nature portfolio

Peer Review File

The role of climate and population change in global flood exposure and vulnerability

Corresponding Author: Dr Justin Rogers

Parts of this Peer Review File have been redacted as indicated to remove third-party material.

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Overall, this is a well written paper that is well structured, and has a strong argument and message. I am pleased to have reviewed it. However, I am some what struggling with the methods as I am not convinced that some of the physical processes have been fully thought through. The methods are vague, and as a coastal modeller, I can see many gaps, which then also come across in the results. Some of the results I have heard before, so the originality is the story line needs to come across stronger. There are other references that could be cited to support this work (maybe the authors have reached the reference limit?) - doi:10.1017/cft.2023.3 is a prime example.

Main. This is well written and presents a strong argument throughout. It is well structured and justified.
Line 40-47. Check the use of the word 'risk' here when referring to flood models. Is it flood risk or flood impacts?

Figure 1 and caption. SSP should be in capitals, please check throughout. I find the axis labels hard to work out. Imagine if this was a single Powerpoint slide – could you understand what the axis titles mean? Please can a clearer title, writing in full rather than abbreviations, be added. Consistency in decimal places in each sub-plot is needed. I am wondering whether the scale in (c) should be the same as (b) to demonstrate the difference better – can you give this a go and see whether this has a greater impact / better communicates the key message you would like these figures to bring?

Figure 2. I have some issues with the coastal diagram. North of Adelaide, there is a very square piece of land that comes up as inundated area. How is this inundated by the sea? Where is the tidal limit here? What is the hydrological connectivity? This looks odd. These lakes are salt pans, but what does that mean in terms of inundation from the sea? Another point here is the Caspian Sea. This is an inland sea, so is controlled by different physical mechanisms, mostly through land run off. How has this been considered in modelling with climate change (I see the methods note that present trends are extrapolated)? There are other inland seas with the same issue. What's going on in Canda? Large swaths of inundation – have you considered land uplift? Also think about Brazil -- yes, this has a long tidal range, but how has this been incorporated into the analysis, and other physical processes such as the backwater effect? What about defences? Much of the Netherlands, Germany and the UK are inundated in the figure – but they aren't today, so will they in the future? These examples make me think that the authors have not considered the physical mechanisms of flooding have not been thought through. I therefore question the validity of the findings here for the entire paper.

Figure 3. Please add the caption and legend in so it is a stand alone figure. The pattern I see here is broadly what I would expect to see (at least on the coast), but some global North countries are not showing as high as expected? Is this because vulnerability is accounted for here? I'm not finding it clear.

Figure 4. This is an interesting figure which I am trying to unpick. What would be helpful here is to have a figure illustrating the population change in each region (raw numbers). Africa is driven by population change (this is already well known). Asia can go up or down depending on population – and critically China, which isn't mentioned in the manuscript. I bit more digging is needed in here to explain the results more.

Figure 5. This figure is too complex. I don't understand it.

Figure 6. There is a lot of information in this figure. This is where there is a lack of questioning of what vulnerability means

(see comment in methods below). I am struggling here as vulnerability is very much based on what the authors defined as vulnerable, but not what might happen in practice.

Conclusions. I have seen similar conclusions published before (albeit some at regional scales), so I won't say that these are big break through areas. The results for population being the largest driver, are based largely on the scenario that has been taken forward.

Methods. I find the methods too vague to follow. For instance in the coastal part (lines 304-316) are described, with valid components listed. However, there is no detail as to how this information is combined and used. Where is the uncertainty? What methods did you use to combine data and how? There is no mention on the geoid or GIA. Furthermore, on line 337 you mention you modify the HAND algorithm, but why and how? Additionally, on combining data, you mention you use the maximum depth for each pixel (line 349). Is this over the same input data? The same models? Why is it valid to do this? Why the maximum – is this truly representative? What uncertainty is there around these numbers to provide robustness in the findings? How was the data combined? There authors go onto say they have undertaken extensive validation of the data sets (line 350), but don't say what this is. There are lots of missing pieces here. As a reader I sadly don't really understand in detail what the authors have done, so the methods are not reproducible in practice. The authors go on from line 353 to explain this, but it feels like some kind of expert knowledge is missing, rather than manipulating data sets. To me, this is a major flaw in the paper as I cannot be confident in how the results are derived. This really comes through when looking at Figure 2, as the methods and expert view would have picked up the issues that I have identified in the coastal area there.

I am not an expert in the population and GDP, so cannot comment on specifics here.

Vulnerability (line 411). There are many definitions of vulnerability. These need to be acknowledged.

The authors say that the data will be added to Zotero upon publication (line 430). This is a good move. It would have been good to see this data during the review process too, to better understand the meaning and methods in the data presented.

Reviewer #2

(Remarks to the Author)
Review of the manuscript by Rogers etal. "The role of climate and population change in global flood exposure and vulnerability" submitted to Nature Communications.

The presented manuscript analyzes the effect of flood hazard changes for fluvial, pluvial and coastal flooding driven by climate change and population growth in floodplain on the population exposure at the global scale. Though some regional and global analysis of changes in flood risk exist, the presented work fills a research niche by focusing on changes in exposed population. After analyzing the projected changes in climate and population growth, the authors attributed changes in exposed population to these two drivers. The manuscript is very well written and structured. The analysis methodology is clearly described, except for the flood hazard model.

My major concern with the presented research is the fact that the central modelling tool used for assessment of flood hazard from fluvial, pluvial and coastal flooding is unpublished, commercial closed-source model. The reference for the model (L84, Ref. 1) – a grey-literature non-peer-reviewed Swiss Re report, which does not present any details on the model – is inappropriate. The methodological description (L295ff) does not contain any technical information and does not present any model validation results. Though I believe, an insurance company would seek the best possible model implementation, the formal assurance of scientific credibility of the presented results is not provided here, neither with the reference to a grey-literature report, nor with a brief verbal description of the overall modelling approach. In the worst case, any model producing any numbers can be used in such a way to deliver some results. An example of research that subsequently builds upon published and validated models, which are probably similar to those used in the presented study, is given by the work of Bates et al. (2021) and analyses mounting upon it. I am afraid, we cannot deviate from the established process of securing scientific soundness and reproducibility of the results through peer-reviews. On these grounds I must regretfully reject the otherwise very interesting manuscript. Maybe the authors can consider using the hazard layers from the model by Bates et al. (2021). In this way, the presented approach would be valid and quality assured.

Another concern I would like to share is the way how the final attribution statement is articulated (L22-23, L165-167, L252-255). The incremental attribution approach taken in this study (L398-399) decomposes the total change in exposed population into three components: (1) change in exposed population due to climate change (increased hazard) given the population in the baseline scenario, (2) change in exposed population due to population growth given the baseline hazard and (3) additional increase in exposed population due to the simultaneous effect of both the increasing inundation areas for a 100yr flood and the projected population growth. Important is here to mention upon which condition, the incremental change is calculated, i.e. "given the baseline hazard/baseline population". At least in the discussion of the results and conclusion, a more detailed and profound description of these numbers should be given.

References

Bates, P. D. , Quinn, N. , Sampson, C. , Smith, A. , Wing, O. , Sosa, J. , et al. (2021). Combined modeling of US fluvial, pluvial, and coastal flood hazard under current and future climates. Water Resources Research, 57, e2020WR028673. https://doi.org/10.1029/2020WR028673

Reviewer #3

(Remarks to the Author)
This paper presents a comprehensive analysis of global flood risk, addressing the increasing challenges posed by climate change impacts on flooding and population growth. The aim of this study is to employ a high-resolution flood model integrating pluvial, fluvial and coastal impacts in order to assess the flood exposure worldwide.
The research process is well outlined. The introduction emphases the rise challenge of future flood risk and the rise of in loses and damages connected. The framework is explained and essential process steps are presented in detail. The methodology and the investigations are well established. The manuscript certainly provides insights to flood risk researchers and flood modelers. However, several points must be clarified and several questions must be answered before this draft can be approved for publication.
The main concerns are:
• It’s unclear and arguable, if a resolution of 90 m is sufficient to model and assess pluvial flood risks, as pluvial flooding are happening at local scale typically associated with convective high-intensity, short durations storms.
• Which regions could be modelled with a higher resolution using a finer DEM?
• How are the three different flooding types (pluvial, fluvial, and coastal) modelled in an integrated manner? It seems that the combined flood prediction simply takes the maximum of the separate model results.
• Which hydraulic model is used for the fluvial flood simulations?
• Furthermore, please discuss in detail the limitations of the methodology. Which limitations arise from the independent modeling strategy?
• The discussion lacks statements and references of other studies. Try to interpret your methods and results in a more global context as well as to other studies including references.
• Some of the figures are too small and hard to read (Figure 5, Figure 2, Figure 1)

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Thank you for an extremely thorough response to my comments. It is very much appreciated. I have checked each of my comments below. I have not specifically tracked the other reviewers’ comments.

I remain concerned that the Methods are vague (and are not elaborated on in the Supplementary Material). Other impact (or risk) models have whole papers dedicated to this, so it’s important that there is sufficient information there. This includes why a certain approach was taken, what works and what doesn’t, how data gaps are dealt with etc. I would particularly like to see more detail on the lakes, levees, wind/tropical cyclones. Please may you also rethink whether this is the best title of the paper given the changes you have made to the manuscript (e.g. you say it considers all three hazards, vulnerability is hard to define). I think the title could sell to USP of your work more, making it more attracting to others.

Explanation on layout below: Each number represents the ordering as present in your rebuttal and I have added a few words on the original comment so it is clear to track.

1. Thank you for taking the time to review this manuscript….

The methods have been substantially improved, but I feel still need further work in its description. It is a substantial amount of work to get these models work together and correctly, and the challenge does feel adequately captured. For example, from line 786 there is no sense of scale or resolution involved, if or how data sets match up to each other and detailed post-processing needed.

I still have a big issue with lakes. These are not coastal. Can they be put in a separate category? They, like inland seas operate under different physical mechanisms. There is a lack of detail here on how the data was resolved, how many lakes there are, the types of lake and whether the same physical mechanism to represent one lake type can be used for another. The reader just gets passed to another reference.

