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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Simulating Synthetic Tropical Cyclone Tracks for Statistically Reliable Wind and Pressure Estimations" *by* Kees Nederhoff et al.

Kees Nederhoff et al.

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Dear editor, dear reviewers,

On the July 31, 2020, we have submitted the following manuscript to the Journal of Natural Hazards and Earth System Sciences titled: "Simulating Synthetic Tropical Cyclone Tracks for Statistically Reliable Wind and Pressure Estimations" (MS No.: nhess-2020-250). On the October 7, 2020, we were informed that the open discussion was completed. In total, we received comments by two reviewers which provided a very positive feedback on the work done and valid suggestions. We would like to acknowledge their time and efforts, which have led to an improvement in the quality of our

manuscript. Below you find a point-by-point reply to all specific questions and suggestions. Attached you also find the revised manuscript with the changes made to address the review comments tracked.

Kind regards, Kees Nederhoff

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James Done (Referee #2)

Specific Main Comments:

1. I agree that synthetic track simulation adds events and overcomes the sampling problem. But these need to be interpreted in the correct context. These synthetic tracks are constrained to reproduce the statistics of the historical record. This means that this tool would not, for example, produce a Hurricane Sandy-like track before Sandy occurred in the historical record. A physical model on the other hand has the potential to produce physically credible but not observed track behaviors. I suggest making this point in the discussion. We agree and have added the comment of physically-credible and statistically-unlikely tracks in the discussion (P24 L18-19). Note that because we divide the basins into grid cells and define the distributions per cell, the tool is capable of generating tracks that have not occurred before.

2. Another limitation of the tool is the assumption of stationarity in the historical record. We know that change has been detected in some TC characteristics in some regions (Knutson et al. 2019). Perhaps this limitation can be stated in the discussion. We have added the assumption of stationarity to the discussion (P24 L19).

3. I'm glad to see the option to include inland wind decay of Kaplan and De Maria 1995) in addition to the implicit decay through the KDE of Vmax. But it's important to state in the manuscript that at-sea winds will still extend inland before the TC center crosses the coast and the Kaplan and De Maria wind decay turns on. I think this is a possible reason for your high bias in 10-year return winds in some coastal regions (Fig.

10). We extended our discussion on the limitations of the landward decay based on De Maria and Kaplan in the discussion section (P25 L9-10).

4. Section 2.5: Can you explain in more detail how asymmetry is considered? The Vmax in BTD is ground-relative and so includes a component of asymmetry. Did you remove the component of asymmetry from the BTD Vmax before creating th synthetic tracks and running the symmetric Holland model (and then add asymmetry back to the spatial wind field afterwards)? TCWiSE in its generation of synthetic tracks does not take into account asymmetry since it is focused on intensity evaluation of the eye. The Wind Enhanced Scheme (WES; Deltares, 2018) handles asymmetry in the computation of the spatially-varying winds. In particular, asymmetry is removed from the synthetic track via the relationship of Schwerdt (1979) prior to fitting the Holland wind model and asymmetry is added afterward again on the spatial wind fields. In this paper, we mainly want to focus on the TCWiSE tool and have therefore not added this explanation, however, we did add a more explicit reference about WES (see P9 L18).

5. There are a couple of notable omissions from the reference list. Arthur (in review) has a paper under discussion at NHESS that describes a synthetic track model that has similar functionality to this study. Lee et al. (2018) published a synthetic track model that differs from your data-driven approach by accounting for environmental drivers of TC behavior. Thank you for pointing this out, we have included these references in the introduction (P3 L1-2 and P3 L8-9).

6. The paper is generally well written. But there are a few grammatical quirks and awkward word choices that can be corrected by a thorough review of English grammar. To improve the language in the MS, a native speaker has reviewed the paper which hopefully resolved the few grammatical quirks and awkward word choices.

Specific comments

1. Abstract, lines 10-12: This sentence makes the point that short historical records may not represent the parent population. This is a valid point but I suggest not using

the term ‘future TCs’ in this sentence because that implies climate change and nonstationarity which is a separate issue. We agree with Reviewer 2 and have changed it into ‘hypothetical’.

2. Introduction: The sentence spanning lines 27-29 about first and second order effects doesn’t appear to fit well in this paragraph about extreme value modeling. We have moved this part of the introduction to the first paragraph since this is a point we would like to make.

3. The introduction talks a lot about the multi-hazard nature of TCs but then the paper describes a tool for TC wind only. I suggest toning done the discussion of surge, waves and rainfall in the discussion and just mentioning it briefly. We agree and have therefore changed the wording in the introduction (see P1 L22).

4. To improve the flow of the introduction, can the point about the need for a larger sample size be made just once? It is currently discussed twice in the first and third paragraphs. We have removed the first reference of the sample size in order to improve the flow of the introduction.

5. Section 2.2. Why not choose a threshold of 17ms to include all Tropical Storms rather than is seemingly arbitrary 25 m/s? We agree and have changed this value in the default settings (the user can always change it to another value).

6. Section 2.4.3. What are the units ‘10kn/s’. Do you mean knots? Yes, we have changed kn/s to knots.

7. Can you comment on the computational performance of the tool? How long does it take to run 10,000 years of the North Atlantic, for example? Computational performance depends strongly on numerical settings and computational power available to the user. We did add in a separate discussion section in which we mention the computational cost for the configuration we ran in our case to give readers an impression (see P26 L18-21).

8. Fig. 5. Would it be useful to additionally plot the difference field to highlight the differences discussed in the main text? Figure 5 (and subsequent ones) are used to show qualitative patterns that are supported with quantitative evaluation via metrics such as the Kirchhofer metric score. We feel that a difference plot will focus too much on the minor variation between historical and synthetic spatial maps instead of focusing on the larger-scale patterns.

9. In Section 3.3, use of the fourth-highest recorded value for the 10-year return wind will probably be noisy. Would it be better to fit an extreme value distribution to the observations to estimate the return value? This may produce a better agreement with the model. We agree that there are uncertainties associated with this empirical approach and that an extreme value distribution might give less noisy results. This is why we show both the non-parametric (empirical) and parametric (fit) results from TCWiSE.

10. Figure 11: Can you clarify what the historical TC wind data are please? Is it Holland model run along historical tracks? Yes, tracks and intensity. The historical is simply the BTD in combination with the Holland wind profile to get to spatially-varying wind fields. The synthetic TC is the same but then based on synthetic tracks and intensities. In order to clarify this, we have changed the caption of the figure to include these explanations (P22 L2-5).

11. The description of Fig. 11 in main text has 'Port Arkansas'. The correct name is Port Aransas. 12) Figures 10 and 12: Please state the grid spacing used in these Thank you for noticing. We have corrected the text and added the spacing (P21 L9-10).

12. Section 4.2. I don't see what you are referring to about the synthetic TC tracks having a less clear southwest to northeast orientation. I think this needs to be quantified in some way or excluded from the manuscript. We have deleted this sentence.

13. The Hoek (2017) reference was incomplete in my pdf version. Thank you for noticing and it has been corrected (P29 L4-5).

14. The Bader (2019) reference is missing from the reference list. Thank you for noticing and it has been added (P28 L5-6).

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