



EGUsphere, referee comment RC2
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Comment on egusphere-2022-101

Anonymous Referee #2

Referee comment on "Modeled storm surge changes in a warmer world: the Last Interglacial" by Paolo Scussolini et al., EGU sphere,
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General Comments

The purpose of this paper is to understand how global storm surges varied during the Last Interglacial period (LIG) and relate the variations in water levels to storm climate. The LIG period is of interest as it represents a potential future climate state of the earth and the overall relationship between changes in storminess and storm surges is an ongoing topic of research. The approach taken in this paper involves using modeled wind and pressure outputs from a GCM and then forcing a global hydrodynamic model to predict storm surges. The authors find various changes in storminess in a warmer climate during the LIG period note qualitative patterns between the two.

I find the topic and the overall scope of the work very interesting and potentially very informative; I would be able to recommend this article pending major revisions and would like to see this happen. I find the discussion, presentation and analysis of results vague and somewhat superficially analyzed in several places. In my opinion, more detailed analyses are required before final publication can be considered. Further, several technical details need to be clarified that as well are elaborated in my review. Of major concern for the reviewer is how one can reliably use relatively coarse resolution approximately 1-degree horizontal resolution GCM outputs to directly force a hydrodynamic model without quantifying/adjusting for potential biases in meteorological forcing inputs.

Specific Comments

Overall, the introduction could use more precise language and focus.

"Specifically, for the LIG, Kaspar et al. (2007) found a strengthening of the winter mid-

latitude storm tracks, along with a northward shift and an extension to the east." There are several sentences that state very broad and vague changes to "future storms". Sometimes the word 'storm' is qualified as 'tropical' or 'extratropical', or even 'winter mid-latitude storm tracks' and sometimes it is not. Please be consistent and clarify.

"Ensembles of climate models project a future poleward shift of boreal extra-tropical cyclones, and a decrease in their occurrence (Chang et al., 2012). For the boreal mid-latitudes, the most recent generation of global climate models associate future global warming with a southern shift of winter storm tracks, and weakening of summer storm tracks (Harvey et al., 2020)." To me, these last two sentences appear to be in direct contradiction from one GCM model generation to the next, which raises some serious concerns in the application of GCMs if interpreted literally. Perhaps the authors mean to express that there is great uncertainty in what can happen in terms of future changes to storm tracks? More explanation here is necessary as to why these changes are occurring in modeled simulations from a dynamical perspective.

"To achieve this, we employ meridional and zonal wind speed and sea level pressure from simulations of LIG and PI climate with a global climate model to force a global 90 hydrodynamic model to simulate the extreme water levels along coastlines resulting from storm surges." This statement raises the important question: how are the meteorological inputs deemed reasonable to force a hydrodynamic model? Normally historical periods are used for a statistical validation of inputs and compared with reanalysis datasets. It is well known that relatively coarse horizontal resolutions associated with the GCMs produce largely biased surface winds at weather time scales that are important for storm surge prediction.

"We note that results in the extra-tropical latitudes must be considered more reliable than in the tropics. This is because the spatial resolution of the climate forcing does not allow GTSM to simulate tropical cyclones with realistic frequency and magnitude (Roberts et al., 2020)." Please clarify what you mean by "reliable". Both extratropical and tropical cyclones are poorly represented in GCMs hence the large body of research on both statistical and dynamical downscaling to improve the representation of these storms. This brings me back to the previous point about how you determine that the meteorological inputs are physically representative/accurate for storm surge prediction.

Section 3 needs improvement and more quantitative analysis. Perhaps a clustering algorithm could be used to relate the changes in storm surge return periods (globally) and atmospheric variables to better understand their relationship(s). Correlation maps between variables and predictions could also demonstrate the relationship between them and strengthen the discussion. Perhaps also separating out the tropical "warm-season" from the "cool-season" would be helpful to further refine the results since the seasonal separation is opposite for each hemisphere (e.g., summer is JJA in the northern hemisphere while winter in the southern).

I find the visualization of points colored by magnitude in Figures 3, 4 and 5 difficult to analyze. Perhaps these figures could be divided into regions using subplots for select areas

of interest that are discussed in the text instead of showing the entire global picture? I also find the colormaps somewhat non-intuitive in Figures 4 and 5 (i.e., blue and red should be inverted).

Section 4 is also confusing in that the authors state that a comparison with previous modeled results would not be valid but then later it is stated that the surface temperature patterns are similar between LIG simulations and modeled warmer future climates, so it'd be "meaningful" to perform intercomparisons.

"Nevertheless, the validity of a comparison with those studies is limited by differences in the climate models and reference climate benchmarks employed, such that it is not possible to separate the effect of differences in climate forcing and of different models" This statement is then later followed by a *"On the other hand, spatial patterns of warming during JJA in the Northern Hemisphere are similar across simulations of the LIG and of warmer futures, as evident from the fact that boreal summer sea surface temperature patterns in the LIG simulation somewhat resemble those of the projected futures (Fig. S8). A qualitative 230 comparison of results for the summer of the Northern Hemisphere is therefore meaningful."*

Of concern by the reviewer is this statement in Section 4.2: *"While regional studies have attempted to correct for such biases (Marsooli et al., 2019), global studies have not."* Do the authors have confidence that this statement is correct?

"However, if we assume biases to equally affect the PI and the LIG simulations, the anomalies between results based on the two simulations, which we report here, should not be impacted by the presence of biases." That is a significant and important assumption that the biases are equal throughout the PI and LIG simulations. Given the significant changes to the atmosphere and general circulation between the PI and LIG simulations, I would not expect biases to be equivalent at all. The authors have also presented no information to back up this assumption. I would recommend that this is further investigated before publication can be recommended.

Technical Corrections

There are some grammar and spelling errors throughout that the authors should work to address before my other comments mentioned are addressed. Please be consistent with

how the word "storms" are referred to whether that means extra-tropical or tropical, as discussed earlier on.