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

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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.

The bandwidth matrix is a 2x1 array that defines the bandwidth for x- and y-variation of the kernel function. Updated manuscript to better describe the bandwidth matrix.

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

Seasonality is considered in the genesis of events and the environmental pressure, but is not considered in other model components. To do so would detrimentally affect the sample sizes for the gridded statistical values applied in the track generation component.

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

We have also noted this, and remain cautious of the resulting implications for ARI wind speeds at low intensity. Commentary added to the manuscript in section 5.2

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)

Noted. Most relevant is the pathways to ET, and the likelihood of each in different basins and the implications for parametric wind fields.

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?

The genesis probability for simulations is not shown, only the observed genesis probability.

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 15S. 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?

To minimise the volume of ancillary data that is needed to support the model initial development we aimed for a purely statistical approach. The lower limit for the pressure deficit is set to μ + 5*σ for the grid cell. However, we note that PI is potentially a more instructive limit, and we are presently working on enhancements that will consider this. Additional commentary added to section 4.2.

7) 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?

Storms that do not intensify beyond a pressure deficit of 5 hPa, or survive for more than 12 hours are discarded. Additional commentary in section 4.1.

8) 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.

Added figures of time to maximum lifetime intensity for observed and simulated events (Fig 21 in the manuscript). Added figure of CP sigma (Figure 12 in the manuscript). Added figure of categorised TC landfall (Figure 23).

9) 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 map of Fig 1.

Noted. Updated figure accordingly.

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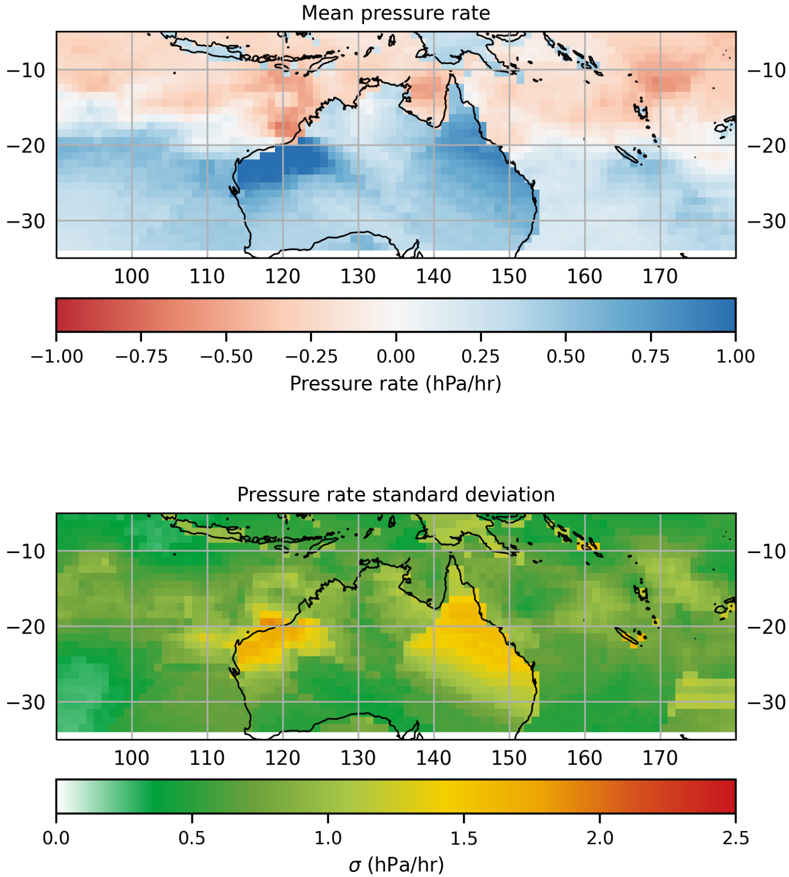

Fig. 1. Mean (top) and standard deviation (bottom) of rate of change of central pressure (hPa/hour), based on IBTrACS v03r09 (1981-2016).

