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Discussions

Interactive comment on "A statistical-parametric model of tropical cyclones for hazard assessment" *by* William C. Arthur

Anonymous Referee #2

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Overall, this is a nice summary of a new stochastic TC model for the Australia section. The text is well written, and the Figures are clear. The methodology employed is not novel. But making the model open source is valuable and uncommon. As the model may well be used by various stakeholders, this documenting publication is definitely warranted. Before publication, however, I have some issues that need to be clarified.

1.P2L30: "L is the bandwidth matrix." This sounds as if L varies through the domain. Is that right? If so, more detail is warranted, perhaps a sentence or two stating the range and typical values of L.

2.Section 4.1. Genesis: Is there seasonality in the genesis? Or in the other model components?

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3.P6L39-P7L1: The assumption that Vtang » Vtran is often not good even for purely tropical systems, if they're not high intensity.

4.P7L5: Additional recent relevant work on ET transition are documented in two papers by Bieli et al. (2019). The second paper, especially, addresses statistical modeling of ET transition. The references are Bieli et al, 2019, A global climatology of extratropical transition, Part I: characteristics across basins, J. Clim, 32, 3557-3582, and Bieli et al, 2019, A global climatology of extratropical transition, Part II: statistical performance of the cyclone phase space, J Clim, 32, 3583-35997)

5.P8L28-31: I don't follow this. When I look at Fig 5 I do indeed see a local maximum in the 120-130E, 10S region of the genesis probability density function. What am I missing?

6.I'm surprised that the CP model seems to work well. One of the reasons that a track model like this works is that a TC's location strongly influences its propagation, due to the role of large-scale climatological steering winds. So, it's reasonable to estimate where a TC at location X goes next by analyzing where historical TCs near X have gone next, without any additional information. But this is less true for CP. Fig 11 shows essentially two zones, north a south of roughly 15°S. North of that, the average CP tendency is negative, and south it's positive. Many TCs in the region spend the bulk of their time in the northern negative-tendency zone. For these TCs, what keeps their CPs from declining without limit? I realize there's a large stochastic component, but, without some cap, how is the TCs intensity bounded? For example, in their stochastic TC model, James and Mason (2005) employed an elastic cap at low CP to limit decline below the CP set by the local Maximum Potential Intensity. Perhaps here, given the proximity of landmasses in the region, there's rarely an opportunity for a TC's CP to decrease below plausible meteorological limits?

Similarly, for the few TCs that form south of 15S, in the positive-tendency zone, what causes their CPs to decline on average (the storms to intensify), as opposed to just quickly attenuating to lysis? Is some kind of filter applied, to only accept storms that stochastically intensify beyond some threshold?

Discussion about these points is warranted. In addition, it would be helpful to show some examples of over-ocean CP time series from stochastic simulations. It would also be helpful to have additional panels in Fig 19 that show landfall probabilities for TCs with CP below specified thresholds. (I assume the current version of Fig 19 applies to all TCs, regardless of CP.) Finally, I'd like to see the magnitude of the CP sigma, perhaps in a second panel of Fig 11, to compare to the mean tendencies.

7.Figs 1 and 19: It would help to have mileposts (e.g., towns) indicated on the landfall profile of Fig 19 and correspondingly on the map of Australia, perhaps the ma of Fig 1.

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