Are winds tropical cyclones included in the same way as lakes (near the coast) and the coast? How to winds / tropical storms relate to the extreme water levels? This is not described in sufficient detail.

There is a lack of information about the coverage of the levee data. What about countries (or parts of countries) where there is no data – do they just use the global river data from the Netherlands? What about coasts? How do the national and global datasets compare? How did you combine this data. NB: The UK and England appear to be muddled.

Line 929 – are these predictions or scenarios? Another vague sentence is: ‘This is based on an approach of flood risk based on the probability of occurrence' What 'approach' do the authors mean here? I'm not really sure.

I have found examples of similar papers with similar messaging for each separate hazard (or combined but undertaken by different modelling teams so are not so well integrated). As the authors say, it is hard to distinguish a paper that has brought this altogether. However, the key messages have been previously presented elsewhere – why is this paper more than the sum of its parts? What is the value of the combinations (and potential double counting or where are there potentially riskier areas from all three hazards – I think this could help sell the key differences more.

Figure 2 – this is very insightful, thank you. Please could GDP be written in capitals as it should be?

2. Main. This is well written and presents

N/A

3. Line 40-47. Check

Thank you.

4. Figure 1 and caption. SSP should be in capitals,

Thank you for the revised Figure 2. It is much better. Can you note what the uncertainty represents please.

5. Figure 2. I have some issues

I very much appreciated the issues with the political boundaries. However, this still does not fully address my concern about how lakes are handled in the data analysis (see comment 1). I am still not sure from your response on the hydrological connectivity of the saltpan, and whether all lakes concerns are really coastal. This is important as the physical mechanisms of flooding will differ, which provides very mixed messages when merged with the coastal result. If lake levels have not considered climate change, this needs to be made much clearer in text as lake levels could also vary for many other reasons, including engineering ones.

6. What's going on in Canda? Large

Thank you for the explanation. This is the prime point of confusion of lakes v coasts. In the methods, please ensure it is clear the GIA is integrated into the SLR component. Terminology here is also important – it should be relative SLR not SLR.

7. Also think about Brazil –

Thank you.

8. What about defences?

See comment 1 above. There is still too many gaps in the explanation here as to how these are used, compared, gaps, missing data etc.
For coastal areas, it is highly likely that defences will get upgraded as sea-levels rise. It is worth adding one sentence in text just acknowledging this, otherwise the evolving risk factor with time isn't being fully considered.

9. These examples make me

Thank you for the detailed explanation and effort made in addressing my above concerns. It is much appreciated. I still remained concerned about the methods section, as I feel further detail is needed to describe how the model was brought together (e.g. resolution, data challenges etc), as I still struggle to see the reproducibility within the method. My particularly concerns are with lakes, levees and wind/tropical cyclones.

10. Figure 3. Please add

Thanks for the explanation here. Please can you be really clear what exposure means here (e.g. add 1 sentence). Other journal papers have it purely as a headcount with no defence. I think yours includes the defence – is that correct?

11. Figure 4. This is an interesting figure

Thanks – you're got some great figures in the manuscript.

12. Figure 5. This figure

N/A.

13. Figure 6. There is a lot of information

Thank you.

14. Conclusions. I have seen

Re originality – please see comment under comment 1.

15. Methods. I find the methods

I still feel this could be stronger / more detail as I can't seem to bring together the method in my head. Please see comment 1 above.

16. Where is the uncertainty?

Thank you.

17. What methods did you

I still find this vague. Please see comment 1.

18. There is no mention

Thanks.

19. Furthermore, on line 337 you

Thanks.

20. Additionally, on combining

Thanks. Getting the combination is hard to do as the events don't happen often / physical mechanisms differ, so being pragmatic is sometimes all that can be done!

21. What uncertainty is there

Thank you.

22. There authors go onto

Thank you.

23. There are lots of missing pieces here

Please see comment 1. I still feel the methods and approach could be more detailed. Other authors dedicate whole papers to their methods, so this is much shorter in comparison!

24. I am not an expert in the

Thanks – much appreciated.

25. The authors say that

Thank you.

Reviewer #2

(Remarks to the Author)
Review of the manuscript by Rogers et al. „The role of climate and population change in global flood exposure and vulnerability"

I review the presented manuscript for the second time after the authors carried out significant revision. Indeed, the description of the models was too sparse in the original manuscript and a wrong reference caused confusion. The authors considerably extended the Method section and improved the overall presentation. In particular, the model validation is highly appreciated. The manuscript provides a valuable insight into the dynamics of population exposure to coastal, fluvial and pluvial flooding on a global scale.

I have one major issue and a few minor comments which need attention. If properly addressed, I would be happy to recommend the manuscript for publication.

Major comment:

L882-893: The description of the inundation model is still not sufficient in my view, in particular it is not clear how the 1D model is linked to the inundation algorithm. If I understand correctly, the inundation model is based on water spreading algorithm following HAND DEM (ref. 45) which is not a mass-conservative. Hence, the available flood volume is not considered. How are then water levels computed in 1D model in each 90-m cell/cross-section replenished into floodplains? Without mass conservation, the inundation model would require some constraints on inundation extent, i.e., how far the water level from 1D cell is replenished downstream following the hydraulic connectivity. How this constrain is technically imposed? How exactly the propagation away from the channel is computed (L84-85)?

After these details are clarified, the authors should clearly state the limitation of the inundation model to be a non-hydrodynamic, non-mass conservative (as far as I understand), which may potentially kead to overestimation of inundation areas.

Minor comments:

L21: …exposed to the 1% annual risk (100-year) of flooding …
The 100-year flood area the model computes does not correspond to a single flood event, but is a spatial composite of multiple potential 100-year events produced by steady-state flood modeling.

L29: risk exposure -> flood exposure

L45-46: the figures can be updated for recent years
https://www.swissre.com/institute/research/sigma-research/sigma-2024-01.html

L59: Is the reference #18 a correct one to support this statement? This is a very technical paper on parallelized model development.

L66 and L68: I feel the references might be messed up here. In L66 you are talking about fluvial floods, but reference a paper on pluvial flood risks. Same for L68.

L87-89: B people or billion people? Make it consistent here and elsewhere.

L140: a reference is needed here.

L202-203: name a couple of those regions as examples.

L207-209: unclear sentence.

L301: uncertainty in 100-year flood area

L309: check the references.

L405: flood risk -> flood hazard (see hazard, exposure, risk definition by IPCC).

L429: joint correlation -> joint effect of CC and PC

L432: 100-year flood area (see my comment to L21).

L465: should it read PEu?

L842: do you mean references 37-38?

Reviewer #4

(Remarks to the Author)
The manuscript examines the relative contribution of climate and population change to global flood exposure, considering coastal, fluvial, and pluvial flood hazards. The work is important and advances our knowledge of flood exposure at a global scale. The authors competently executed somewhat complicated tasks with careful logic and modeling. Like any modeling study, the current study has some limitations, most of which are acknowledged by the authors. While I do not think new analysis is needed, the authors consider addressing the following issues, which need further clarifications.

1. The vertical spatial resolution of DEM needs to be clarified. Given that the authors used 10cm as the threshold depth for pluvial flooding (line 437), I presume the authors may have used a vertical spatial resolution finer than the threshold depth. However, I cannot find that information in the manuscript.

2. Considering the depth and spatial extent of pluvial flooding are associated with storm infrastructure variables such as storm pipes particularly in urban areas, how did the authors consider these variables in modeling pluvial flooding?

Additionally, it is unclear if any green stormwater infrastructure variables were used to model pluvial flooding. If not, caveats of the current modeling approach should be noted in the discussion section.

3. Flood defense structures such as levees appeared to be considered from only a few Western countries (lines 395-400). Given that other countries in Australia, East Asia, and Europe have such defense structures, it would be prudent to include them from these countries for fluvial modeling. Otherwise, fluvial flooding potential can be overestimated.

4. Population estimates in urban regions are underestimated since the authors used the same population growth rate for all regions within each country. Considering that urbanization will continue with rural-to-urban migration in the coming decades, the authors consider the caveats of the current assumption when estimating flood exposure in urban regions.

5. While generally well-written and easy to follow, the manuscript has a few places that need clarifications or trimming.
a. Lines 107-108. Is there a supporting reference for this statement?
b. Lines 146-148. A confusing statement.
c. Figure 2. Y axes units should be added to each figure.
d. Lines 304-306. These statements are already mentioned previously.
e. Line 368. What inundation model was used?
f. Consider using past tense when reporting methods. (e.g., line 386, 387, 389, 406, etc.)

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Many thanks on putting in significant effort to answer my questions and queries. I am happy that you have answered by questions and would like to thank you for being really clear with your answers and providing additional methods in the Supplementary Material. It's a very paper good paper.

I continue to be concerned, and at times confused in the lakes and coastal analysis. I feel that they should be presented separately to avoid confusion given that they have different physical drivers. That clarity can really help. I'm a coastal scientist and I have previously seen the two confused or incorrect analysis being undertaken in reports or journal paper reviews. I appreciate that this is nothing to do with you, but it is reflective of the scientists that you are communicating with. Overall, I feel the communication of this should be clearer throughout, such as in the figures where they are still referred to as 'coastal' rather than stating they are lakes and open coast. This is to avoid confusion (e.g. if you were to present this in a Powerpoint slide or read the figure without the caption – which does happen – is the key message getting across?).

Reviewer #2

(Remarks to the Author)
I am pleased all my concerns and remarks have beed satisfactorily addressed in the last revision round. I happy to recommend the manuscript for publication.

Reviewer #4

(Remarks to the Author)
I appreciate the authors' endeavors put into the revisions. The authors adequately addressed all of my comments.

Response to Review Comments

The role of climate and population change in global flood exposure and vulnerability

Justin S. Rogers, Marco M. Maneta, Stephan R. Sain, Luke E. Madaus, Joshua P. Hacker

We wish to thank the three reviewers for their comments and suggestions for improving the manuscript. The suggested changes have been made wherever possible.
A detailed reply to the referee's specific comments follow (in *italic* font). In addition, we have included a revised manuscript with track changes to easily highlight revisions to the document. Changes to the document were made throughout based on reviewer feedback.