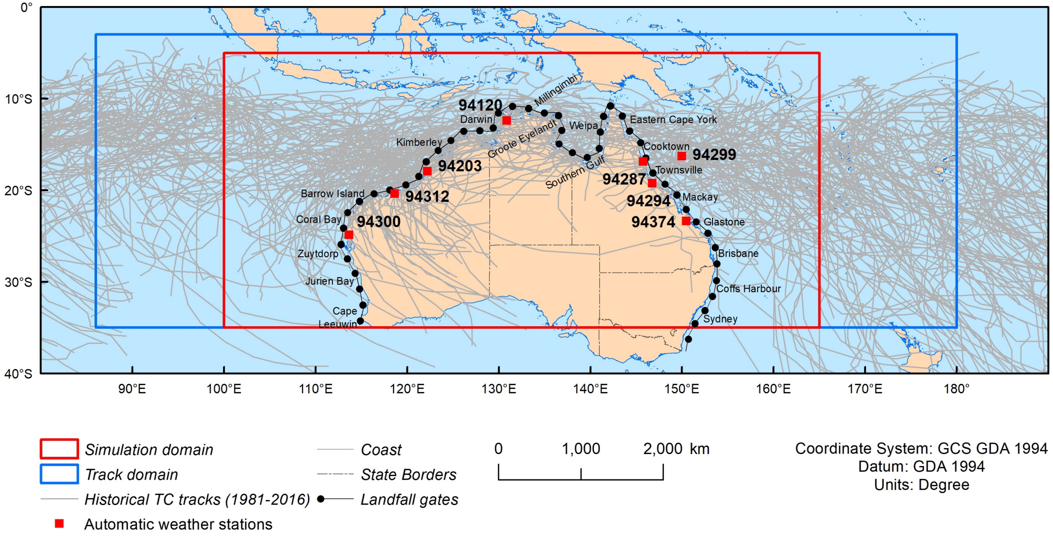

Fig. 2. Updated Figure 1 including milepoosts around the coastline

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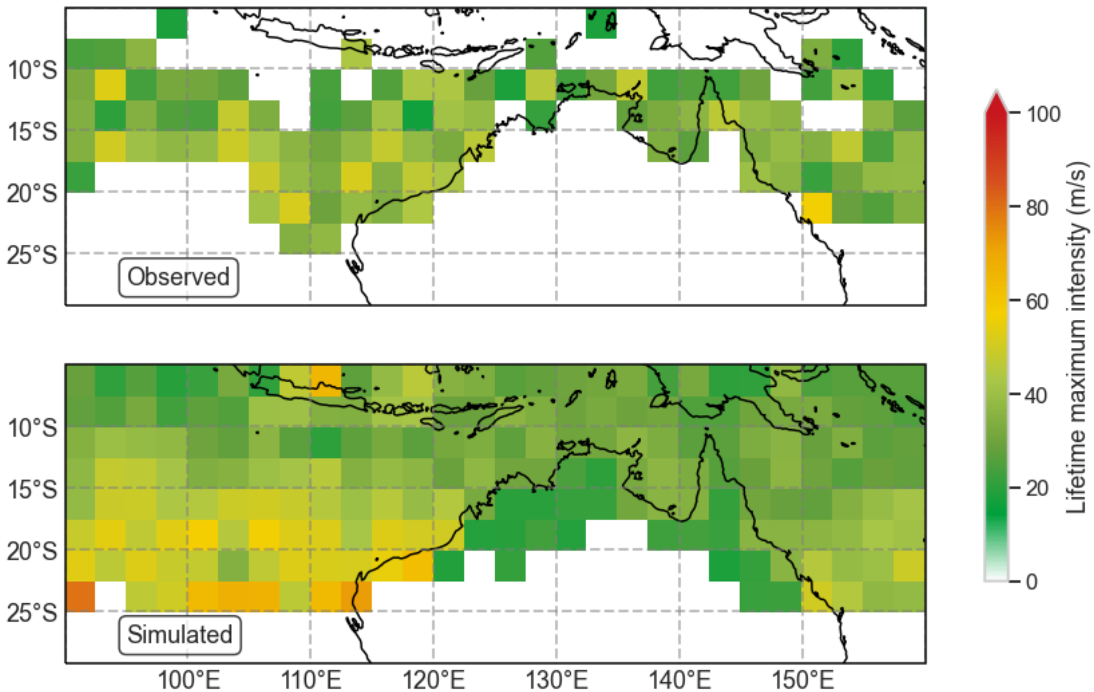

Fig. 3. Historic (top) and synthetic (bottom) mean lifetime maximum intensity. Synthetic values are the mean of 1000 simulations of 35 years of TC activity.

