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Natural Hazards and Earth System Sciences
Discussions

# *Interactive comment on* "Modeled changes in 100 year Flood Risk and Asset Damages within Mapped Floodplains of the Contiguous United States" *by* Cameron Wobus et al.

**Cameron Wobus et al.**

cameron_wobus@abtassoc.com

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Dear Referees: We thank you for taking the time to review our manuscript in NHESS Discussions, entitled "Modeled changes in 100-year flood risk and asset damages within mapped floodplains of the contiguous United States." Please find below our responses to your general and specific comments on the manuscript. In all cases, reviewer comments are marked with an "RC1" or "RC2" denoting which reviewer they are addressing; and our responses are marked with an "AC", reflecting author comments. Sincerely, Cameron Wobus, on behalf of all authors

Reviewer #1 General comments



RC1: This paper considers how flood frequency and the associated flood damages might evolve depending on different greenhouse gas (GHG) emissions pathways. To my knowledge this is the first paper that proposes an automated methodology at the continental scale to estimate the potential cost of GHG emissions through their effect on flood damages. The manuscript addresses the issue of climate change in monetary terms (the cost of modeled flood damages given different emissions pathways) and as such is a timely and important contribution to the literature, and one which I expect will be of interest to a broad audience. Additionally, the authors frame the issue in a positive manner, by showing how global GHG reductions can be used to limit possible increases in flood damages.

The methods are mostly well explained. The authors begin with the mapped 1% annual exceedance probability flood extent (100-year flood) across the continental United States. They then assess how these damages will evolve under two different GHG emissions scenarios. Some aspects could be slightly better clarified (see specific comments below), but the authors are generally upfront about the limitations of the work: they state that the projections "should be considered order-of-magnitude estimates: : :" (p3L.10); they discuss the limitations of GCMs in resolving precipitation (p8), the uncertainties in the hydrological forecasts, the limited data on assets exposed to flooding, the fact that the approach does not account for the effects of changes more/less extreme floods than the 100-year flood, and that it does not take into consideration societal adaptation to flooding. However, these limitations and assumptions are discussed mainly in the conclusions, so the reader is left wondering about some of these matters (e.g. the potential influence of changes in land use) throughout most of the paper. I feel it would help to include a brief statement earlier in the paper, mentioning that the method assumes that there are no changes in land use (no additional constructions in the floodplains, no change in land cover, etc.) and therefore that the overall changes in flood hazard/damage are based mostly on climatic changes.

AC: We appreciate the need for additional detail in certain parts of the paper. We

will include a statement earlier in the paper explicitly noting the assumptions so that the reader does not need to be "left wondering" about some of these matters until the end. We have also critically reviewed our manuscript based on specific comments from the Reviewer, and we will make further clarifications to the text as requested and summarized below.

RC1: The overall approach makes sense given the continental scale of the analysis: the authors consider the distribution of results across all 29 GCMs for each RCP and compute the total number of flood events across the CONUS in each year of the model simulation. While this provides an interesting first estimate of potential future changes in flood damages at the continental scale, the uncertainties may be more problematic at the local scale. Also, as stated by the authors, this general approach is relatively conservative and thus likely underestimates the influence of potential increases in extreme precipitation.

AC: we agree and acknowledge that the uncertainties from our method are certainly larger at a local scale. We will revise the text to ensure that these uncertainties are explicit in the revised manuscript. We will also highlight these limitations in the discussion section of the manuscript.

RC1: In terms of results, I feel that the paper would benefit from a little more explanation. For instance, it is interesting that some regions (like the Southeast) are more affected by increasing flood damages under RCP8.5 than others, but there is no explanation or suggestion why.

AC: There are a range of potential reasons why different regions exhibit more dramatically increasing flood damages than others. These include differences in the climate change signal, and differences in the distribution of infrastructure within mapped 1% AEP floodplains. We recognize that the manuscript could benefit from more discussion of these nuances, and we will expand the explanation of topics such as this in the discussion section of our revised manuscript.



