PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	COVID-19 in Ethiopia: A geospatial analysis of vulnerability to infection, case severity, and death
AUTHORS	Alene, Kefyalew; Assefa, Yalemzewod; Fetene, Dagnachew; Koye, Digsu; Melaku, Yohannes Adama; Gesesew, Hailay; Birhanu, Mulugeta; Adane, Akilew; Muluneh, Muluken; Dachew, Berihun; Abrha, Solomon; Aregay, Atsede; Ayele, Asnakew; Bezabhe, Woldesellassie; Tadesse, Kidane; Gebremedhin, Tesfaye; Tesfay, Amanuel; Gebremichael, Lemlem; Geleto, Ayele; Kassahun, Habtamu; Kibret, Getiye; Leshargie, Cheru; Mekonnen, Alemayehu; Mirkuzie, Alemnesh; Mohammed, Hassen; Tegegn, Henok; Gebresilassie, A; Tesfay, Fisaha; Wubishet, Befikadu; Kinfu, Yohannes

VERSION 1 – REVIEW

REVIEWER	Mohamed A Daw Department of Medical Microbiology &Immunology, Faculty of Medicine, University of Tripoli, Tripoli LIBYA
REVIEW RETURNED	05-Oct-2020

GENERAL COMMENTS	Accept.

REVIEWER	Bernard Ekumah University of Cape Coast
REVIEW RETURNED	06-Oct-2020

GENERAL COMMENTS	Thank you for giving me the opportunity to assess this manuscript titled 'COVID-19 in Ethiopia: a geospatial analysis of vulnerability to infection, case severity, and likelihood of death' for publication. The aim of the manuscript was clearly defined. The study will be relevant in preventing and controlling the spread and treatment of COVID-19 in Ethiopia. The study will also contribute to literature on COVID-19 especially the application of geospatial techniques. Besides, the study will help inform policymakers to design targeted interventions to address future pandemics in Ethiopia. However, the authors needs to address some important comments before this paper can be considered for publication. 1. Page 6, line 175, the sentence "The analysis data..." should be reframed. 2. Page 7, line 215, select should be selected 3. Page 8, line 234, Authors should mention the tools in ArcGIS which were used to create the Fishnet. 4. It is surprising that the authors kept referring to "1km x 1km" as high resolution at a country level studies. This could have been allowed if the study was a global one. Even 30m resolution data

	cannot be regarded as high resolution data at the country level. I disagree with that assertion and they should find an appropriate term or desist from it use. The study will still be relevant without the "high resolution" phrase. 5. Authors should indicate the spatial resolution of all the data used in the study in Table 1. If some of the raw data were not geolocated, they should include in the Method section how the non-spatial data were assigned geolocations. 6. One major issue I have with this manuscript has to do with repeatability. The way some portions of the Method section was presented makes it difficult for others to repeat the study and come out with similar findings. Authors need to take a critical look at this and address it accordingly. For instance, why and how was the aggregation done (page 8, lines 254 and 255)? Why was PCA appropriate for the aggregation? How was the composite index derived and which of the geostatistical tool was used? 7. All models/predictions are accompanied with some level of uncertainties. Some of the data points used in this study (EDHS datasets) were not uniformly distributed in space. Some areas might have been dense and others sparse, how did the authors account for it in their predictions? Authors should generate uncertainty map that corresponds to each of the vulnerability maps. 8. If COVID-19 cases reported in Ethiopia are disaggregated, then authors should provide data on current case count at least at the regional level which can be referred to in the discussion to enrich it. 9. One of the limitations of the study that the authors failed to mention in the manuscript is the non-uniformity of the data collection year of the variables used in this study. How was this addressed?

