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Comment on nhess-2021-161

Bret Webb (Referee)

Referee comment on "Development of damage curves for buildings near La Rochelle during Storm Xynthia based on insurance claims and hydrodynamic simulations" by Manuel Andres Diaz Loaiza et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-161-RC2, 2021

The authors present the results of a coupled Delft3D+SWAN hindcast simulation of extratropical storm Xynthia and present damage curves derived through analysis of the hindcast results in combination with insurance claims data near La Rochelle, France. Through their results, the authors demonstrate that grid/mesh resolution can impact the shape of the resulting damage curves, and that the best explanatory variables for damage are water depth and total hydrodynamic force. The authors suggest in their concluding statements that their work may have broad application to assess damage from future events along the French Atlantic coast, but subsequently provide numerous qualifiers on their work that contradict the preceding claim.

This certainly is an interesting piece of work and I believe it has a strong foundation that can be improved upon in subsequent revisions. While the technical focus is appropriate, I found the current version of the manuscript lacking in a few substantial ways. The deviation from standard practice when developing damage curves notwithstanding, the work shows promise and will be an excellent contribution to the published literature with a few substantial improvements to the analyses and manuscript.

My general recommendations for improving the manuscript are as follows:

1) Improve the organization of the manuscript, especially the early sections of the text. There should be a clear and distinct progression from the introduction to the methods. The methods section contains information about the study area and the storm, which would be better presented in the introductory section of the manuscript. The organization of the methods section is inconsistent and could be improved to flow more logically. For example, there is discussion of the models and model setup in multiple places of 2.1, 2.2, and 2.3. Furthermore, this section begins with (cf. 2.1) a detailed discussion of the particular storm event without first describing the storm or the models. Section 2.2 could be combined with another section in the reorganization. One may also argue that the validation results belong in the "Results" section, not in the method section. Section 2.4, albeit brief, is appropriately placed and contains helpful information. I will, however, note that use of the term "damage level *x*" in line 124 is somewhat inconsistent with your chosen approach and terminology. Also, that "*x*" is not the damage ratio but rather the

value of the conditional variable (the hazard) for a specific damage ratio increment. Therefore, you likely need a subscript on P such that P_i(x)=… gives the probability of experiencing hazard value "x" for damage ratio value "i" and so on. I'm sure that is what you did in the analysis, but the typesetting of Equation 2 and the corresponding text should be improved.

2) The analyses, while well intentioned, are not particularly robust in their presentation. For example, there is no quantitative assessment of model errors or bias in the prediction of either water levels or wave heights. Simply plotting predictions and measurements and saying the agreement is "good" does not inspire confidence, particularly when the disagreement between the two for wave heights appears to be quite substantial. As a second example, there really is not enough information provided relative to the development of the damage curves given its prominence in the title of your manuscript. So while the content of the existing manuscript is strong, it is simply short on details and could benefit from an expanded discussion in many places (a few of which are noted below).

3) There is a duality in the manuscript that I am having a hard time reconciling, particularly given point #2 above (lack of detail). There is a significant emphasis placed on the influence of grid resolution on the resulting damage functions. However, there is not enough supporting detail provided for these grids/meshes. Given that there is similarly a lack of detailed information regarding the development and application of the damage curves (additional comments below), this leaves the manuscript lacking in technical details as mentioned earlier. While the impact/influence of the grid resolution is noteworthy, it does not appear to be the focal point of the paper (not in the title) so I would suggest minimizing its relevance and adding much more detail to the damage curve discussion. Alternatively, if the authors would prefer not to expand the discussion of the damage curves and instead reorient the focus of the paper to one associated with the grid resolution, then consider greatly expanding details regarding the features and characteristics of those grids and perhaps modify the manuscript title accordingly.

4) I have some reservations about your analysis methodology. Not to say that it is in any way "wrong" but it does suffer from a lack of explanation (again, just my opinion). I would like to see some detailed description of the building archetypes considered in this analysis. Are all buildings considered to be of the same archetype (I assume so because there is no differentiation in the results)? Can you provide more details beyond "stone masonry" such as number of floors/heights, foundation types, age of structures, roof types/materials, etc.? Without the qualifiers, I think it would be very easy for someone to misapply your methodology. Also, I would like to see a better presentation of the explanatory variable (hazard) values for the damage curves. I know that you have presented them graphically in the appendix, but it would be valuable to also list the means and standard deviations (likely for only one grid) of those variables/variable groups. Finally, can you add some discussion regarding potential weaknesses of your chosen "damage ratio" approach to representing damage? There are many weaknesses with using this as a substitute for the more common "damage state" because the damage ratio does not correct for valuation based on location among other weaknesses. As another example, "insured value" is often a personal/elective choice made by the homeowner and there is bound to be substantial inconsistency in what one chooses to insure their property for. To expand a bit further, a low damage ratio value may be the result of minimal damage or a very high insured value. Therefore, the damage ratio is sensitive to two metrics, one of them choice-based, as opposed to a traditional damage state classification which, while somewhat objective, focuses only on the severity of damage to the property. My primary concern here is founded upon the fact that nearly one-half of your 423 reported claims have an assigned damage ratio <0.1 (cf. Figure 1). Finally, in a traditional damage/fragility analysis one would also consider structures with no damage. I do not recall any mention of non-damaged structures in your analysis. Therefore, the resulting damage curves may very

well be biased.