RC1: In sum, the paper is very well written, agreeable to read, and aptly illustrated. The technical language is appropriate, and the references are appropriate and accessible. The title and abstract are both pertinent and clear, with an appropriate and complete summary of the contents of the paper.

AC: We thank the reviewer for this positive summary of the paper.

Reviewer #1 Specific comments

RC1: P2 L.1-5. I feel that this paragraph (on climate attribution) does not fit in very well here – the narrative could be strengthened and clarified.

AC: We will expand this paragraph on revision, to provide a more coherent summary of both recent work in climate attribution and the need for long-term projections of trends in flood frequency and magnitude.

RC1: P2 L.9. Perhaps the authors could state explicitly why those two RCPs were chosen?

AC: These two RCPs loosely represent a future with little to no action on GHG mitigation (RCP8.5) and one with relatively concerted efforts to reduce GHG emissions (RCP4.5), and as such provide a good backdrop for evaluating how flood damages could be influenced by a change in emissions. These two RCPs are also being recommended for use in the Fourth National Climate Assessment. We will make all of these rationales more explicit in the revised manuscript.

RC1: P2 L.18. I think there are more recent studies on streamflow trends at the scale of the entire CONUS.

AC: We agree. While our list of examples cited here was not meant to be exhaustive, we will include more recent references on streamflow trends in the CONUS, including Tamaddun et al. (2016); Ivancic et al. (2017)

RC1: P2 L.22. "Because available hydrologic records tend to be short. . .". I feel this

sentence misses the main point of the paragraph. It seems the issue here is not that historical trends are inconclusive or that existing data records are too short (the USGS database has thousands of sites with more than 50 years of streamflow data; and existing analyses are not all inconclusive), but rather that historical trend analyses are unable to tell us much about the future, and therefore there is increasing interest in using climate model outputs to evaluate future flood risk.

AC: We thank the reviewer for pointing out the confusion from this statement. We will revise this paragraph to clarify our meaning.

RC1: P4 L.21. It's not entirely clear to me how realistic the simulated time series are compared to observed time series- perhaps I missed something; could this be clarified?

AC: This analysis is included in Mizukami et al. (in review), but as noted in the reviewer's comment below (P4 L.3) we recognize that it is difficult for the reader to evaluate this study since it is not yet published. We have developed an additional supplemental information file to more clearly describe the salient results from Mizukami et al. to illustrate the degree of correspondence between the simulated and observed timeseries. We also note that the results from our study are based on a delta approach – that is, we are not using absolute magnitudes of flow to drive any of our modeling results; only changes in frequency of events exceeding a model-derived threshold. We will include additional discussion in the body of the manuscript that summarizes all of these points.

RC1: P3 L.5. (& discussion P8 L.31) "only the 100-year floodplains are consistently mapped and available at a national scale": for future work, it might be interesting to use an automated digital elevation model floodplain extraction method.

AC: We agree that it would be interesting and informative to repeat this analysis for a wider range of flood magnitudes. We have done some preliminary analysis of assets exposed to a wider range of flood magnitudes using data from the FEMA Risk MAP program, and we will include some discussion of this in our revision. However, ex-



tending our results using an automated DEM extraction method would be a significant undertaking, and one that is well beyond the scope of this study.

RC1: P4 L.3. "Full details of the. . .methodologies are available in Mizukami et al. (In Review)" – it is difficult to comment on a methodology that is under review in WRR. . .could the authors comment on this?

AC: Mizukami et al. (in review) has been revised and resubmitted in response to reviewer comments, and we anticipate that the manuscript should be in press relatively soon. However, as noted above we have included salient details of the Mizukami et al. paper in our supplemental information file to ensure that the reader has enough information available to understand the basics of the method.

RC1: P5 L.29. "We created a random sample of flood depths". This section and the calculation of depth-damage function is interesting, but it is a little unclear how the depths were calculated. I assume the bathymetry of the river and any changes in river capacity are not considered; if so, this would be worth commenting on (and the potential implications for the results).