REVIEWER	Ibrahim ADEKUNLE Olabisi Onabanjo University
REVIEW RETURNED	09-Oct-2020

GENERAL COMMENTS	Introduction section is structurally deficient. Readers are required to read around two pages to really come to the research question of this study. To keep readers engaged, make your objective clear at the beginning of the introduction.You should consider re-writing introduction section in the following order: (i) Your research idea (aim/hypothesis); (ii) Why is it important? (iii) What is new about your work?; (iv) Your approach; (v) Findings & contributions. You almost discussed all these points, but it is all over the place and very difficult to follow. Therefore, I strongly suggest you should restructure the introduction section as per the order mentioned above. The authors need to provide credible justification for inclusion of each variable in a model that predicts COVID-19 severity in Ethiopia. I am not clear on how the average number of persons per household, the proportion of the population aged ≥ 65 years, the proportion of males, and the number of households in the lowest wealth quintile represents COVID-19 infection and cases severity. Unless, there is a sperate statistical database that clearly point to how this indices relates to COVID-19, i disagree that they are appropritate measure for to socio-demographic indices of coronavirus.

REVIEWER	Muhammad Farhan Ul Moazzam Jeju National University, South Korea

REVIEW RETURNED	24-Nov-2020

GENERAL COMMENTS	The paper seems interesting and innovative, it can be accepted.

REVIEWER	Leah Rosenkrantz Simon Fraser University British Columbia, Canada
REVIEW RETURNED	27-Nov-2020

GENERAL COMMENTS	Introduction: Line 135-136 on African countries having reported low numbers of COVID-19. It would be good to see more context provided here. Why have African countries reported lower numbers of cases and deaths? Is this a testing issue (i.e. not enough tests to confirm cases) or something else? At the very least, I think it is important to acknowledge this situation for Ethiopia. Line 137. Please clarify what is meant by “recently spread”. Line 138. Please clarify what is meant by “various levels”. Line 139. This sentence does not fit with what is then explained in the paragraph. It leads the reader to believing the paragraph will be about the severe health and economic consequences Ethiopia has faced from COVID-19. Methods: Line 171. “The four regional states” should be changed to “Four of these regional states” to clarify you are talking about a subset of the nine administrative states mentioned previously. Line 172. Please define “developing regional states”. Line 190. How was 50,000 people decided upon? What is the rational/evidence from literature for using this figure. Line 197-200. The evidence you cite here re. public transportation is from a study conducted in China. The public transportation infrastructure there is widely different from that in Ethiopia. Furthermore, major roads do not seem like a very good proxy for per capita public transportation use rate in my mind for several reasons (and especially in a country that does not have a strong public transportation infrastructure). Line 210- 216. Is it possible you are double counting certain variables in your model here? Smoking and alcohol are highly correlated with NCDs. Same thing for TB and CRD service preparedness in the paragraph after. Double counting could be skewing your model. Discussion: Line 330-335. Re. public health interventions: I think some recognition of the political and economic climate in Ethiopia is critical here. It is easy to say they should strengthen and expand health care services, but this is not so easy. Line 351: Some examples of non-pharmaceutical interventions might