Reviewer #1 (Remarks to the Author):

Overall, this is a well written paper that is well structured, and has a strong argument and message. I am pleased to have reviewed it. However, I am some what struggling with the methods as I am not convinced that some of the physical processes have been fully thought through. The methods are vague, and as a coastal modeller, I can see many gaps, which then also come across in the results. Some of the results I have heard before, so the originality is the story line needs to come across stronger. There are other references that could be cited to support this work (maybe the authors have reached the reference limit?) - doi:10.1017/cft.2023.3 is a prime example.

> ***Author Response:*** *Thank you for taking the time to review this manuscript. We have significantly expanded the methods section to better describe the flood model. The model uses well-accepted methods and is based on publicly available data, which is detailed in the methods section. We have also added a new supporting information document with detailed flood model validation. The flood model results compare well to the comparison datasets. We hope that this expansion is sufficient to describe the modeling framework and methods.*
>
> *We have also streamlined the text so that the key message comes across stronger. To our knowledge this is the first study showing population exposure to floods under climate change from coastal, fluvial, and pluvial flooding at a global scale. Other works have certainly completed components of this (regional models, or only one flood hazard) but not all together.*
>
> *In the revised manuscript, we now include uncertainty estimates in the summary flooding and exposure results (Figure 2). As shown by the results, the uncertainty in the flood model (Figure 2a) dominates the present-day 2020 uncertainty in exposure (Figure 2c). However, for future 2100 estimates, the population (Figure*

2b) dominates the uncertainty in the exposure. This means that for evaluating changes to exposure by the year 2100, which is the primary result of the paper, the population changes are highly uncertain and the flood results less so. This implies that as long as the flood model results are reasonable, we have confidence that the exposure results are reasonable within the margins of error. This, combined with the validation comparisons for the flood model, and comparison studies on exposure, gives us additional confidence in the results.

Thank you for pointing us to the Reimann 2023 reference, we have included it and others in the revised document.

Main. This is well written and presents a strong argument throughout. It is well structured and justified.

Author Response: *Thank you.*

Line 40-47. Check the use of the word 'risk' here when referring to flood models. Is it flood risk or flood impacts?

Author Response: *Thank you, we have revised to use "flood impact".*

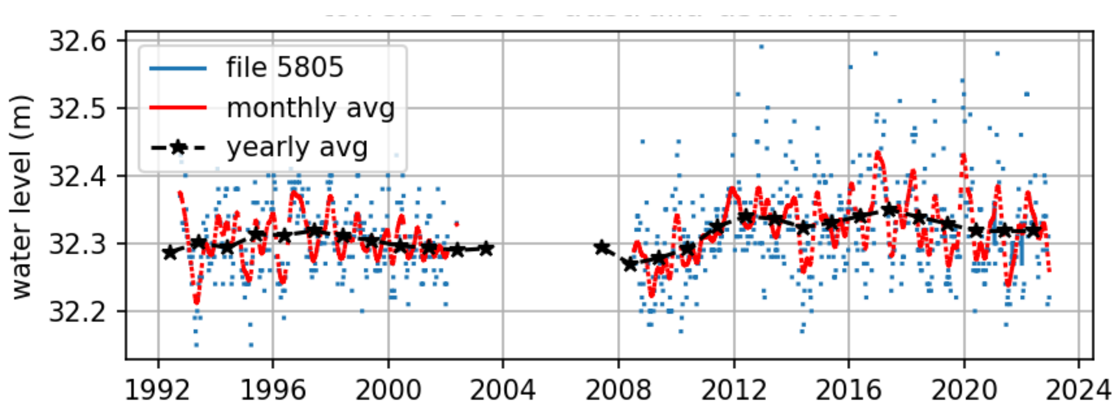
Figure 1 and caption. SSP should be in capitals, please check throughout. I find the axis labels hard to work out. Imagine if this was a single Powerpoint slide – could you understand what the axis titles mean? Please can a clearer title, writing in full rather than abbreviations, be added. Consistency in decimal places in each sub-plot is needed. I am wondering whether the scale in (c) should be the same as (b) to demonstrate the difference better – can you give this a go and see whether this has a greater impact / better communicates the key message you would like these figures to bring?

Author Response: *This is an excellent point, the original figure was hard to read. Please see revised (now) Figure 2. We have reorganized this figure to focus on (a) flooding with respect to coastal, fluvial, pluvial sources, (b) population changes in urban and non-urban areas, and (c) population exposure across a range of variables including flood source, urban setting, and gdp. Additionally, we have included uncertainty in these estimates, which better constrains the results.*

Figure 2. I have some issues with the coastal diagram. North of Adelaide, there is a very square piece of land that comes up as inundated area. How is this inundated by the sea? Where is the tidal limit here? What is the hydrological connectivity? This looks odd. These lakes are salt pans, but what does that mean in terms of inundation from the sea?

Author Response: *The global flood results are presented as the average inundation over aggregate political boundaries. So, these are not the 90m flood* *results themselves. We have added a new Figure 1 to illustrate this difference and walk the reader through the analysis methods. The 90m flood results are visible at the local scale of New York City in Figure 1a, however at a global scale are very difficult to see in a single figure. Therefore, we have opted to show the percent inundated area aggregated over political boundaries shown in example Figure 1c over the globe to illustrate both flood inundation in Figure 3 and changes to flood inundation in Figure 4. The blocky nature of the results in Figures 3 and 4 arises from the political boundaries. We have noted this difference in the Figure captions.*

Specifically, regarding north of Adelaide in Figure 3a, there are some lake beds which are typically dry, but may contain shallow water seasonally based on our data from the G-REALM database (see figure below). Here, the model approximates the 100-year lake levels as 10-20 cm depth. In terms of population impacts, this area is not highly populated, so note that population exposure from coastal flood is very low (Figure 4a). Based on this information, we believe the results in this region are reasonable.

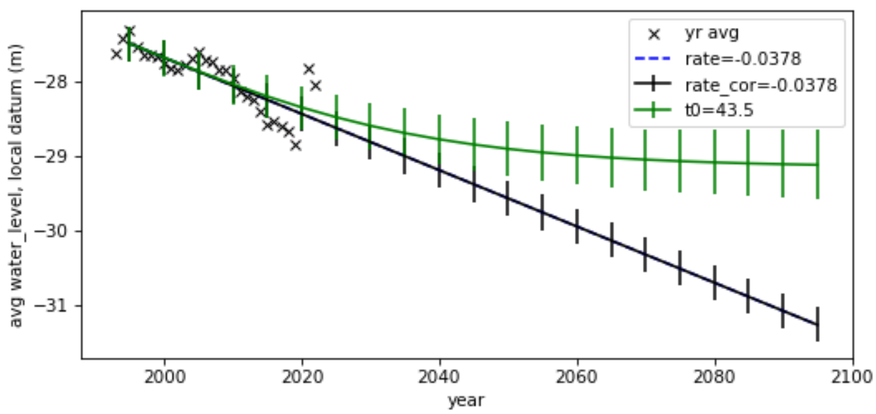


Lake Torrens Australia, water level elevation (m) USDA G-REALMS.

Another point here is the Caspian Sea. This is an inland sea, so is controlled by different physical mechanisms, mostly through land run off. How has this been considered in modelling with climate change (I see the methods note that present trends are extrapolated)? There are other inland seas with the same issue.

Author Response: *Inland lakes and seas are included in the coastal flood model, and the long-term lake levels are based on projections using historical data trends. See figure below for the Caspian Sea based on G-REALM data, showing the linear projection (black) and smoothed/reduced projection (green) which is assumed for the coastal flood model. Climate scenarios are not included in the inland lake estimates in the current version of this model but are an area* *for future development. As you correctly point out, land-based runoff, as well as other processes including evaporation, precipitation, etc., will all come into play here which makes for a complex problem. To our knowledge, no databases currently exist for global lake level projections including climate change but would be a welcome development. Therefore, our approach has been to model these lakes with a reasonable approach which is tractable at a global scale.*

We have included the following text in the methods: flood modeling section, "Long-term lake levels are projected based on historical trends and variability (G-REALM)[43]. Specifically, the long-term lake levels follow the recent trends in the near term, but which are gradually reduced to zero trend towards the end of century. This is to account for uncertainty in forecast tends using historical data projections. Thus, the coastal flood model contains climate change signals from sea level rise and changing winds, but inland lake levels are based on historical trends without climate scenarios, which is an avenue of future development."



Caspian Sea Lake Levels (m) showing annual average (x), linear projection (black), and smoothed/reduced projection used for model (green), G-REALM.

What's going on in Canda? Large swaths of inundation – have you considered land uplift?

Author Response: *Here, we assume this comment relates to coastal flood. Primarily, this stems from the same comment as above for Australia – these results are the percent inundation averaged over the political areas. In northern Canada, there are an abundance of lakes and coastal facing areas. Additionally, the political boundaries in northern Canada are quite large, which leads to the blocky nature of the inundated area for coastal flood in Figure 3a. See example in Figure 1 of the revised manuscript.*

The IPCC AR6 Sea Level Rise estimates included in the coastal model do include land movement relative to the ocean. Specifically in arctic regions, *continental rebound effects lead to lower than average sea level rise in these areas. For example, Churchill Canada shows declining sea levels for SSP2-4.5.*

Figure Redacted

IPCC AR6 Sea Level Projections for Churchill, Canada for SSP2-4.5 (from https://sealevel.nasa.gov/ipcc-ar6-sea-level-projection-tool)

Also think about Brazil -- yes, this has a long tidal range, but how has this been incorporated into the analysis, and other physical processes such as the backwater effect?

> ***Author Response:*** *Yes, tides are included in the GTSR storm surge model used in this model, and in the observational database used to debias the coastal water levels (GESLA-3). These are included at a global scale and do have coverage in Brazil, where some regions have large tidal ranges. Backwater effects are considered in the fluvial flood model, the following sentence is included in the methods flood modeling section, "Backwater effects from boundary conditions at the ocean are considered at high tide."*

What about defences?