AC: We will clarify this part of the description in revision. In addition, we have added a reference to a presentation on the National Flood Risk Characterization tool in the manuscript. This reference contains many of the details sought by the reviewer here.

RC1: P7 L.17-21. "changes in flood damages broadly mimic changes in flood frequency. . .". I believe this finding is to be expected, if the method assumes that flood frequency is driven solely by meteorological change, without considering potential temporal changes in the spatial distribution of assets, land use, water management, and/or channel capacity. At this point it would be worth mentioning these assumptions explicitly, rather than waiting until the last paragraph of the manuscript.

AC: We thank the reviewer for pointing this out. We will be more explicit about assumptions at this point in the manuscript, as well as in the discussion. In particular, we will

reiterate that the lack of modeled changes in land use and infrastructure in our method mean that changes in flood frequency are driven entirely by changes in climate forcing.

RC1: P8 L.1. It seems that the difference in projected flood damages between the Southeast and Northeast is considerable ($2 billion per year by 2100 versus $1 billion per year by 2100), and would be worth explaining.

AC: Given that our results are primarily driven by changes in precipitation, this result indicates that precipitation and runoff during the months that cause flooding are projected to increase more consistently in the Southeast than in the Northeast, and/or that there is more infrastructure at risk in the Southeast than in the Northeast. We will expand the discussion to more explicitly address this finding and our hypotheses for why this is occurring; however, this is in large part an avenue for future research.

RC1: P8 L.26. "We generated preliminary comparisons of hydrologic projections using two different VIC parameter sets". This is a little vague and is not explained in the paper; perhaps the authors could be more explicit, or include details in the supplementary materials.

AC: We will expand this discussion, and we have also included more information on this topic in the new supplemental information file we developed for our resubmittal. Reviewer #2 General Comments RC2: This is an interesting paper, combining several modelling approaches to give order of magnitude estimates of economic losses related to 1% flood events increasing with climate change over the 21st century. I enjoyed reviewing it and broadly speaking I think the paper can be published with minor revisions, principally around tightening up some of the language to convey precise meanings. In short I'd recommend the methods are fine as they are, but the discussions need to take extreme care around how far the results can be extrapolated. This is especially important given that the results could have wider public, policy and media interest, and from that perspective it is perhaps even more important to make sure someone reading the paper without all specialist knowledge/training will not potentially misinterpret some



of the findings/discussion. Specifically, I think the fact the study is delivering order of magnitude estimates and should be considered a 'first pass' at answering the question of future flood hydrology/ risk and damage need to be incorporated into the discussion a little more. Even more importantly this needs to be covered in the abstract for the reasons above.

I should add the caveat that I do not consider myself competent to review all technical aspects of downscaling of GCMs and so would defer to the other reviewers and editor on those aspects of the paper.

AC: We thank the reviewer for this overall positive review. As with the comments from Reviewer #1, we recognize the need for additional clarity/explanation of some components of the paper, and will address these concerns on revision.

Reviewer #2 Specific Comments

RC2: 10 – The two clauses in the opening sentence don't directly follow from each other. The first part makes link between flood occurrence and extreme weather and says extreme weather events will increase. The second part says therefore flood DAMAGE will increase. Not directly supportable to link increased frequency with increased damage in a general sense. This would need to be amended (at the least) to say "thus [potentially] increasing flood damage.." Or alternatively use a more general concept like increasing risk or exposure.

AC: We recognize the potential incongruity as written. We will revise the text as suggested.

RC2: 13 – (and elsewhere – pg 3, line 3). I'm not sure about the terminology of referring to them as "locations", would "reaches" or "catchments" convey this better?

AC: Agreed. We will revise "locations" to "reaches" here and elsewhere for consistency and clarity.

RC2: 19 – Care in language needed here (and elsewhere). Paper is specifically talking

about flood damage, but here talks about flood risk. Not same thing. Would be better to be consistent throughout to avoid confusion.

AC: We will clarify the use of “damages” vs “risk” throughout the manuscript.

