Natural Hazards
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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.

General comments

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.

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.

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.

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.

Specific comments

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.

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

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

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.

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?

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.

P4 L.3. "Full details of the . . . methodologies are available in Mizukami et al. (In Re-

C3

view)" – it is difficult to comment on a methodology that is under review in WRR. . .could the authors comment on this?

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).

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.

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.

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.