	be helpful for the reader. Line 357- 360. Unclear… what comparisons are you referring to? Policy implications: All of what is recommended requires much more analysis and evidence than what your study looks at/focusses on. Instead, the policy implications should center on WHERE public health interventions/measures should be applied to have greatest impact. This, after all, is what your study provides evidence for. Strengths and Limitations: Line 391-392. Please elaborate on what you mean by time-varying variables and how they would change when new interventions are introduced. Line 392-392. Please elaborate on the ongoing political turmoil in the country. Please specify ways that the dynamics of transmission could change as a result. Line 395- 396. Please explain the effect of not using a weighted model. This is really critical, and while I do not blame you for not being able to use one, I do think its limitations and effect on your findings need to be more clearly stated. Line 397. What other psychosocial and clinical factors would you have wanted to include? General: There has been evidence that suggests population density itself is not associated with COVID-19 (see below citations for some examples). Please justify its use in your model based on this conflicting evidence. • Hamidi, S., Sabouri, S., & Ewing, R. (2020). Does Density Aggravate the COVID-19 Pandemic?: Early Findings and Lessons for Planners. Journal of the American Planning Association, 1–15. https://doi.org/10.1080/01944363.2020.1777891 • Coryne, H. (2020). In Chicago, Urban Density May Not Be to Blame for the Spread of the Coronavirus. ProPublica. https://www.propublica.org/article/in-chicago-urban-density-may-not-be-to-blame-for-the-spread-of-the-coronavirus?token=l0i8JndZRzf9U7hmG1DlFV6RjLJo1zYf • Fang, W., & Wahba, S. (2020). Urban Density Is Not an Enemy in the Coronavirus Fight: Evidence from China. World Bank Blogs. https://blogs.worldbank.org/sustainablecities/urban-density-not-enemy-coronavirus-fight-evidence-china • Maroko, A., Nash, D., & Pavilonis, B. (2020). Covid-19 and Inequity: A comparative spatial analysis of New York City and Chicago hot spots. Publications and Research. https://academicworks.cuny.edu/sph_pubs/258 • Pafka, E. (2020). As coronavirus forces us to keep our distance, city density matters less than internal density. The Conversation. http://theconversation.com/as-coronavirus-forces-us-to-keep-our-distance-city-density-matters-less-than-internal-density-137790 I am concerned about the equal weighting of the model. While I understand that there is limited evidence on COVID-19 to weight the variables, I feel that your inclusion of climactic variables skews your model strongly for something that is a much smaller factor in COVID-19 infection than say handwashing or household crowding.

	For me, it seems odd that Addis had low likelihood of infection where more rural areas had higher. I think this could be a result of climactic variables masking results. Spelling mistake in figure 4. I think Risk of death model needs to be renamed to better describe what it captures (i.e. service preparedness/access to care). Otherwise, it should also include indicators for severity as this is a strong predictor of death. Consequently, discussion around this model will need revising.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1: Mohamed A Daw; Institution and Country: Department of Medical Microbiology &Immunology, Faculty of Medicine, University of Tripoli, Tripoli LIBYA
Comments to the Author
Accept
Response: We thank you for the positive feedback and your valuable time to review our work.

Reviewer 2: Bernard Ekumah; Institution and Country: University of Cape Coast
Comments to the Author

Thank you for giving me the opportunity to assess this manuscript titled 'COVID-19 in Ethiopia: a geospatial analysis of vulnerability to infection, case severity, and likelihood of death' for publication. The aim of the manuscript was clearly defined. The study will be relevant in preventing and controlling the spread and treatment of COVID-19 in Ethiopia. The study will also contribute to literature on COVID-19 especially the application of geospatial techniques. Besides, the study will help inform policymakers to design targeted interventions to address future pandemics in Ethiopia.
However, the authors needs to address some important comments before this paper can be considered for publication.
Response: Thank you for your valuable feedback and taking the time to review our work. We have addressed all the comments and have revised the manuscript carefully.
1. Page 6, line 175, the sentence "The analysis data..." should be reframed.
Response: This has now been corrected as “The data for this study were assembled from multiple sources” in the revised version of the manuscript on page 6, line 183.
2. Page 7, line 215, select should be selected
Response: This has been now corrected (see page 7, line 224).
3. Page 8, line 234, Authors should mention the tools in ArcGIS which were used to create the Fishnet.
Response: The Fishnet was created using a sampling tool under the data management tools in ArcToolbox. This information is now included in the revised version of the manuscript on page 8, line 243-247.
4. It is surprising that the authors kept referring to "1km x 1km" as high resolution at a country level studies. This could have been allowed if the study was a global one. Even 30m resolution data cannot be regarded as high resolution data at the country level. I disagree with that assertion and they should find an appropriate term or desist from it use. The study will still be relevant without the "high resolution" phrase.
Response: We agree with the reviewer and have now removed the phrase ‘high resolution’ throughout the revised version of the manuscript.
5. Authors should indicate the spatial resolution of all the data used in the study in Table 1. If some of the raw data were not geolocated, they should include in the Method section how the non-spatial data