RC2: 22 – This sentence needs rewording and maybe more caveats adding. At the moment the argument is somewhat tautological when it’s boiled down – “we think we are being conservative, therefore our conclusions are conservative”. I think this needs be stated in a way which does not seem to infer what the findings of future work would be! A key issue is that the result is an order of magnitude estimate; there are many assumptions made in the methods (either in choices or models) which are assumed to give uncertainty of an order of magnitude less (hence order of magnitude estimate), but for many of these we don’t know whether they are over or under. I think what you are trying to say here is that more advanced techniques can constrain this uncertainty for future work. It’s almost a separate point to say that you feel you’ve made methodological choices which would tend towards underestimating total damage. Indeed it may be worth separating out these two ideas/statements.

AC: We appreciate the reviewer’s concern about our use of the word “conservative” here. The reviewer is correct that the point of this sentence is not to imply that we are underestimating or overestimating damages, but to state that further refinements to our methods could improve our understanding of results and their sensitivity to methodological choices. We will clarify the text accordingly.

RC2: Intro 26 – I don’t follow this statement I’m confused how an annual average can have a range, or how annual damage can be an average? – I.E. if annual damage is averaged over 100 years it is a single number? Does this mean just the measured annual damage ranges between x and y, or is it estimated from different sources? Or perhaps decadal/regional averages? Clarify.

AC: We recognize the confusion in the text as currently written. The intended meaning here was that nationwide inland flooding damages each year typically fall within a range



of dollar values. We will clarify our meaning to ensure that there is no confusion.

RC2: 28– clarify the “damage” here; is this estimated economic costs, actual rebuild costs, including all economic losses not just physical ones? Important as this relates directly to paper findings so important to know.

AC: The numbers we quote here are reported damages in terms of insurance losses, rebuild costs, etc as compiled from “federal, state, or county level government officials”, as summarized in NOAA (2016). We will clarify the text as requested.

RC2: 29 – Care with language. This flooding is “historical” in what context? Largest ever? Or do you just mean “large flood events”?!

AC: We are referring to these events only as “very large flood events.” We will clarify the text accordingly.

RC2: PG2. 1-7 – I think this paragraph could be framed better. I recommend rewording slightly as the three sentences don’t seem to exactly follow on, one from the other. In the first it says challenging to understand events to climate change. Then says this is advancing, as well as attributing extremes in general to warming. Then finally says long term trend forecasting is important for stakeholders. At this point you are first making the case for why you would do this work, so I think it would be more powerful to suggest why the approach in the first two sentences is not fit for purpose and so therefore why the trend approach used later on is better/necessary/more useful in an explicit sense. Would be an early marker as to why this is all important and sell it to the reader(s).

AC: We thank the reviewer for suggesting ways we could improve this paragraph. We will revise the paragraph to improve the way this part of the introduction frames the remainder of the manuscript.

RC2: 12 – be explicit here whether you are talking about the mean damage in the 1% event per year, or the cumulative damage of all such events over the time span.

AC: We will clarify the text in this part of the manuscript, to make it explicit that our

study looks at projected damages from 1% events in each year, based on an ensemble across a suite of GCMs.

RC2: 15 – I'm uncomfortable with the paper claiming a "deep body of previous work" but not citing any! Is there at least 2-3 review papers that could be cited in terms of "(see A et al, 2006, B & C, 2010: : :.)"

AC: We thank the reviewer for pointing this out. As written, we reference some of the "deep body of previous work" in the sentences following this one. However, we will revise to bring some of these references up further in the paragraph to avoid the incongruity pointed out by the reviewer.

RC2: 22 – reference(s) for inconclusive studies needed. RC2: 23 – references for significant interest, or be more explicit about the source of this if not based on literature.

AC: We will revise this paragraph and the paragraph preceding it to make better reference to the previous literature in the context of our own study.

RC2: 30 – Use of "flood risk" here, but this time to apply to (I think) the frequency of flood events. If so this is more broadly how I would understand the term, but clashes with usage elsewhere. This needs to be more explicit in this context, or alternatively could define flood risk as a term for purposes of this paper.