> ***Author Response:*** *Flood defenses are included in the coastal and fluvial flood models. The following is added to the methods section, "Flood defenses are included in the algorithm based on datasets from the US National Levee Database (https://nld.sec.usace.army.mil/), the Netherlands (https://www.opendelve.eu/), the UK (UK Environment AIMS 2020 Spatial Flood*

Defences), France (Bureau de Recherches Géologiques et Minières, Flood Zoning – 2020 Report) and Germany (local government databases Baden-Württemberg, Bremen, Lower Saxony). The defense logic is based on a return period protection level under baseline conditions. For a given protection level, areas behind a levee area protected, but if levels increase in a future climate, levee is assumed to overtop."

Much of the Netherlands, Germany and the UK are inundated in the figure – but they aren't today, so will they in the future?

> ***Author Response:*** *We assume this comment is referring to coastal flooding. As shown in Figure 3a, the coastal areas of Netherlands, Germany and UK do have some inundated areas on average for the political areas shown. Flood defenses are included in the model in these locations, and many areas have coastal flood defenses at the 100-year level, but some do not. Changes to coastal flooding are shown in Figure 4a, which shows increasing areas of inundation up to 2% for these coastal regions. This is primarily driven by sea level rise, with changing winds playing a secondary role.*

These examples make me think that the authors have not considered the physical mechanisms of flooding have not been thought through. I therefore question the validity of the findings here for the entire paper.

> ***Author Response:*** *We believe we have addressed the concerns raised above to better document the strong mechanistic basis for the coastal flood model. We have provided more detailed explanation of the modeling methods used in the manuscript. Additionally, we believe the clarification of Figures 3 and 4 average area inundated rather than the 90m flood depth is an important distinction, and now better clarified in the new Figure 1. Additionally, we have added extensive model validation comparisons in a supporting information document and summarized in the results section. As we have discussed above and in the manuscript, the coastal flood model contains a strong mechanistic basis and is based on well-established methods and data, and compares well to external validation data.*

Figure 3. Please add the caption and legend in so it is a stand alone figure. The pattern I see here is broadly what I would expect to see (at least on the coast), but some global North countries are not showing as high as expected? Is this because vulnerability is accounted for here? I'm not finding it clear.

> ***Author Response:*** *We have added the legend description to now Figure 5. These results are population exposure for 2020 (panel a) and change in population exposure (panel b). Units here are people per km2. This result is only about exposure to flooding, not vulnerability. Due to the color scheme used in the original manuscript, some of the changes in exposure did not show very well. We*

have modified the color scheme to better highlight the changes. As shown in the revised figure, there are changes in exposure in the global north (Figure 5b). North America, UK, and Northern Europe are increasing, while central Europe and Eastern Asia are decreasing.

Figure 4. This is an interesting figure which I am trying to unpick. What would be helpful here is to have a figure illustrating the population change in each region (raw numbers). Africa is driven by population change (this is already well known). Asia can go up or down depending on population – and critically China, which isn't mentioned in the manuscript. I bit more digging is needed in here to explain the results more.

Author Response: *Thank you for your suggestions here, and we have modified this analysis. First, we have now included the spatial information for total population in 2020 (Figure 3e) and the expected spatial change in population by 2100 (Figure 4e) to illustrate the geographic changes in population. As noted, China is facing strong population declines, while Africa and South Asia face strong population increases. The results section now reads, "which can vary regionally with eastern Asia expecting population declines, while Africa and South Asia expecting population increases (Figure 4e)".*

Secondly, we have modified now Figure 6 to show these terms spatially rather than summarized by continental region. This new approach now highlights the differences within regions where for example China shows increasing climate effects (Figure 6a), but decreasing population (Figure 6c), and decreasing joint CC/PC (Figure 6d). We have updated the results section to reflect these changes.

Figure 5. This figure is too complex. I don't understand it.

Author Response: *Agreed, this figure is complex. Our intent is to break down the changes in population exposure within different geographic types and regions to highlight where important changes are occurring. However, the geographic plots showing spatially the changes in exposure in Figure 5 and Figure 6 are the central result and likely sufficient. We have moved this figure and the accompanying results text to the supporting information section.*

Figure 6. There is a lot of information in this figure. This is where there is a lack of questioning of what vulnerability means (see comment in methods below). I am struggling here as vulnerability is very much based on what the authors defined as vulnerable, but not what might happen in practice. Maybe remove vulnerability and just stick with extreme event sensitivity?

Author Response: *Agreed, the measure of vulnerability here is somewhat adhoc and resilience is a complex subject. In light of this, we have moved the discussion to focus on low- medium- and high-gdp regions and their relative* *population exposure. This draws the same basic conclusions, but without making assumptions about vulnerability. We have removed the vulnerability metric from the document, figure and methods as suggested. Additionally, we have slightly revised the "most sensitive" category definition to be the lowest 1/3 sensitive areas, where previously this was the lowest ¼ areas. This is to be consistent with the new definition of low- medium- and high-GDP regions also defined by 1/3 quantiles. The results section now reads, "The global increase in exposure between 2020 and 2100 is primarily driven by low-GDP regions, and low-GDP regions also face a disproportionally high population exposure compared to high-GDP regions (Figure 2c)."*

Conclusions. I have seen similar conclusions published before (albeit some at regional scales), so I won't say that these are big break through areas. The results for population being the largest driver, are based largely on the scenario that has been taken forward.

Author Response: *While some components of this study have been published before, to our knowledge no prior work has undertaken a global analysis of climate attribution vs population attribution including a comprehensive flood analysis from multiple hazard types. Therefore, we believe our work is significant and worthy of publication.*

The results for population do dominate the exposure results, and alternate population projection would affect the result. Our approach here was to take the middle of the road climate scenario SSP2-4.5 and medium population projections and look at the relative magnitude. Additional iterations could explore the variation between population and climate scenarios. The following has been added to the discussion, "Additionally, a single population scenario (UN medium) and climate scenario (SSP2-4.5) are assumed in this work to illuminate the broad trends, but further study could explore the range of variation between assumed projections."

Methods. I find the methods too vague to follow. For instance in the coastal part (lines 304-316) are described, with valid components listed. However, there is no detail as to how this information is combined and used.

Author Response: *The methods section for the coastal model has been expanded significantly. We are also limited by word count, but we have done our best to include more details within these limits. We now introduce the model framework and then detail sequentially how the model is constructed. "The basis of the model is at coastal points globally, whereby a series of sequential operations are conducted to compute the return-period based water levels for a range of years and climate scenarios. These coastal water levels are then passed to the inundation model to compute depths of flooding on the grid (see below)." Similar statements have been added to the fluvial and pluvial model paragraphs.*

Where is the uncertainty?

> ***Author Response:*** *This is a great point, and our initial manuscript did not include the effects of uncertainty, although we had computed these. Now, the manuscript includes uncertainty quantification of both the flood results and exposure, which significantly enhances the manuscript. We have also detailed in the methods section where the uncertainty in the flood results comes from.*

What methods did you use to combine data and how?

> ***Author Response:*** *We have significantly expanded the coastal flood methods section to illustrate the modeling in a sequential way. Hopfully this is what the reviewer was envisioning.*

There is no mention on the geoid or GIA.

> ***Author Response:*** *The following sentence has been added, "Finally, datums are adjusted from mean sea level to EGM96 framework using the Mean Dynamic Topography[44]".*

Furthermore, on line 337 you mention you modify the HAND algorithm, but why and how?

> ***Author Response:*** *The methods section has been rewritten to clarify this and now reads, "For fluvial flood, we apply the HAND algorithm to simulate propagation away from river channels. For coastal flood, HAND simulates flood propagation inland from the coastline, where we had added a constant energy dissipation term to reduce storm surge and tides linearly with inland propagation."*

Additionally, on combining data, you mention you use the maximum depth for each pixel (line 349). Is this over the same input data? The same models? Why is it valid to do this? Why the maximum – is this truly representative?

Author Response: *The following has been added to the methods section, "The combined flood prediction takes the maximum coastal, fluvial, and pluvial flood depth at each pixel for a given return period, year and climate scenario. This is based on an approach of flood risk based on the probability of occurrence. Because we are evaluating flood risk from coastal storm surge events, small-scale high-intensity pluvial flood events, and large-scale medium-intensity fluvial events, these processes are on average independent. Coincident event types are rare, but certainly possible, leading to compound flood hazards. In general, these joint hazards can increase flood levels in areas where hazards overlap. Thus, these results may underestimate flood levels in some selected areas. While modeling compound events is possible computationally at regional scales[10], it is currently not feasible at global scales, and is a topic of future development."*

What uncertainty is there around these numbers to provide robustness in the findings? How was the data combined?

> ***Author Response:*** *We have provided uncertainty quantification in the revised manuscript. See Figure 2 and Figure 7. As noted above, the relative uncertainty in the population projections is much greater than the relative uncertainty in the flood projections (for future years). This implies that while the flood results are important to the exposure estimates, they are less important than the population projections.*

There authors go onto say they have undertaken extensive validation of the data sets (line 350), but don't say what this is.

> ***Author Response:*** *We have added significant new results to the paper to back up this statement. There is a new Supporting Information document with detailed flood model validation. We have also summarized this in the results (flood inundation changes).*

There are lots of missing pieces here. As a reader I sadly don't really understand in detail what the authors have done, so the methods are not reproducible in practice. The authors go on from line 353 to explain this, but it feels like some kind of expert knowledge is missing, rather than manipulating data sets. To me, this is a major flaw in the paper as I cannot be confident in how the results are derived. This really comes through when looking at Figure 2, as the methods and expert view would have picked up the issues that I have identified in the coastal area there.

