Natural Hazards
and Earth System
Sciences
Discussions

Interactive comment on "Regional tropical cyclone impact functions for globally consistent risk assessments" *by* Samuel Eberenz et al.

Anonymous Referee #1

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This manuscript by Eberenz et al. evaluates the model-simulated damages from tropical cyclones, and provides suggestions to improve this assessment and reduce the uncertainty of simulations by using regionally calibrated data. While the premise of the paper seems straightforward (e.g. "improving the calibration of the model will result in closer simulation of observed events"), the execution of the work in the paper is well done as it explores the limitations of their proposed approach. While overall the manuscript is well-presented and organised, there are opportunities to improve the text, particularly the analysis in the case study for the Philippines.

1) Some initial minor comments include better consistency in the risk language used in the paper; overall, it is good but there are some errors, e.g. para 35: "natural risk" –> is this hazard? risk? and para 180 "from natural catastrophes are records are available"

and following line ("natural disasters"). As you might well know it is a common refrain in the disaster/hazard community that "there are no natural disasters" – so do double check for the consistency. I also think that in para 55 where you say "one [...] function... might be inappropriate [...]", as this is the main argument of your work, could be made even stronger to create a deeper impression of the purpose of your research.

2) I find it interesting that the model over-simulates damages in the NWP basin, as it could be easily imaged that the damage function would not be able to simulate the myriad impacts of the associated hazards you mention such as the storm surges. In line with this I think it would have been interesting to provide an hypothesis for your research question, for example in para 70 where you pose this question.

My main comments are related to the case study of the Philippines.

4) Firstly, there is some confusion in the TC nomenclature which should be addressed for consistency (Table A4). For example, Ondoy is the local name and Ketsana is the international name.

5) Additionally this case study of the Philippines is very brief and only an assessment of asset exposure, and not vulnerability. I think that this section could use more context of the vulnerabilities associated with the Metro Manila region, enhanced by locally-led scientific literature on vulnerability (e.g. Porio 2011) as well as an analysis of the hazard events themselves to give the reader more context (see e.g. the work of Lagmay et al, Abon et al re: Ketsana and Haiyan (incl. effect of mountain ranges to improve your Done 2019 reference), Cayanan et al 2011 and Cruz/Narisma on SW monsoon effect on TCs, Yumul et al on TC Fengshen). Indeed I think these references could also be visited as Espada (2018) is often your only reference (Table A4).

5) There are some paragraphs that could use more attention and more geographical nuance, for example para 470 on Typhoon Haiyan (2013). This TC impacted mainly Tacloban City; indeed, this reflection of imbalanced damages appears to be simulated in your model output (Figure 6c) but Iloilo and Cebu cities are mentioned in lieu of this.

6) Having more context would also provide you an opportunity to refute an argument you pose earlier in your paper related to the CLIMADA setup, in that "[...] no impact is expected for low wind speeds," when it is evident many high-impact events in exposed and vulnerable regions cannot be estimated on wind speeds alone; geography/topography, exposure and vulnerability, local climate conditions (e.g. SW monsoon) play a significant and sometimes, larger role in realised damages from TCs.

7) With a better focus on this I think it would provide a richer and more meaningful assessment of exposure and vulnerability that give better context to your paper and the need for more regionalised calibration of damage estimates from TCs.