AC: We thank the reviewer for pointing out our use of this potentially confusing term. We will clarify our intended meaning of "flood risk" throughout the paper

RC2: PG4. 5 – This needs to be less definitive I think – "are likely to be conservative" rather than "are conservative", unless this is supported with methodological references.

AC: We will clarify text as requested.

RC2: PG5. 20 – reference to the tool needed – ideally to some form of report/paper/website. And also the name of the tool needed.

AC: We will include reference to a publicly available presentation so that the



flood risk tool is more fully referenced. This presentation is available here: http://www.iwr.usace.army.mil/Portals/70/docs/frmp/Flood_Risk_Char/NFRCT_Slides_FRM_

RC2: PG7 23 – this is an interesting use of the word "modest" to refer to $1bn! I take the point, but recommend changing.

AC: We thank the reviewer for pointing this out. We will revise language here.

RC2: PG8 10 – Not sure about "calculate" here, think "estimate" or something similar is more accurate.

AC: We will revise to use "estimate" instead of "calculate".

RC2: PG9 5 – I'm not convinced by the way this is framed. I agree that larger floods can be more damaging, but not necessarily that they always ARE. Likewise, small, more frequent floods can also cause damage, but not always. This will be very catchment and site specific and depend on the floodplain topography and siting of assets. In some cases, it may be that the 1% event floods all assets in a location, and therefore a bigger flood makes no additional difference. I'm therefore a bit uncomfortable with the certainty that all the estimates are underestimates of damage, particularly given levels of uncertainty in the methods anyway. I'd recommend this section is reworded to be less explicit in predicting the results of refining the methods! Perhaps just highlighting the absence of the frequent small floods and the potential effects of larger events in some (most? many?) catchments and saying it will invariably effect the damage estimates, rather than specifically state your estimates are definitely underestimates of damage in all cases.

AC: We appreciate this comment from the reviewer, and also recognize this to be a clear avenue for future research. We will reword this section to more clearly reflect what we can and cannot infer from our results.

RC2: Figure 3 – I'm not sure about the p-value reported in the caption. The purpose of a p-value is only to show that it is less than the alpha value set for significance,

which is normally 5% or 1% in natural sci. The value of <0.00000001 reported is unnecessary as it doesn't give any more info than something like p<0.001 (0.1%) and may incorrectly imply an incredibly high level of significance is being looked for (as alpha is not explicated stated elsewhere)

AC: We appreciate this comment and will revise the figure caption accordingly.

RC2: Figure 8 – I am perhaps admitting my ignorance of US geography here! But I was not able to easily visualise what the different labelled regions coincided with, particularly given it is being published in a European based journal (albeit an international one) it may be worth adding a map of where you divide up the regions, perhaps this could be incorporated into one of the existing map figures as a background layer to save adding another figure?

Addendum: After typing my report I read the other review comment and noted they have recommended a little more discussion of some of the regional based results. In light of that I really think a map reference of some kind to guide the reader through, as suggested in my figure 8 comment above, would be very helpful.

AC: These region labels are included in Figure 4, but we recognize that they could be missed if they are not described more explicitly in figures such as Figure 8. We will add reference to Figure 4 here and elsewhere in the manuscript, so that it is more clear to the reader where each of the regions is located.

New References

Ivancic, T. J., & Shaw, S. B. (2017). Identifying spatial clustering in change points of streamflow across the contiguous US between 1945 and 2009. Geophysical Research Letters, 44(5), 2445-2453.

US Army Corps of Engineers Institute for Water Resources (2014). "National Flood Risk Characterization Tool: Overview of Capabilities and Current Limitations." Presentation at an Inter-agency flood



risk characterization workshop, February 25, 2014. Available at: http://www.iwr.usace.army.mil/Portals/70/docs/frmp/Flood_Risk_Char/NFRCT_Slides_FRM_

Tamaddun, K., Kalra, A., & Ahmad, S. (2016). Identification of streamflow changes across the Continental United States using variable record lengths. Hydrology, 3(2), 24.

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