> ***Author Response:*** *As we have explained above, the results presented are average inundate aggregated over political areas, which seems to have led to confusion about what we were presenting. With the new Figure 1 illustrating this connection between the 90m native flood depth results and the aggregate flood inundation and exposure, this is hopefully clearer. We have significantly expanded the methods section in further detail as requested, and included uncertainty estimates. We have also added flood validation information, and highlighted the population exposure validation information that was in the original manuscript. Both the flood model validation and population exposure validation compare well to external datasets and studies, which lend confidence in our results.*

I am not an expert in the population and GDP, so cannot comment on specifics here. Vulnerability (line 411). There are many definitions of vulnerability. These need to be acknowledged.

> ***Author Response:*** *A statement has been added to this line, "While there are many definitions of vulnerability, here we define the relative vulnerability as…"*

The authors say that the data will be added to Zotero upon publication (line 430). This is a good move. It would have been good to see this data during the review process too, to better understand the meaning and methods in the data presented.

> ***Author Response:*** *The data has been uploaded at the following link so it is available for the review process: https://drive.google.com/file/d/1AI0Q_awa1YaA2w7HZrzOHXEgoqlqd8RA/view?usp=sharing. Once the paper is accepted, a permanent data link will be available on zenodo.*

Reviewer #2 (Remarks to the Author):

Review of the manuscript by Rogers etal. "The role of climate and population change in global flood exposure and vulnerability" submitted to Nature Communications.

The presented manuscript analyzes the effect of flood hazard changes for fluvial, pluvial and coastal flooding driven by climate change and population growth in floodplain on the population exposure at the global scale. Though some regional and global analysis of changes in flood risk exist, the presented work fills a research niche by focusing on changes in exposed population. After analyzing the projected changes in climate and population growth, the authors attributed changes in exposed population to these two drivers. The manuscript is very well written and structured. The analysis methodology is clearly described, except for the flood hazard model.

> ***Author Response:*** *Thank you for your review and comments on the manuscript. We have expanded the flood hazard model description significantly.*

My major concern with the presented research is the fact that the central modelling tool used for assessment of flood hazard from fluvial, pluvial and coastal flooding is unpublished, commercial closed-source model. The reference for the model (L84, Ref. 1) – a grey-literature non-peer-reviewed Swiss Re report, which does not present any details on the model – is inappropriate. The methodological description (L295ff) does not contain any technical information and does not present any model validation results. Though I believe, an insurance company would seek the best possible model implementation, the formal assurance of scientific credibility of the presented results is not provided here, neither with the reference to a grey-literature report, nor with a brief verbal description of the overall modelling approach. In the worst case, any model producing any numbers can be used in such a way to deliver some results. An example of research that subsequently builds upon published and validated models, which are probably similar to those used in the presented study, is given by the work of Bates et al. (2021) and analyses mounting upon it. I am afraid, we cannot deviate from the established process of securing scientific soundness and reproducibility of the results through peer-reviews. On these grounds I must regretfully reject the otherwise very interesting manuscript. Maybe the authors can consider using the hazard layers from the model by Bates et al. (2021). In this way, the presented approach would be valid and quality assured.

> ***Author Response:*** *We believe this statement was based on a misinterpretation of Line 84 referring to reference #1, while in reality it was meant as footnote #1, which is located at the bottom of the page. We sincerely apologize for this typesetting/formatting issue which has been corrected. The source of our flood hazard model is described in detail in the methods section on Flood Modeling and is not a commercial closed source model. The flood modeling methods section has been expanded in the revised manuscript. All the data and methods used for the flood modeling are based on well-known approaches and publicly available datasets as described. Any novel approaches are described in the methods section.*
>
> *Additionally in the revised manuscript we now include an extensive supporting information section on validation of the flood model, and discussion in the Flood Results section. Here we compare the flood results to three separate datasets: FEMA river elevations in the US, GESLA coastal water levels globally, and USGS high water marks for two high-intensity rainfall events. Overall, the flood model results compare well to the validation datasets, giving confidence that the flood hazard results are reasonable. We could expand the comparison to Bates (2021), but as this is a model itself, we believe the validation comparisons provided sufficiently establish the flood results as reasonable. We have added the Bates (2021) paper as a reference throughout.*
>
> *In the revised manuscript, we now include uncertainty estimates in the summary flooding and exposure results (Figure 2). As shown by the results, the uncertainty in the flood model (Figure 2a) dominates the present-day 2020 uncertainty in exposure (Figure 2c). However, for future 2100 estimates, the population (Figure 2b) dominates the uncertainty in the exposure. This means that for evaluating changes to exposure by the year 2100, which is the primary result of the paper, the population changes are highly uncertain and the flood results less so. This implies that as long as the flood model results are reasonable, we have confidence that the exposure results are reasonable within the margins of error.*

Another concern I would like to share is the way how the final attribution statement is articulated (L22-23, L165-167, L252-255). The incremental attribution approach taken in this study (L398-399) decomposes the total change in exposed population into three components: (1) change in exposed population due to climate change (increased hazard) given the population in the baseline scenario, (2) change in exposed population due to population growth given the baseline hazard and (3) additional increase in exposed population due to the simultaneous effect of both the increasing inundation areas for a 100yr flood and the projected population growth. Important is here to mention upon which condition, the incremental change is calculated, i.e. “given the baseline hazard/baseline population”. At least in the discussion of the results and conclusion, a more detailed and profound description of these numbers should be given.

> ***Author Response:*** *Thank you for your suggestion here. We have modified the discussion and abstract as follows. The discussion now reads, "Of this increase in population exposure given the baseline flood hazard and population, approximately 21.1% is due to climate change, 76.8% is due to population change and 2.1% is due to both climate and population changing simultaneously in a given area. This change in exposure is based on the relative increase from the baseline 2020 exposure." The abstract now reads, "Of this change from the baseline 2020 exposure, we attribute…"*

References

Bates, P. D. , Quinn, N. , Sampson, C. , Smith, A. , Wing, O. , Sosa, J. , et al. (2021). Combined modeling of US fluvial, pluvial, and coastal flood hazard under current and future climates. Water Resources Research, 57, e2020WR028673. https://doi.org/10.1029/2020WR028673

Reviewer #3 (Remarks to the Author):

This paper presents a comprehensive analysis of global flood risk, addressing the increasing challenges posed by climate change impacts on flooding and population growth. The aim of this study is to employ a high-resolution flood model integrating pluvial, fluvial and coastal impacts in order to assess the flood exposure worldwide.

The research process is well outlined. The introduction emphases the rise challenge of future flood risk and the rise of in loses and damages connected. The framework is explained and essential process steps are presented in detail. The methodology and the investigations are well established. The manuscript certainly provides insights to flood risk researchers and flood modelers. However, several points must be clarified and several questions must be answered before this draft can be approved for publication.

> ***Author Response:*** *Thank you for the thoughtful review, please see detailed responses below.*

The main concerns are:

• It's unclear and arguable, if a resolution of 90 m is sufficient to model and assess pluvial flood risks, as pluvial flooding are happening at local scale typically associated with convective high-intensity, short durations storms.

Author Response: *See changes in Methods: Flood Modeling, where the text now reads, "In locations within the USA and Europe where DEM data are available at 10 m resolution, the pluvial model is run at the native higher resolution and then downsampled to 90 m. Elsewhere, the model is run at 90 m resolution, which may underestimate pluvial flooding in dense urban areas in the FSM method. In these cases, the rational method provides a reasonable flooding result for the overland flooding over the pixel on average."*

• Which regions could be modelled with a higher resolution using a finer DEM?

Author Response: *Regions modeled at 10-m natively are the USA and Europe where 10 m DEM data exists.*

• How are the three different flooding types (pluvial, fluvial, and coastal) modelled in an integrated manner? It seems that the combined flood prediction simply takes the maximum of the separate model results.

Author Response: *See methods: Flood Modeling, Combined, the text now reads, "The combined flood prediction takes the maximum coastal, fluvial, and pluvial flood depth at each pixel for a given return period, year and climate scenario. This is based on an approach of flood risk based on the probability of occurrence. Because we are evaluating flood risk from coastal storm surge events, small-scale high-intensity pluvial flood events, and large-scale medium-intensity fluvial events, these processes are on average independent. Coincident event types are rare, but certainly possible, leading to compound flood hazards. In general, these joint hazards can increase flood levels in areas where hazards overlap. Thus, these results may underestimate flood levels in some selected areas. While modeling compound events is possible computationally at small scales, it is currently not feasible at global scales, and is a topic of future development."*

• Which hydraulic model is used for the fluvial flood simulations?

Author Response: *See Methods, Flood modeling, which now reads, "Finally, the fluvial process employs a 1D river channel model to solve for the river depth, using a hydraulic model based on a quasisteady state form of the St. Venant equations, local river geometry cross-sections at 90 m spacing, Manning's roughness, and stream flows. The quasi-steady state form drops the local acceleration from the inertial terms but keeps the convective acceleration term. Boundary conditions at the ocean are considered at high tide."*

• Furthermore, please discuss in detail the limitations of the methodology. Which limitations arise from the independent modeling strategy?

Author Response: *see discussion above on combined flooding.*

• The discussion lacks statements and references of other studies. Try to interpret your methods and results in a more global context as well as to other studies including references.

> ***Author Response:*** *Excellent observation. We have added additional references to the discussion and added more global context to the results.*

• Some of the figures are too small and hard to read (Figure 5, Figure 2, Figure 1)

> ***Author Response:*** *We agree, some of the original figures were too small and hard to read. We have increased the size of the figures for readability through simplifying the layout and splitting more complex figures into multiple. Please see revised document.*

Response to Review Comments

The role of climate and population change in global flood exposure and vulnerability

Justin S. Rogers, Marco M. Maneta, Stephan R. Sain, Luke E. Madaus, Joshua P. Hacker

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We wish to thank the three reviewers for their comments and suggestions for improving the manuscript. The suggested changes have been made wherever possible.
A detailed reply to the referee's specific comments follow (in *italic* font). In addition, we have included a revised manuscript with track changes to easily highlight revisions to the document. Changes to the document were made throughout based on reviewer feedback.

Reviewer #1 (Remarks to the Author):

Thank you for an extremely thorough response to my comments. It is very much appreciated. I have checked each of my comments below. I have not specifically tracked the other reviewers' comments.