were assigned geolocations.
Response: Thank you. The spatial resolution of all the data used in the study has been now included in Table 1. Additional information on computational procedures applied on 'non-spatial' data has also been included in the revised version of the manuscript on page 8, line 242-247.
6. One major issue I have with this manuscript has to do with repeatability. The way some portions of the Method section was presented makes it difficult for others to repeat the study and come out with similar findings. Authors need to take a critical look at this and address it accordingly. For instance, why and how was the aggregation done (page 8, lines 254 and 255)? Why was PCA appropriate for the aggregation? How was the composite index derived and which of the geostatistical tool was used?
Response: Thank you for these comments. We have noted that the terminology 'aggregation' is not the appropriate term to explain what we have done in the analysis. We have now amended our statement to say that all indicators included in our analysis were combined to produce a composite index, measuring the risk of COVID-19. The composite indices were calculated for data reduction. The calculation of the composite index was based on the unweighted average by assuming equal importance for all indicators included in this study. The risk maps were then created separately for infection, case severity, service preparedness, and death from the composite index for each domain using geostatistical tools (i.e., kriging and semivariograms) in ArcGIS. The principal component analysis (PCA) and geometric method were also used as alternative methods of generating composite indexes and checking the robustness of the 'aggregation methods used in the study. All methods have produced broadly similar results (see page 9, line 271-275).
7. All models/predictions are accompanied with some level of uncertainties. Some of the data points used in this study (EDHS datasets) were not uniformly distributed in space. Some areas might have been dense and others sparse, how did the authors account for it in their predictions? Authors should generate uncertainty map that corresponds to each of the vulnerability maps.
Response: Thank you. We have used kriging with semivariograms to create a prediction grid surface from a scattered set of points for unsampled areas. Raster grids were then resampled to the common georeferenced system at a spatial resolution of 1 km x 1 km. We have now included an additional map showing the distribution of the datapoints for the EDHS surveys in the supplementary information (new Supplementary Figure 1).
8. If COVID-19 cases reported in Ethiopia are disaggregated, then authors should provide data on current case count at least at the regional level which can be referred to in the discussion to enrich it.
Response: At the time of this study, there were not enough number of reported COVID-19 cases to produce a map. However, we have now provided the current number of COVID-19 at the regional level in Ethiopia (new Supplemental Figure 5). Data below region level are not available.
9. One of the limitations of the study that the authors failed to mention in the manuscript is the non-uniformity of the data collection year of the variables used in this study. How was this addressed?
Response: We agree with the reviewer that this remains to be a limitation of the current study. This has been now included as limitation in the discussion sections of the revised version of the manuscript on page 13, line 410-414.

Reviewer 3: Ibrahim ADEKUNLE; Institution and Country: Olabisi Onabanjo University

Comments to the Author
Introduction section is structurally deficient. Readers are required to read around two pages to really come to the research question of this study. To keep readers engaged, make your objective clear at the beginning of the introduction. You should consider re-writing introduction section in the following order: (i) Your research idea (aim/hypothesis); (ii) Why is it important? (iii) What is new about your work?; (iv) Your approach; (v) Findings & contributions. You almost discussed all these points, but it is all over the place and very difficult to follow. Therefore, I strongly suggest you should restructure the introduction section as per the order mentioned above.
Response: Thank you for your time in reviewing the work and the valuable feedback. It is entirely

possible to organize the introduction section as suggested. Although we did not exactly follow the formats suggested by the reviewer, we have now revised the contents of the introduction to address the reviewer's concern and to make it clearer for readers. The first paragraph in our study explains what is already known (essential information needed to understand the background/problem). The second paragraph describes what is unknown or should have been known, which identifies the gaps in the current knowledge and demonstrate the need for the current study (help the reader to understand why the research is being performed). The third paragraph is about the objective of the study (it explains the research question needed to be addressed in the current study). We, therefore, believe that the current structure of the introduction is appropriate and provide readers with the background information needed to understand our study, the reasons why we conducted this study, and the main objectives of our study. We have also revised the contents of the introduction to address the reviewer's concern and to make it clearer for readers.

