrated the information about the door-to-door approach in another sentence.
-page 6, lines 30-33, how were the psychometric properties of all measures were explored and found to be reliable	Thank you for this comment. As we said to the first reviewer, we removed the sentence. The description of the instruments contains all the information about the psychometric properties of measures used.
-suggest moving the participants section to results	Thank you for this suggestion. However, we think it is important to keep it in the methods section and give such details before the description of the measures and

	the analytic plan. However, if the editor decides that it is best to move it, we do not have any concerns with that.
-suggest including a map of the areas	We have provided a Map from CDC. We asked different colleagues from DRC for a map, but there is no map for the eastern regions. However, the map provides a good idea of the size of the province and the urban and rural areas.
Results -page 9, line 10 are the percentages flipped around for men and women in the brackets (39 vs 48%)?	Thank you for this comment. We corrected it: "[respectively, 48.0%, (95% CI: 45.6-50.5) and 39.9% (95% CI: 37.6-42.3), $x^2 = 4.7$, $p = .03$]."
-page 9, lines 54-55- it is interesting that those with higher education/professional training had more symptoms, is it possible that some of these individuals worked in the healthcare sector? could this be explored in the data?	Thank you for this comment. We were very excited to analyze our data and see the results. We crossed two items to investigate (level of education and do you work as a healthcare worker during the EVD outbreak), but unfortunately there is no link.
-page 10, lines 14 to 16, is it possible that those in relationships had children or had to support their families financially which might account for more depressive symptoms? It would be interesting if the data exist to explore this in the analysis or if not, to comment on this finding in the discussion.	Thank you for this comment. Unfortunately, we do not have data to make this comparison. However, we do think it would be complicated to make this assumption since those who are married (they can have children and families also) did not present more depressive symptoms but only those who are in an unmarried relationship. We think this result could relate more to the precarious status of unmarried people in the province. We do not have any studies on that, but we are currently conducting interviews and one of the situations reported is the fact that when women are unmarried, they experience more stigma. Also, partners can leave them without any financial security for themselves and their children.

Discussion: -the authors should comment on relationship status and depressive symptoms -the authors should also comment on professional degree and higher education with depressive symptoms	We have made considerations on both relationship status and higher education. However, this discussion is very limited since there is no study on depression in this population.
-the authors should comment on accessibility of mental health services in both rural and urban settings	We added more details on mental health services. Since they are almost inexistent within the province of Equateur and completely inexistent in rural areas, we decided to include these considerations in the implications section. Please, see the paragraph just before the conclusions.
Research and clinical implications: -could the authors comment on suggestions for improving access to mental health services in rural settings?	Thank you for the comment, but we are not sure to understand well. The recommendations are based on the almost absence of mental health services and professionals we have observed in the Province of Equateur. They are also based on the results observed in the only systematic review on mental health programs in the context of EVD outbreaks. Since it will take time and means to train mental health professionals, it is important to find different perspectives to prevent and provide mental health care to the population. This is the motivation of our recommendations.
Table 1: -why were these age groups selected? suggest breaking the age groups down into fewer categories that may make more sense from a risk factor perspective (e.g. 18-39; 40-64, 65 and older). This may also provide more insight into risk factors and significance -suggest simplifying marital status as well, divorced and separated could be combined -were individuals asked if they were not religious at all? -was survivor status accounted for?	We selected this distribution of age groups to be consistent with statistical reporting in the DRC. In addition, the sample is well enough distributed to observe if there are significant differences for the age groups. Except for those aged 65 and older, we have over 100 people in each age group. Keeping the analyses this way will allow for better details and comparison with future studies.

-were individuals asked about children? if so would include this as a variable	In the first analyses, divorced and separated were joined and this had no impact on the results. This prompted us to separate them to allow readers to make more detailed observations. Yes, we asked people if they were not religious at all. Less than 1% responded that they were not religious at all. We added it to Other since it didn't make sense to leave it as a category. There are only 23 survivors after this outbreak. We identified 16 for qualitative studies. But the number was too small for quantitative considerations. The participants were not asked about this aspect.
Table 3: -suggest specifying what the comparison group was in the chart and not in the footnote -see comments for Table 1 -how was age studied? was it a continuous variable?	Thank you. We added reference criteria directly in Table 3. Age was a continuous variable. We also entered it as the age groups and with three categories as you have suggested. There is no difference in the results.

VERSION 2 – REVIEW

REVIEWER	Maurice, A de St University of California Los Angeles
REVIEW RETURNED	26-Oct-2021

GENERAL COMMENTS	The authors have addressed most of our concerns however I think the methods could still be elaborated on. 1. On page 6: the randomization process is still unclear. what process did the authors use to randomly select a home? Was any randomization technology used?

VERSION 2 – AUTHOR RESPONSE

Comments	Response
On page 6: the randomization process is still unclear. what process did the authors use to randomly select a home? Was any randomization technology used?	Thank you for this comment that gives us the opportunity to clarify. We added the information (p. 6). We first used the website: (www.stattrek.com/statistics/random-number-generator.aspx). After the field considerations, our statistician programmed a random number generator herself to better took into account of geographical and gender issues. "Data was collected during door-to-door surveys in the three "health zones" (Bikoro, Iboko, and Wangata) affected by the 9th EVD outbreak in the DRC. These "health zones" include 18 rural and urban areas in the Province of Equateur (see Figure 1). Households in the 18 affected areas were selected randomly using the same sampling frame as the Demographic Health Survey of 2013-2014 and the Multiple Indicator Surveys conducted by National Institute of Statistics [11]. The sample was selected randomly from the most recent household list using a computer-based random number generator for each affected health zone separately. When a house was found vacant by investigators or when a person wanted to participate but did not meet inclusion criteria, the next house was selected."