> ***Author Response:*** *Thank you for taking the time to review this manuscript again and for your thoughtful comments to improve the paper.*

I remain concerned that the Methods are vague (and are not elaborated on in the Supplementary Material). Other impact (or risk) models have whole papers dedicated to this, so it's important that there is sufficient information there. This includes why a certain approach was taken, what works and what doesn't, how data gaps are dealt with etc. I would particularly like to see more detail on the lakes, levees, wind/tropical cyclones.

> ***Author Response:*** *Based on your feedback as well as the other reviewers, we have expanded the model details significantly in the requested areas, especially focusing on the coastal flood model components of lakes, levees and winds. We have expanded the information on lakes, areas of defense coverage and wind effects. The effects of winds are shown to be quite small on coastal flood due to relatively small changes in wind forcing under future climate scenarios. We are at our limit from Nature for the length of the methods section, and while we could include additional information in the Supplementary Information, we believe the reader should be able to understand the core methods from the main paper.*
>
> *There are some complex elements to the model, however, everything is based on well-established guidance and methods as described. Our results compare well to flood hazard validation observations and other studies (see*

Supplementary Information). And our results for flood exposure compare reasonably well to previous studies (see Results, Population exposed). These results give confidence that the aggregate effect of the complex model is on the right track.

Finally, the coastal flood component of exposure is very small relative to fluvial and pluvial flood (Figure 2). Lakes are a very small fraction of the coastal component. Thus, small differences in the coastal model methodology will not affect the main conclusions on global population exposure. Hopefully, the revised document addresses your concerns.

Please may you also rethink whether this is the best title of the paper given the changes you have made to the manuscript (e.g. you say it considers all three hazards, vulnerability is hard to define). I think the title could sell to USP of your work more, making it more attracting to others.

Author Response: *Thank you for the suggestion regarding the title and the unique selling point of this paper. We have added additional language to the discussion to distinguish this work (see below). After considerable deliberation, we have decided to leave the title as is, with the intent of having a concise description of the primary purpose of the work, which is to focus on the population exposure results from climate change. We feel that adding the flood hazard components to the title focuses too much on the methods and detracts from this core message.*

Explanation on layout below: Each number represents the ordering as present in your rebuttal and I have added a few words on the original comment so it is clear to track.

1. Thank you for taking the time to review this manuscript….

The methods have been substantially improved, but I feel still need further work in its description. It is a substantial amount of work to get these models work together and correctly, and the challenge does feel adequately captured. For example, from line 786 there is no sense of scale or resolution involved, if or how data sets match up to each other and detailed post-processing needed.

Author Response: *Based on the comments below, we have significantly expanded the methods section and added two new figures to the supplementary information. Figure SI8 now shows the coastal ocean and lake points globally, which have variable spatial resolution. We have described the operations in a sequential manner, in the order they are conducted in the model. The coastal model is all computed on the coastal ocean and lake points (Figure SI8) to compute the total water level values. There is some limited spatial interpolation where SLR is assigned to the coastal points (as they have a higher resolution).*

Otherwise, all datasets are co-located. As discussed in the methods, these points are then passed to the HAND method to compute the inundation on the 90m grid.

At the same time, we need to balance the word limit of the methods and give equal weight to the various model components. While these comments are focused on the coastal flood model, it represents a very small component of the overall flood exposure globally, which is primarily dominated by inland floods and population changes (see Figure 2). Therefore, while documenting the coastal flood methods are important, they should not come at the expense of the rest of the model components which we believe should receive equal weight.

I still have a big issue with lakes. These are not coastal. Can they be put in a separate category? They, like inland seas operate under different physical mechanisms. There is a lack of detail here on how the data was resolved, how many lakes there are, the types of lake and whether the same physical mechanism to represent one lake type can be used for another. The reader just gets passed to another reference.

Author Response: *Inland seas and lakes operate on a continuum from very large scales which are indeed very similar to coastal ocean conditions (i.e. US Great Lakes, Caspian Sea, etc.) where wave processes and wind-driven storm surge are important. Smaller lakes also have these processes, but they are generally small effects, while seiches, and inter-annual water level variability dominate. Long-term water level variations are indeed different between the ocean (driven by sea level processes), and lakes (driven by freshwater volume balance), and we model these effects separately as described in the methods. For the short-term variability, while different processes dominate in different ocean and lake settings, our general approach is data-driven, extreme value analysis which is broadly applicable to a range of physical mechanisms. For lakes, we are using the direct observations to inform the extreme value analysis, and thus we have high confidence in these extreme value estimates. The expanded methods section is now reorganized and hopefully clearer.*

We have included a new figure SI8 in the supplementary information which shows the location of all the coastal ocean and lake boundary points in the model. We have also noted in the text that 656 lakes are modeled, and 14,979 coastal ocean points are included in the model.

It is common to use the term "coastal" to describe any type of waterline along a water body, so we have opted to keep this descriptor in the text for both ocean and lake settings. However, to clarify the methods we have added significantly more detail and explicitly defined "coastal ocean" as separate from "coastal lake" and described these separately, and then how the two are combined. Please see methods section for the new edits.

Are winds tropical cyclones included in the same way as lakes (near the coast) and the coast? How to winds / tropical storms relate to the extreme water levels? This is not described in sufficient detail.

> ***Author Response:*** *Yes, winds are applied in the same way to coastal ocean and lake settings for the short-term extreme values. We have added the following language to the methods section to further expand on this point,*
>
> *"Parameterized climate effects of changing winds on short-term extreme values were applied to both coastal ocean and lake points, with the assumption that wind energy is the primary driver of short-term water level fluctuations arising from a variety of mechanisms including wind driven storm surge, wave setup, seiches, etc. This assumption may under- or over-estimate changes in extreme values locally, however due to weak climate trends for future wind conditions, the impact on the global 100-year coastal water levels is small with no long-term trend and less than 10% variation. In tropical cyclone-prone areas there is a 4% average increase by 2100 and less than 20% variation (see Supplementary Information Figure SI9)."*
>
> *While the parameterized approach here is approximate, the overall trend in winds over the next century are quite weak, with only modest changes in TC prone regions. This means the implications of this decision are not impactful to the overall coastal flood result. Further, because coastal flood is the smallest component of global population exposure (Figure 2), these effects are not consequential to the overall conclusion of the paper.*

There is a lack of information about the coverage of the levee data. What about countries (or parts of countries) where there is no data – do they just use the global river data from the Netherlands? What about coasts? How do the national and global datasets compare? How did you combine this data. NB: The UK and England appear to be muddled.

> ***Author Response:*** *We have expanded the details on flood defenses in the methods section. We have clarified which regions have full coverage, and which are partial. Thanks for the note on UK vs England, and indeed the coverage here is England only. The flood defenses contain both coastal ocean and river defenses, which are included in the same database.*
>
> *We are unclear what you mean by national and global datasets. We are only including coverage in the USA, Netherlands, Germany, UK, France, based on the datasets noted in the text. The OpenDelve dataset does include some redundant regions of coverage, but we opted to use the region-specific data if there was a conflict. The section now reads,*

“Flood defenses for rivers and coasts were included in the algorithm based on datasets covering the USA, (US National Levee Database, https://nld.sec.usace.army.mil/), the Netherlands (https://www.opendelve.eu/), portions of the UK (UK Environment AIMS 2020 Spatial Flood Defences for England), France (Bureau de Recherches Géologiques et Minières, Flood Zoning – 2020 Report) and portions of Germany (local government databases Baden-Württemberg, Bremen, Lower Saxony). Flood defense information outside these regions was not available at the time of model development but will be included in future model updates when available. This assumption implies potentially overpredicted flood inundation in regions where flood defenses exist on the ground but not in the model. The defense logic was based on a return period protection level under baseline conditions.”

Line 929 – are these predictions or scenarios? Another vague sentence is: ‘This is based on an approach of flood risk based on the probability of occurrence’ What ‘approach’ do the authors mean here? I’m not really sure.

Author Response: *Agreed, these were poorly worded.*

The phrase “flood predictions” has been changed to “flood model result.”

And the phrase “This is based on…” has been changed to “The assumption of maximum depth is based on the probability of occurrence of a flood hazard”

I have found examples of similar papers with similar messaging for each separate hazard (or combined but undertaken by different modelling teams so are not so well integrated). As the authors say, it is hard to distinguish a paper that has brought this altogether. However, the key messages have been previously presented elsewhere – why is this paper more than the sum of its parts? What is the value of the combinations (and potential double counting or where are there potentially riskier areas from all three hazards – I think this could help sell the key differences more.

Author Response: *It is important to distinguish this paper from the rest. Our primary contribution here is a comprehensive high-resolution assessment of future global population exposure to flooding, based on detailed modeling from coastal, fluvial, and pluvial sources. While other groups have contributed components of these analysis, the full picture provides the most accurate assessment to date of global flood exposure. The last paragraph of the introduction contains this unique selling point. We have added the following to the discussion to highlight these ideas,*

“The results presented here are based on the extent of flood inundation and population exposure from coastal, fluvial, and pluvial sources between 2020 and 2100, and to our knowledge, the first time this has been done at a global scale applied to population exposure.”

“The results of this study are, to our knowledge, the most comprehensive assessment of future global population exposure to floods. They highlight…”

Finally, regarding the double counting, that is a valid point. Other studies considering only one flood type may overpredict exposure when combined with separate studies. We have added the following sentence to the discussion,

“By presenting the flood exposure from components of flood risk (coastal, fluvial, pluvial) as well as combined flood, we avoid potential double counting of exposure when only one flood risk type is considered, or multiple studies are aggregated.”

Figure 2 – this is very insightful, thank you. Please could GDP be written in capitals as it should be?

Author Response: *Glad you like the new format. We have changed the notation to “GDP” as suggested.*

2. Main. This is well written and presents

N/A

3. Line 40-47. Check

Thank you.

4. Figure 1 and caption. SSP should be in capitals,

Thank you for the revised Figure 2. It is much better. Can you note what the uncertainty represents please.