The authors need to provide credible justification for inclusion of each variable in a model that predicts COVID-19 severity in Ethiopia.
Response: Thank you for this comment. Variable selection was guided by two factors: based on previous evidence and availability of relevant country-wide representative data at a level or scale needed for analysis. The justification for the inclusion of each variable is presented in Table 2 on pages 21 and 22.

I am not clear on how the average number of persons per household, the proportion of the population aged ≥ 65 years, the proportion of males, and the number of households in the lowest wealth quintile represents COVID-19 infection and cases severity. Unless, there is a sperate statistical database that clearly point to how this indices relates to COVID-19, i disagree that they are appropritate measure for to socio-demographic indices of coronavirus.

Response: Table 2 provides information on available evidence on the relevance of each of the indicators used in the analysis. For example, high household density, which can be measured using an average number of persons per household, is known to a fertile ground for respiratory disease and can increase COVID infection rate in a community because of household congestion. Similarly, the presence of older people increases severity levels of the disease, as people older than 65 years tend to have a higher risk of hospitalization and to develop severe forms of the disease.
Reviewer 4: Muhammad Farhan Ul Moazzam; Institution and Country: Jeju National University, South Korea
Comments to the Author
The paper seems interesting and innovative, it can be accepted.
Response: We thank the reviewer for their positive feedback and taking their valuable time to review our work.

Reviewer 5: Leah Rosenkrantz; Institution and Country: Simon Fraser University British Columbia, Canada

Comments to the Author
Introduction:
Line 135-136 on African countries having reported low numbers of COVID-19. It would be good to see more context provided here. Why have African countries reported lower numbers of cases and deaths? Is this a testing issue (i.e. not enough tests to confirm cases) or something else? At the very least, I think it is important to acknowledge this situation for Ethiopia.

Response: Thank you for spending your time to review our work and your valuable feedback. We have now provided additional information in the introduction section to provide contextual meaning for the low number of cases in Ethiopia on page 5, line 137-142.

Line 137. Please clarify what is meant by “recently spread”.
Response: This sentence was meant to highlight that the first case of COVID-19 was reported on 13 March 2020 in Addis Ababa (the capital city), and currently affecting almost all regions at various levels. We have rephrased the sentence to clarify this, as shown on page 5 line 137-142.
Line 138. Please clarify what is meant by “various levels”.
Response: This means not all regions and parts of the country were affected equally, that there were regional variations. We have rephrased the sentence for clarity as shown on page 5 line 137-142.

Line 139. This sentence does not fit with what is then explained in the paragraph. It leads the reader to believing the paragraph will be about the severe health and economic consequences Ethiopia has faced from COVID-19.
Response: Thank you. We have now removed this sentence from the revised version of the manuscript.
Methods:
Line 171. “The four regional states” should be changed to “Four of these regional states” to clarify you are talking about a subset of the nine administrative states mentioned previously.
Response: As suggested, we have now amended the sentence in the revised version of the manuscript on page 6 line 176.
Line 172. Please define “developing regional states”.
Response: We have now included additional information to clarify what is meant by developing regional states on page 6 line 178-180.
“They lag behind the rest of the country in all indicators and other human development indicators”
Line 190. How was 50,000 people decided upon? What is the rational/evidence from literature for using this figure.
Response: This is a definition was used by the Malaria Atlas Project (MAP) 1 when calculating the travel time required to reach the nearest urban centre. However, we have now removed this in the revised manuscript to avoid confusion.
Line 197-200. The evidence you cite here re. public transportation is from a study conducted in China. The public transportation infrastructure there is widely different from that in Ethiopia. Furthermore, major roads do not seem like a very good proxy for per capita public transportation use rate in my mind for several reasons (and especially in a country that does not have a strong public transportation infrastructure).
Response: The evidence from China was used for this variable because at the time of this study, there was no study assessing the relationship between public transport and COVID-19 in Ethiopia. We have now revised the citation using a study conducted in Ethiopia2. When we run the analysis with and without this variable, the result (map) was almost similar.