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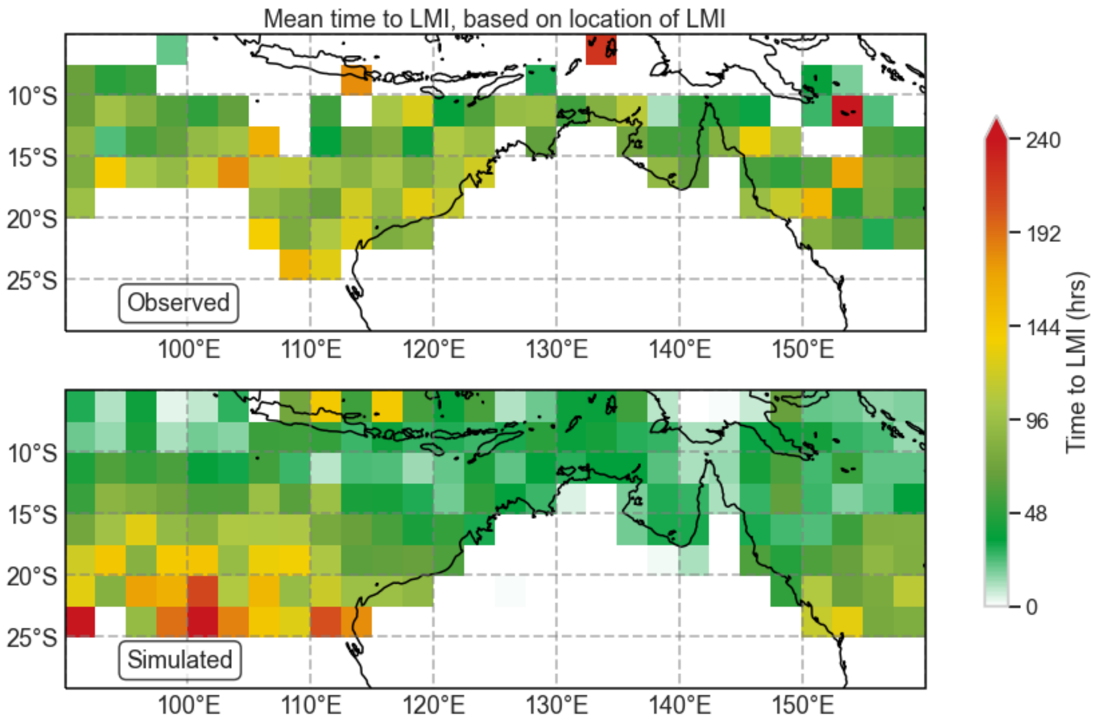

Fig. 4. Mean time taken (hours) to achieve lifetime maximum intensity for historical (top) and synthetic (bottom) TCs. Synthetic values are the mean of 1000 simulations of 35 years of TC activity.

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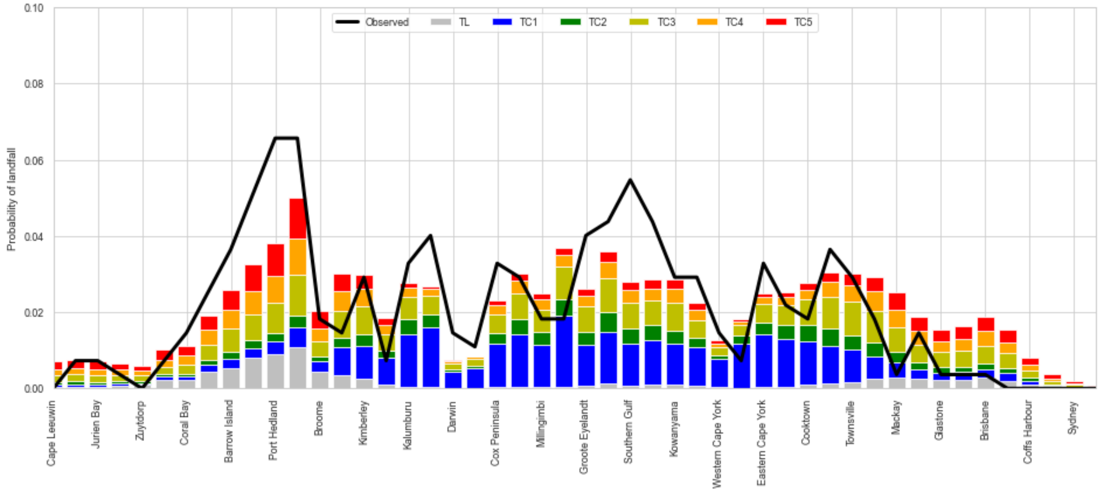

Fig. 5. Mean distribution of landfall intensity (by TC intensity category) for synthetic TCs. Categories are based on the Australian TC intensity scale. The observed landfall probability is shown in black.

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