Author Response: *The following has been added to the caption for Figure 2, “Uncertainty bands represent the 5^{th}/95^{th} percentile flood exposure (i.e. the 5^{th} percentile flood hazard with the 5^{th} percentile population projection).”*

5. Figure 2. I have some issues

I very much appreciated the issues with the political boundaries. However, this still does not fully address my concern about how lakes are handled in the data analysis (see comment 1). I am still not sure from your response on the hydrological connectivity of the saltpan, and whether all lakes concerns are really coastal. This is important as the physical mechanisms of flooding will differ, which provides very mixed messages when merged with the coastal result. If lake levels have not considered climate change, this needs to be made much clearer in text as lake levels could also vary for many other reasons, including engineering ones.

Author Response: *We have expanded the methods section on the coastal model to clearly distinguish how the coastal ocean and lakes are treated. As noted above, we feel the term "coastal" is widely used as applicable for both ocean and lake coastlines, and therefore the model description is acceptable. The methods section now clearly separates how we handle each regime, and how they are combined. We have also added the following sentence to the methods, "This is to account for uncertainty in forecast tends using historical data projections, which can arise from the climate but also human water use."*

6. What's going on in Canda? Large

Thank you for the explanation. This is the prime point of confusion of lakes v coasts. In the methods, please ensure it is clear the GIA is integrated into the SLR component. Terminology here is also important – it should be relative SLR not SLR.

Author Response: *As noted above, hopefully the new organization is clearer. Regarding SLR, the following sentence is now included,*

"We then included relative sea level rise projections from the IPCC AR6 projections[28] *(which include estimates of local land movement, glacial isostatic adjustment, thermal expansion, water storage, etc.)."*

7. Also think about Brazil –

Thank you.

8. What about defences?

See comment 1 above. There is still too many gaps in the explanation here as to how these are used, compared, gaps, missing data etc.

For coastal areas, it is highly likely that defences will get upgraded as sea-levels rise. It is worth adding one sentence in text just acknowledging this, otherwise the evolving risk factor with time isn't being fully considered.

Author Response: *We believe we have described our flood defense data sources in detail with cited sources and coverage areas. Areas outside of these coverage areas are considered undefended. In terms of how they are used, the following is included in the methods,*

*"The defense logic was based on a return period protection level under baseline conditions (*i.e. *overtopping occurs if water levels exceed the baseline protection level)."*

Regarding future adaptation, that is a great point. We added the following sentence to the methods, *"Additionally, we do not consider future climate adaptation measures or improvements to existing flood defenses in the model."*

9. These examples make me

Thank you for the detailed explanation and effort made in addressing my above concerns. It is much appreciated. I still remained concerned about the methods section, as I feel further detail is needed to describe how the model was brought together (e.g. resolution, data challenges etc), as I still struggle to see the reproducibility within the method. My particularly concerns are with lakes, levees and wind/tropical cyclones.

> ***Author Response:*** *Based on your feedback as well as the other reviewers, we have expanded the model details significantly in the requested areas. Hopefully this addresses your concerns. There are some complex elements to the model, however, everything is based on well-established guidance and methods. Our results compare well to flood hazard validation observations and other studies (see Supplementary Information). And our results for flood exposure compare reasonably well to previous studies (see Results, Population exposed). These results give confidence that the aggregate effect of the complex model is on the right track.*
>
> *Finally, the coastal flood component of exposure is very small relative to fluvial and pluvial flood (Figure 2). Small differences in the coastal model methodology will not affect the main conclusions on global population exposure.*

10. Figure 3. Please add

Thanks for the explanation here. Please can you be really clear what exposure means here (e.g. add 1 sentence). Other journal papers have it purely as a headcount with no defence. I think yours includes the defence – is that correct?

> ***Author Response:*** *We have added the following sentence to the Results: Population exposed section where Figure 5 is introduced,*
>
> *"Here, we define exposure as the number people at risk of the 100-year flood hazard, considering flood defenses where available in the model (see methods)."*

11. Figure 4. This is an interesting figure

Thanks – you're got some great figures in the manuscript.

12. Figure 5. This figure

N/A.

13. Figure 6. There is a lot of information

Thank you.

14. Conclusions. I have seen

Re originality – please see comment under comment 1.

> ***Author Response:*** *Thank you for your feedback.*

15. Methods. I find the methods

I still feel this could be stronger / more detail as I can't seem to bring together the method in my head. Please see comment 1 above.

> ***Author Response:*** *Please see response above.*

16. Where is the uncertainty?

Thank you.

17. What methods did you

I still find this vague. Please see comment 1.

> ***Author Response:*** *Please see response above.*

18. There is no mention

Thanks.

19. Furthermore, on line 337 you

Thanks.

20. Additionally, on combining

Thanks. Getting the combination is hard to do as the events don't happen often / physical mechanisms differ, so being pragmatic is sometimes all that can be done!

> ***Author Response:*** *Agreed. The joint compound hazard problem is interesting and important in some places. However, at scale it's not tractable to solve with our current methodology and computing resources. So we need a pragmatic approach.*

21. What uncertainty is there

Thank you.

22. There authors go onto

Thank you.

23. There are lots of missing pieces here

Please see comment 1. I still feel the methods and approach could be more detailed. Other authors dedicate whole papers to their methods, so this is much shorter in comparison!

> ***Author Response:*** *Please see response above.*

24. I am not an expert in the

Thanks – much appreciated.

25. The authors say that

Thank you.

Reviewer #2 (Remarks to the Author):

Review of the manuscript by Rogers et al. „The role of climate and population change in global flood exposure and vulnerability“

I review the presented manuscript for the second time after the authors carried out significant revision. Indeed, the description of the models was too sparse in the original manuscript and a wrong reference caused confusion. The authors considerably extended the Method section and improved the overall presentation. In particular, the model validation is highly appreciated. The manuscript provides a valuable insight into the dynamics of population exposure to coastal, fluvial and pluvial flooding on a global scale.

I have one major issue and a few minor comments which need attention. If properly addressed, I would be happy to recommend the manuscript for publication.

> ***Author Response:*** *Thank you for taking the time to review the manuscript again and provide valuable feedback. We believe we have addressed your comments, see below.*

Major comment:

L882-893: The description of the inundation model is still not sufficient in my view, in particular it is not clear how the 1D model is linked to the inundation algorithm. If I understand correctly, the inundation model is based on water spreading algorithm following HAND DEM (ref. 45) which is not a mass-conservative. Hence, the available flood volume is not considered. How are then water levels computed in 1D model in each 90-m cell/cross-section replenished into floodplains? Without mass conservation, the inundation model would require some constraints on inundation extent, i.e., how far the water level from 1D cell is replenished downstream following the hydraulic connectivity. How this constrain is technically imposed? How exactly the propagation away from the channel is computed (L84-85)?

> ***Author Response:*** *This is an important question, and it was not clear in the manuscript how we addressed this issue in the model. We have expanded the the methods section on Floodplain Inundation which now reads,*
>
> *"The floodplain inundation model used for both fluvial and coastal floods was based on the Height Above Nearest Drainage (HAND) algorithm[45], which applies flood levels of constant elevation along flow paths. For fluvial flood, we applied the HAND algorithm to simulate propagation away from river channels. The classic application of HAND does not conserve mass because it is typically applied using water elevations obtained from calibrated rating curves at the channel that do not adequately capture the geometry of the floodplain and its effect on energy losses. This can cause unrealistic flood extents. To resolve this limitation, we used the valley-averaged channel and floodplain cross-section to calculate the flow cross-sectional area in the 1D river channel equation. The resulting flow depth (for a given channel slope and roughness) produces an average flow cross-section that when multiplied by the reach length conserves mass (i.e. conservation of mass would be perfect if the floodplain cross-section was constant along the reach length). This causes HAND flood extents to be consistent with mass balance constraints. Discrepancies in the mass balance are introduced when the valley geometry deviates from its average shape at different points along the reach. Thus, the method conserves mass on average at a reach scale, but there is some scatter for individual cross-sections. While it contains limitations, this methodology is a significant improvement in the application of the HAND method in rivers."*

After these details are clarified, the authors should clearly state the limitation of the inundation model to be a non-hydrodynamic, non-mass conservative (as far as I understand), which may potentially kead to overestimation of inundation areas.

> ***Author Response:*** *We have added the following statement to the methods section on Inundation to clarify this issue:*
>
> *"Thus, the inundation model can be characterized as non-hydrodynamic, mass-conserving at a river reach scale (for fluvial flood), and non-mass conserving energy-based (for coastal flood). These approximations may lead to differences in inundation at a local scale compared to high-resolution dynamical models, but*

on average, results compare well to external validation data (see Supplementary Information).”

Minor comments:

L21: …exposed to the 1% annual risk (100-year) of flooding …

The 100-year flood area the model computes does not correspond to a single flood event, but is a spatial composite of multiple potential 100-year events produced by steady-state flood modeling.

> ***Author Response:*** *Agreed, the 1% risk is at each pixel and does not represent an “event”. This is a challenge to explain to a broad audience in a simple way. We have changed the description of the “100-year flood event” to the “100-year flood hazard” to address this. The noted line now reads,*
>
> ***“…****the population likely exposed to 1% annual risk (100-year) flood hazard will increase…”*

L29: risk exposure -> flood exposure

> ***Author Response:*** *The suggested change has been made.*

L45-46: the figures can be updated for recent years

https://www.swissre.com/institute/research/sigma-research/sigma-2024-01.html

> ***Author Response:*** *Thank you for this updated reference. We have added the following sentence to the introduction,*
>
> *“Losses from storms and floods have steadily increased over the past decades, with losses in 2017 costing the global economy over 140 billion USD[1], and the subsequent four out of five years (2018 through 2023) having losses over 100 billion USD annually[2]”*

L59: Is the reference #18 a correct one to support this statement? This is a very technical paper on parallelized model development.