Line 210- 216. Is it possible you are double counting certain variables in your model here? Smoking and alcohol are highly correlated with NCDs. Same thing for TB and CRD service preparedness in the paragraph after. Double counting could be skewing your model.
Response: We agree that double-counting could affect the data generating process. Two procedures were applied to prevent this in our analysis. One, we aggregated collinear and conceptually intersecting indicators into a sub-index, prior to aggregation, to limit their overrepresentation in the overall score. The sub-indexes created prior to the overall score were a socio-demographic indicator, disease prevention and knowledge indicator, hand hygiene indicators, climatic indicators, comorbidity indicators, behavioral indicators, service preparedness indicators. Second, a principal components analysis was also used to overcome the problem when two or more indicators partially measure the same behavior. This method used to extract statistical correlations between indicators enabling a core group of indicators to be identified which statistically best represent the remaining excluded indicators. The findings from both methods were broadly similar and presented as a supplementary material (Supplemental Figure 3).
Discussion:

Line 330-335. Re. public health interventions: I think some recognition of the political and economic climate in Ethiopia is critical here. It is easy to say they should strengthen and expand health care services, but this is not so easy.
Response: Thank you for these suggestions. We agree that providing such a recommendation is challenging, especially in the current political and economic context of Ethiopia. The relevant phrase has been removed in the revised manuscript.
Line 351: Some examples of non-pharmaceutical interventions might be helpful for the reader.
Response: We have now included examples of non-pharmaceutical interventions such as physical distancing, mask use, and closure of schools in the revised version of the manuscript on page 12, line 368-369.
Line 357- 360. Unclear… what comparisons are you referring to?
Response: Sorry, this was not much clear. A comparison was made between the border regions and other regions in the country. We have now included this additional information in the revised version of the manuscript on page 12, line 376.
Policy implications:
All of what is recommended requires much more analysis and evidence than what your study looks at/focusses on. Instead, the policy implications should center on WHERE public health interventions/measures should be applied to have greatest impact. This, after all, is what your study provides evidence for.
Response: We appreciate the reviewer's concerns and have removed the policy implication section from the revised version of the manuscript on page 13.
Strengths and Limitations:
Line 391-392. Please elaborate on what you mean by time-varying variables and how they would change when new interventions are introduced.
Response: "The data used in this study were not collected in the same year and the results might be changed if recently available data used in the analysis…" This is now explained in the revised version of the manuscript on page 13, line 409-414.
Line 392-392. Please elaborate on the ongoing political turmoil in the country. Please specify ways that the dynamics of transmission could change as a result.
Response: We have elaborated this in the revised version of the manuscript on page 13, line 416-419: "For example, in areas of low security resulting from active conflict, the local health systems might be ill-prepared to prevent and control COVID-19. Insecurity also may generate unpredictable population movements, and this in turn could exacerbate infection dynamics in the country."
Line 395- 396. Please explain the effect of not using a weighted model. This is really critical, and while I do not blame you for not being able to use one, I do think its limitations and effect on your findings need to be more clearly stated.
Response: Some of the variables included in our score may have greater effects on vulnerability to infection, case severity, and the likelihood of death than others. Giving equal weight for all these variables may influence the findings of our study, but the exact effect is hard to tell. However, we have calculated a weighted index using PCA as an alternative aggregation method, which produced broadly similar results (Supplemental Figures 2 and 3). We have included this additional information in the revised version of the manuscript on page 14, line 421-426.
Line 397. What other psychosocial and clinical factors would you have wanted to include?
Response: Some psychosocial and clinical factors such as mental illness, quality of life, and social support were not captured in our modelling due to the lack of geocoded data. We have now listed these indicators in the revised version of the manuscript on page 14, line 427-428.
General:
There has been evidence that suggests population density itself is not associated with COVID-19 (see below citations for some examples). Please justify its use in your model based on this conflicting evidence.
• Hamidi, S., Sabouri, S., & Ewing, R. (2020). Does Density Aggravate the COVID-19 Pandemic?: Early Findings and Lessons for Planners. Journal of the American Planning Association, 1–15.

