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Discussions

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

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General Comments:

This study describes a new tool for the creation of synthetic tropical cyclone (TC) tracks based on Markov-chains. While the overall approach doesn't differ substantially from previous studies, its novelty lies in its flexibility. The approach is agnostic to the choice of input data and can therefore be applied to any global TC basin. It can also ingest historical track data or even track from global climate models. The tool permits many options for run-time configuration and is designed to be flexible to allow a variety of applications from scientific research to coastal engineering applications. Perhaps the

most unique aspect of its functionality is the inclusion of climate change parameter choices such as options for TC intensity and frequency shifts.

The paper includes an in-depth tool demonstration and evaluation for the North Atlantic basin with focus on the data-rich Gulf of Mexico. This includes a suite of robust statistical significance tests and a valuable combination of point-location and spatial evaluations. Overall the model performs well.

I fully expect that this open source tool will become widely used both as a research tool and a risk assessment tool. I congratulate the authors in making it available. 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. The subject matter is appropriate for NHESS and is worth being published after my comments below have been addressed.

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.

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.

Knutson, T., Camargo, S.J., Chan, J.C., Emanuel, K., Ho, C.H., Kossin, J., Mohapatra, M., Satoh, M., Sugi, M., Walsh, K. and Wu, L., 2019. Tropical cyclones and climate change assessment: Part I: Detection and attribution. Bulletin of the American Meteo-

rological Society, 100(10), pp.1987-2007.

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).

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 the synthetic tracks and running the symmetric Holland model (and then add asymmetry back to the spatial wind field afterwards)?

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.

Arthur, W. C.: A statistical-parametric model of tropical cyclones for hazard assessment, Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2019-192, in review, 2019.

Lee, C.Y., Tippett, M.K., Sobel, A.H. and Camargo, S.J., 2018. An environmentally forced tropical cyclone hazard model. Journal of Advances in Modeling Earth Systems, 10(1), pp.223-241.

Specific Minor 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 non-stationarity which is a separate issue.

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.

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.

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.

5) Section 2.2. Why not choose a threshold of 17ms to include all Tropical Storms rather than is seemingly arbitrary 25 m/s?

6) Section 2.4.3. What are the units '10kn/s'. Do you mean 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?

8) Fig. 5. Would it be useful to additionally plot the difference field to highlight the differences discussed in the main text?

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.

10) Figure 11: Can you clarify what the historical TC wind data are please? Is it Holland model run along historical tracks?

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

figures.

13) 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.

14) The Hoek (2017) reference was incomplete in my pdf version.

15) The Bader (2019) reference is missing from the reference list.

C5