> ***Author Response:*** *Agreed, this reference does tackle the high-resolution modeling question, but not a discussion on pros and cons of model approaches. We updated the reference to Bates et al (2021), which does have an extended discussion on this topic.*

L66 and L68: I feel the references might be messed up here. In L66 you are talking about fluvial floods, but reference a paper on pluvial flood risks. Same for L68.

Author Response: *Agreed, somehow the references got mixed up for this sentence and have now been fixed. The fluvial flood reference is Dottori et al (2016), and the pluvial reference is Rosenzweig et al (2018). This is one aspect of the Nature format that we don't like, it makes for easy reading but does not highlight the authors as well and it's easy to incorrectly attribute a work.*

L87-89: B people or billion people? Make it consistent here and elsewhere.

Author Response: *Good catch. We have now used "billion people" consistently throughout the document.*

L140: a reference is needed here.

Author Response: *We have added seven references here with relevant papers.*

L202-203: name a couple of those regions as examples.

Author Response: *This sentence now reads, "In some large basins with a general drying trend, such as central Asia and central North America, event intensification cannot overcome the hydrologic deficit to generate more flooding."*

L207-209: unclear sentence.

Author Response: *Agreed, this was poorly worded. The sentence now reads, "The net effect of combined coastal, fluvial, and pluvial flooding is broad flood risk over the globe (Figure 3d) and increasing inundated area in most regions of the world, with some exceptions (Figure 4d)."*

L301: uncertainty in 100-year flood area

Author Response: *The sentence now reads, "In 2020, the relative uncertainty in the 100-year flood area (Figure 2a) is greater than the relative uncertainty in population (Figure 2b); and thus flooding is the largest driver in exposure uncertainty for the present time."*

L309: check the references.

Author Response: *Reference has been fixed.*

L405: flood risk -> flood hazard (see hazard, exposure, risk definition by IPCC).

Author Response: *Yes, this was incorrectly noted as "flood risk." In this paragraph we are specifically looking at changes in population exposure to floods, so the sentence now reads, "On average, the changes in PE from climate are positive, indicating increasing exposure for populations within the floodplain."*

L429: joint correlation -> joint effect of CC and PC

__Author Response:__ Suggested change has been made.

L432: 100-year flood area (see my comment to L21).

__Author Response:__ "100-year flood event" has been changed to "100-year flood hazard"

L465: should it read PEu?

__Author Response:__ Great catch, suggested change has been made.

L842: do you mean references 37-38?

__Author Response:__ Yes, the intended references here are the CMIP6 papers Eyring et al (2016), and O'Neill et al (2016).

Reviewer #4 (Remarks to the Author):

The manuscript examines the relative contribution of climate and population change to global flood exposure, considering coastal, fluvial, and pluvial flood hazards. The work is important and advances our knowledge of flood exposure at a global scale. The authors competently executed somewhat complicated tasks with careful logic and modeling. Like any modeling study, the current study has some limitations, most of which are acknowledged by the authors. While I do not think new analysis is needed, the authors consider addressing the following issues, which need further clarifications.

__Author Response:__ Thank you for taking the time to review this manuscript and provide valuable feedback. See below for specifics.

1. The vertical spatial resolution of DEM needs to be clarified. Given that the authors used 10cm as the threshold depth for pluvial flooding (line 437), I presume the authors may have used a vertical spatial resolution finer than the threshold depth. However, I cannot find that information in the manuscript.

__Author Response:__ The flood models have a vertical resolution of 1 cm, which is now noted in this section. The 10 cm threshold is intended to represent a transition from nuisance flooding to flooding potentially causing damage.

2. Considering the depth and spatial extent of pluvial flooding are associated with storm infrastructure variables such as storm pipes particularly in urban areas, how did the authors consider these variables in modeling pluvial flooding? Additionally, it is unclear

if any green stormwater infrastructure variables were used to model pluvial flooding. If not, caveats of the current modeling approach should be noted in the discussion section.

> ***Author Response:*** *This is an important point that was not clear in the original text. We exclude the effects of storm drain features in the pluvial model.*
>
> *The text in the discussion now reads, "We exclude the effects of storm drains or other stormwater control features in the flood models, which may overpredict flood extents in some urban areas."*
>
> *And the pluvial methods section now reads, "We exclude the effects of storm drains or other stormwater control features in the pluvial model."*

3. Flood defense structures such as levees appeared to be considered from only a few Western countries (lines 395-400). Given that other countries in Australia, East Asia, and Europe have such defense structures, it would be prudent to include them from these countries for fluvial modeling. Otherwise, fluvial flooding potential can be overestimated.

> ***Author Response:*** *This is an important limitation of the current model. We have applied flood defense information everywhere it is currently available. Despite our concerted efforts to obtain flood defense information in other countries, we have not been successful. This does imply that flood hazards are potentially overpredicted in some areas. We have included the following statement in the discussion,*
>
> *"We include flood defenses for countries where this information is available but assume undefended conditions for areas with no defense information (see Methods). This assumption implies potentially overpredicted flood inundation from coastal and fluvial sources in regions where flood defenses exist on the ground but not in the model."*
>
> *and in the methods,*
>
> *"Flood defense information outside these regions is not presently available but will be included in future model updates when available. This assumption implies potentially overpredicted flood inundation in regions where flood defenses exist on the ground but not in the model."*

4. Population estimates in urban regions are underestimated since the authors used the same population growth rate for all regions within each country. Considering that urbanization will continue with rural-to-urban migration in the coming decades, the authors consider the caveats of the current assumption when estimating flood exposure in urban regions.

Author Response: *Excellent point. We note this limitation in the discussion, which now reads,*

"The population changes used in this study assume the same spatial distribution of population density and the same population growth rate within each country. Thus, future adaptation to climate, changes to locations of urban centers, and rural-to-urban migration in the coming decades are not considered, and the urban population exposure estimates in this study are likely underestimates."

5. While generally well-written and easy to follow, the manuscript has a few places that need clarifications or trimming.

Author Response: *Thank you, see below.*

a. Lines 107-108. Is there a supporting reference for this statement?

Author Response: *References added.*

b. Lines 146-148. A confusing statement.

Author Response: *Agreed, this was very confusing. The sentence has been rewritten to read, "The net effect of combined coastal, fluvial, and pluvial flooding is broad flood risk over the globe (Figure 3d), and increasing inundated area in most regions of the world, with some exceptions (Figure 4d)."*

c. Figure 2. Y axes units should be added to each figure.

Author Response: *Units have been added. Additionally, this led us to correct Figure 2(a) which had units of M km2 in the previous version, and we have updated to be percent inundated area for consistency within the document.*

d. Lines 304-306. These statements are already mentioned previously.

Author Response: *This was repeated within the document multiple times and was redundant. We removed the duplicates so that a similar statement appears once in the results (vulnerability) and once in the discussion.*

e. Line 368. What inundation model was used?

Author Response: *The text now reads, "…passed to the HAND inundation model…"*

f. Consider using past tense when reporting methods. (e.g., line 386, 387, 389, 406, etc.)

Author Response: *Agreed, we have modified the methods section to read in past tense.*

Response to Review Comments

The role of climate and population change in global flood exposure and vulnerability

Justin S. Rogers, Marco M. Maneta, Stephan R. Sain, Luke E. Madaus, Joshua P. Hacker

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We wish to thank the three reviewers for their comments and suggestions for improving the manuscript. The suggested changes have been made wherever possible.
A detailed reply to the referee's specific comments follow (in *italic* font). In addition, we have included a revised manuscript with track changes to easily highlight revisions to the document.

Reviewer #1 (Remarks to the Author):

Many thanks on putting in significant effort to answer my questions and queries. I am happy that you have answered by questions and would like to thank you for being really clear with your answers and providing additional methods in the Supplementary Material. It's a very paper good paper.

I continue to be concerned, and at times confused in the lakes and coastal analysis. I feel that they should be presented separately to avoid confusion given that they have different physical drivers. That clarity can really help. I'm a coastal scientist and I have previously seen the two confused or incorrect analysis being undertaken in reports or journal paper reviews. I appreciate that this is nothing to do with you, but it is reflective of the scientists that you are communicating with. Overall, I feel the communication of this should be clearer throughout, such as in the figures where they are still referred to as 'coastal' rather than stating they are lakes and open coast. This is to avoid confusion (e.g. if you were to present this in a Powerpoint slide or read the figure without the caption – which does happen – is the key message getting across?).

> ***Author Response:*** *Thank you for your kind words and for the significant effort you put into reviewing this document.*
>
> *In terms of the definition of the word "coast" it is defined as "the land near a shore" (Merriam Webster Dictionary), or in common use is "the land next to the sea or the line that forms the boundary between the land and the ocean or a lake" (Wikipedia). While the term is most commonly applied to the edge of the ocean, the definition is also commonly understood to apply to the edge of a lake.*
>
> *To clarify these distinctions, we have changed the term "coastal" to "coastal ocean and lake" throughout the document to more clearly delineate our meaning here. Unfortunately, there is not room on the figure graphics for these*

descriptions, but the captions and body text have been edited which will clarify for the reader.

In terms of physical processes that impact water levels, there are differences between lakes and the ocean. Although these operate on a continuum as very large lakes such as the Caspian Sea or US Great Lakes function more like an ocean. The methods section outlines how these were modeled differently in detail. In its current form, the coastal ocean and lake model combines flood results from both sources to create the flood maps used for analysis (e.g. Figure 1). Therefore, we are not able to split the results into separate coastal and lake driven flooding without significant effort. The overall importance of coastal ocean and lake flooding is relatively small in terms of inundated area (Figure 2a) and population exposure (Figure 2c) relative to fluvial and pluvial flooding. Therefore, the distinction between ocean and lake driven flooding is not consequential to the overall message of the paper.

Reviewer #2 (Remarks to the Author):

I am pleased all my concerns and remarks have beed satisfactorily addressed in the last revision round. I happy to recommend the manuscript for publication.

Author Response: *Thank you for taking the time to review and for your recommendation to publish.*

Reviewer #4 (Remarks to the Author):

I appreciate the authors' endeavors put into the revisions. The authors adequately addressed all of my comments.

Author Response: *Thank you for taking the time to review and for your recommendation to publish.*