https://doi.org/10.1080/01944363.2020.1777891
• Coryne, H. (2020). In Chicago, Urban Density May Not Be to Blame for the Spread of the Coronavirus. ProPublica. https://www.propublica.org/article/in-chicago-urban-density-may-not-be-to-blame-for-the-spread-of-the-coronavirus?token=l0i8JndZRzf9U7hmG1DlFV6RjLJo1zYf
• Fang, W., & Wahba, S. (2020). Urban Density Is Not an Enemy in the Coronavirus Fight: Evidence from China. World Bank Blogs. https://blogs.worldbank.org/sustainablecities/urban-density-not-enemy-coronavirus-fight-evidence-china
• Maroko, A., Nash, D., & Pavilonis, B. (2020). Covid-19 and Inequity: A comparative spatial analysis of New York City and Chicago hot spots. Publications and Research. https://academicworks.cuny.edu/sph_pubs/258
• Pafka, E. (2020). As coronavirus forces us to keep our distance, city density matters less than internal density. The Conversation. http://theconversation.com/as-coronavirus-forces-us-to-keep-our-distance-city-density-matters-less-than-internal-density-137790
Response: We agree that there is conflicting evidence regarding the association between population density and COVID-19 infection. There are also studies that showed the effects of population density on COVID-19 3-7. Prior to this study, our team had conducted a comprehensive systematic review to identify risk factors for COVID-19 infection, disease severity and related deaths in Africa (a manuscript currently under-review for publication). In this systematic review, population density was associated with COVID-19 infection.

I am concerned about the equal weighting of the model. While I understand that there is limited evidence on COVID-19 to weight the variables, I feel that your inclusion of climactic variables skews your model strongly for something that is a much smaller factor in COVID-19 infection than say handwashing or household crowding. For me, it seems odd that Addis had low likelihood of infection where more rural areas had higher. I think this could be a result of climactic variables masking results.
Response: The aspect of 'equal weighting' has already been addressed above. As we mentioned earlier, the weighted index was calculated using PCA as an alternative method, which produced broadly similar results. The results of this method have also been presented as a supplementary information (Supplemental Figure 3).
Regarding the comment on Addis Ababa, it is possible that climatic variables may have had a role in the dynamics. That said, rural areas are particularly disadvantaged than Addis Ababa. For example, socio-economic indicators (i.e., number of household members, and wealth index), disease prevention knowledge indicators (i.e., adult illiteracy rate, access to radio, TV and mobile phone, and knowledge toward HIV), and hand hygiene indicators (i.e., travel time to water sources, a place for handwashing, and soap or detergent availability for handwashing) were higher in rural areas than Addis Ababa. All these factors increase the likelihood of COVID-19 infection in rural areas.
Spelling mistake in figure 4.
Response: Thank you. This has been now corrected.
I think Risk of death model needs to be renamed to better describe what it captures (i.e. service preparedness/access to care). Otherwise, it should also include indicators for severity as this is a strong predictor of death. Consequently, discussion around this model will need revising.
Response: As it is presented in Figure 1, the risk of death model contains not only service preparedness and access to care but also all severity indicators including comorbidity indicators, behavioral indictors, and demographic indicators. All these indicators measured the risk of death.
References
1. Weiss DJ, Nelson A, Gibson H, et al. A global map of travel time to cities to assess inequalities in accessibility in 2015. Nature 2018;553(7688):333-36.
2. Ayenew B, Yitayew M, Pandey D. Challenges and opportunities to tackle COVID-19 spread in Ethiopia. Journal of Peer Science 2020;2(2)
3. Rubin D, Huang J, Fisher BT, et al. Association of social distancing, population density, and temperature with the instantaneous reproduction number of SARS-CoV-2 in counties across the United States. JAMA network open 2020;3(7):e2016099-e99.

