



EGUsphere, referee comment RC1  
<https://doi.org/10.5194/egusphere-2022-101-RC1>, 2022  
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## **Comment on egusphere-2022-101**

Anonymous Referee #1

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Referee comment on "Modeled storm surge changes in a warmer world: the Last Interglacial" by Paolo Scussolini et al., EGU Sphere,  
<https://doi.org/10.5194/egusphere-2022-101-RC1>, 2022

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### **General Comments**

The paper aims to use models to understand how the climate patterns of the Last Interglacial (LIG) influenced patterns of extreme sea levels related to storm surges around the world. To do so, the authors use large-scale variables from LIG and pre-industrial global climate model simulations (meridional and zonal wind speed as well as sea level pressure) within a hydrodynamic model to simulate water levels along the world's coastlines. The authors find that in a warmer world (LIG), there are substantial seasonal and annual variations in sea level extremes associated with storm surge around the world's coastlines.

While I find the premise of this study very intriguing, and potentially very informative, I have a number of concerns about the study and how much it can really tell us, being based off of only a few large-scale climate variables from one GCM (though I note that the lack of a full model ensemble is listed as an important caveat by the authors). Portions of the results and discussion feel very vague, and are in need of more detailed analysis or description before final publication.

### **Specific Comments**

In many ways, the introduction seems under-referenced. For example, omitting large but highly relevant review papers like Knutson et al., 2019 seems problematic

*"Since geological proxies do not have the stratigraphic and temporal resolution needed to address storms and cyclones directly, a proposed method to examine these phenomena in the past is climate modelling (Raible et al., 2021)." -- This seems like an overly broad, if*

not outright incorrect statement. There's a lot of literature out there dealing with geological proxies and frequency of storms (see for example Rodysill et al., 2020 [<https://www.nature.com/articles/s41598-020-75874-0>], Bramante et al., 2020 [<https://www.nature.com/articles/s41561-020-00656-2>], or Wallace et al., 2019 [<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019PA003665>]) Please revise the statement and be more attentive to citing appropriate literature.

*"GCMs indicate significant changes in storm tracks under different climate change scenarios (Haarsma et al., 2013; Harvey et al., 2020),"*-- Consider adding also Garner et al., 2021, for a recent study in the North Atlantic [<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021EF002326>].

2.5-km resolution along the coastline for a surge model still seems quite coarse. For instances, other papers, like Lin et al., 2012 use an ADCIRC grid to model surges near NYC under different climate conditions that has a resolution of about 100 m near the coast. Is there anything that can be done to improve this resolution?

*"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)."* -- I find this statement (and the general methodological approach) somewhat confusing. I agree that the resolution does not allow for either the GCM or GTSM to simulate tropical cyclones with any realistic frequency or magnitude. However, the resolution of the GCM and use of only broad scale atmospheric variables in the hydrodynamic model doesn't seem as though it would allow reasonable simulation of frequency or intensity of individual extratropical cyclones either. Also, we know that tropical cyclones and their impacts are hardly contained within the tropical latitudes (e.g., Hurricane Sandy, 2012). Therefore I am confused as to why the authors would think that the approach used is generally more reliable outside the tropics. It seems to me that the methodology is, at best, able to produce broad scale findings of plausible maximum water levels, which may be used to guide more in-depth analyses in the future (e.g., downscaling individual storms in particular basins/time periods, etc.)

*"Several other seasonal anomalies in zonal and meridional winds speed emerge that we do not mention here."* -- Then why bother mentioning it? If the results for these "other seasonal anomalies" were deemed inconsequential to the point of being irrelevant, don't bring it up at all. If these "other seasonal anomalies" are worth noting, please explain why.

Please provide a citation or more background that justifies using EKE calculated from 10-m zonal and meridional winds as a proxy for storminess in climate models.

Section 3.3 is in great need of some quantitative analysis. Even simple calculations of the correlation between variables, or a principle component analysis would be extremely

useful.

The opening of the Discussion seems to dismiss differences between this work and similar studies of modeled future climates as being due to differences in models and climate benchmarks--so much so that a comparison is not relevant or reasonable. Yet, the discussion goes on to say that because sea surface temperature patterns from LIG "somewhat resemble" those of projected futures, it is reasonable to use the results from this study to qualitatively draw conclusions about possible future climates. This section seems to substantially contradict itself, and I find that quite concerning.

Figure 3: It could be helpful to include some labels on maps, or else some other method of highlighting the areas the authors specifically note in their results. Same for Figure 4 and 5 maps

Figure 4: This figure is very hard to read, particular part A. I recommend a bolder or darker color bar to make results more clear.

### **Technical Corrections**

There are some typos throughout that the authors should work to address before final publication