Here are some additional comments that address specific items in the manuscript…

Line 30/Figure 1: recommend normalizing the ordinate values by the total number of claims so that you can report these in terms of their true "frequency" instead of simply counts. If not, please edit the axis title as these are not frequencies.

Line 38: the introduction in its current form is significantly lacking in terms of a thorough review of pertinent literature on damage functions derived from coastal hazard models (e.g., Masoomi et al., 2019 and many others), lacks an orientation to the study area, and does not thoroughly describe the storm event. I would recommend adding:

-much more background on relevant literature

- a detailed description of study area with location map, exposure/vulnerability to extreme events, hydrodynamic setting, etc.

- more information on the history and characteristics of Xynthia

Masoomi, H., van de Lindt, J.W., Do, T.Q., Webb, B.M. 2019. Combined wind-wave-surge hurricane-induced damage prediction for buildings. *Journal of Structural Engineering* 145(1).

Line 41: minor comment but use consistent typesetting of "Delft3D" throughout the document.

Line 48/Figure 2: any reason why there is a font change in this graphic? Was that intentional?

Lines 49-69: I find it odd that you are interjecting more literature review here as opposed to providing it earlier in the document.

Line 63: missing comma… "… storm characteristics, a regional model…"

Line 65: can a resolution of 80 meters accurately capture terrain features and individual homes?

Line 69/Figure 3: There is not enough contrast in this image to make out the details. The date/time codes for every storm report make the figure unnecessarily busy.

Line 73: what data sources did you use for land cover / land use to assign friction coefficients? (nb. Ignore this comment, I see the answer on line 99).

Lines 75-79/Table 1: how do these relate to the 80-meter resolution mentioned previously?

Line 83: "… spatial resolution and temporal every 3hrs." awkward phrasing

Line 99: use of a constant Manning's "n" value for the entire grid is a significant technical weakness in this study. While it "may" be appropriate for some open water conditions, it is certainly not reflective of the terrain where the subject structures were likely found. Could you please provide a justification and suitable citation to support the use of a constant friction factor? I have read numerous papers in the past ten years that point to the importance of accurate representation of terrain roughness through the assignment of proper friction coefficients.

Line 105: what is SHOM?

Line 105: "… during the whole simulation is good (Figure 4)." Is good relative to what? There is no quantitative basis for this statement.

Line 110/Figure 4: Can you please explain the spurious oscillation at the beginning of your simulation results? I incorrectly assumed that this was the surge event, but it appears to be a numerical instability associated with model spinup.

Line 115/Figure 5: there is absolutely no assessment or narrative to accompany these results. Since wave properties are highlighted as one of your preferred explanatory variables in the damage analysis, and since wave processes contribute to coastal flooding (your other explanatory variable), can you provide some commentary on the disagreement between the modeled and measured waves? Why are the observations listed as "swell height"? Are you comparing two different wave statistics in this figure (i.e., swell height and significant wave height)?

Line 121: "… specifically, relates the…" typo and awkward phrasing

Line 127: missing commas after "paper" and "way"

Lines 132-138: Damage curves are often given for different levels/magnitudes of damage. Here it appears that you are integrating the damage results across all of the discrete damage ratio increments. Was this a specific choice/preference, an artifact of your damage indicator scheme, or something else entirely? Was there no interest in disaggregating the damage data as is traditionally done in these types of analyses? For example, developing unique damage curves for different damage states?

Line 133: I don't think "Box-Whisker" is capitalized, not proper nouns

Line 136: "Damage" should not be capitalized

Line 142: "Where" should not be capitalized since you are using the equations as the subject of your sentence.

Line 145/Figure 6: I find these figures to be less helpful than I had hoped. The size (small) and contouring scheme do not allow for much interpretation of the results.

Line 150: "Similarly" -> "Similar"

Line 152/Figure 7: at the scale provided it is difficult to discern details in these figures. Also, there is no explanation of the symbols in these figures.

Line 157: "related with the" -> "related to the"

Line 160/Table 2: For explanatory values (cf Table 2) used in Eq 2, how were means and standard deviations evaluated for combinations of variables that do not necessarily vary consistently in time? In other words, did you estimate the time-variation of each variable group/combination and then take the mean and standard deviation of the entire time-series? Or, did you evaluate the mean/stdev of each individual parameter and then form the variable groups?

Line 164: typesetting of "hsig" -> "Hsig"

Line 207: delete comma after point

Line 215: "… as thin or concrete structures like flood walls at typically only a few 10's of centimeters thick, and so do not appear in digital elevation models." Awkward phrasing.

Lines 215-220: what about errors/uncertainty in your model predictions?

I commend the authors on a very strong first draft of what I'm sure was a very challenging manuscript to prepare. The authors are absolutely on track towards having a very strong publication that will productively add to the body of literature on damage to coastal structures during extreme events.

Sincerely,

Bret Webb