4. Coşkun H, Yıldırım N, Gündüz S. The spread of COVID-19 virus through population density and wind in Turkey cities. Science of The Total Environment 2020;751:141663.
5. Roy S, Ghosh P. Factors affecting COVID-19 infected and death rates inform lockdown-related policymaking. PloS one 2020;15(10):e0241165.
6. Diop BZ, Ngom M, Biyong CP, et al. The relatively young and rural population may limit the spread and severity of Covid-19 in Africa: a modelling study. BMJ global health 2020;5(5):e002699.
7. Kubota Y, Shiono T, Kusumoto B, et al. Multiple drivers of the COVID-19 spread: role of climate, international mobility, and region-specific conditions. medRxiv 2020

VERSION 2 – REVIEW

REVIEWER	Bernard Ekumah University of Cape Coast, Ghana
REVIEW RETURNED	07-Jan-2021

GENERAL COMMENTS	The revised manuscript has seen a tremendous improvement. The authors have addressed my comments however, I need clarifications on two issues: 1. The authors used PCA to create an index but some of the variables included in the PCA were count data. Is it appropriate to run PCA on count variables? 2. The authors used kriging to interpolate some data to generate the vulnerability maps. One of the conditions for running kriging is the distribution of the data. Were data normally distributed? if not, how was this addressed.

REVIEWER	Leah Rosenkrantz Simon Fraser University, Canada
REVIEW RETURNED	30-Dec-2020

GENERAL COMMENTS	Thank you for addressing my comments. This version is much improved! Only one minor comment: there are grammatical errors on line 138 and 384. If these can be reviewed and fixed, that would be great.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1: Mohamed A Daw; Institution and Country: Department of Medical Microbiology &Immunology, Faculty of Medicine, University of Tripoli, Tripoli LIBYA
Comments to the Author
Accept

Reviewer 5: Dr. Leah Rosenkrantz, Simon Fraser University
Comments to the Author:
Thank you for addressing my comments. This version is much improved!
Response: We thank you for the positive feedback and your valuable time to review our work for the second time.
Only one minor comment: there are grammatical errors on line 138 and 384. If these can be reviewed and fixed, that would be great.
Response: These grammatical errors are now corrected in the revised version of the manuscript on

line 138 and 384.

Reviewer 2: Dr. Bernard Ekumah, University of Cape Coast

Comments to the Author:
The revised manuscript has seen a tremendous improvement. The authors have addressed my comments however, I need clarifications on two issues.
Response: Thank you for your valuable feedback and taking the time to review our work twice. We have addressed all the comments and have revised the manuscript carefully.

1. The authors used PCA to create an index but some of the variables included in the PCA were count data. Is it appropriate to run PCA on count variables?
Response: Since the variables had different units of measurement, the datasets were first normalised to a standard scale. After normalisation, the indicators were used to create PCA.

2. The authors used kriging to interpolate some data to generate the vulnerability maps. One of the conditions for running kriging is the distribution of the data. Were data normally distributed? if not, how was this addressed.
Response: In the Kriging interpolation, the basic condition for the truth is the stationary assumption. Normalizing the distribution of data can only improve the conditions for the stationary assumption. Therefore, the normal distribution of data is not a necessary condition for the kriging technique1. In our analysis, we applied universal kriging for the interpolation, which relaxes the assumption of stationarity by allowing the mean of the values to differ in a deterministic way in different locations2.

Reference
1. Bagheri Bodaghabadi M. Is it necessarily a normally distributed data for kriging? A case study: soil salinity map of Ghahab area, central Iran. Desert 2018;23(2):284-93.
2. Bivand RS, Pebesma EJ, Gomez-Rubio V, et al. Applied spatial data analysis with R: Springer 2013.