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Natural Hazards and Earth System Sciences
Discussions

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

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1) Introduction: I understand that this model was motivated by TC risk over Australia. But this is a globally applicable model. I suggest broadening the introduction a little to also discuss global TC risk. Then focus down on Australia to motivate the case study demonstration of capability.

There is commentary in the introduction on the capability to apply the model in other basins. However, the author has not preformed any in-depth tests in other basins. We are aware of applications in other basins (unpublished).

2) It's not clear to me the value of running the wind field model vs. simply running more synthetic years to build up enough tracks in each analysis grid cell. For example, what

is the difference in the 500-year wind speed based on 100 tracks in each grid cell (no windfield module) and 100 wind field values in each grid cell (associated with tracks within and just outside each grid box)?

The zone of strongest winds is displaced to the left of the track (in the southern hemisphere). Using vmax at the track location will give a different result to simulating the full wind field for a given grid cell where the maximum winds will be simulated several grid cells (using a 0.02-degree grid cell) away from the track. Given the desired resolution of the resulting ARI wind speed data, it is more efficient to calculate wind fields. This also offers the opportunity to deliver wind field data to end users from individual events.

3) I think it's important to state more clearly the limitations of the approach in assessing TC risk. The track generator, for example, is not adding new information. It's my understanding that since it samples from the input track parameter distributions it cannot generate tracks far outside the input track distributions (unlike a free running dynamical model). Am I correct? This is important when it comes to interpreting the ARI uncertainty bounds. These uncertainty estimates are uncertainty in the model fit to the observations. These are not uncertainty bounds on the actual TC risk. The actual TC risk would need to account for uncertainty due to the short historical record. Perhaps one other limitation is that the TCRM as currently developed does not account for trends in TC frequency or TC intensity. It therefore assumes stationary statistics. It could certainly be modified in future releases to account for temporal effects.

I agree on the leading point here - we are restricted by the input track distributions. The uncertainty in ARI wind speeds beyond, say 100 years, using point observations is becomes very large, as the observations are often less than 50 years (in the Australian region at least). Using this track model produces a larger synthetic record that is in line with the observed record, but also extends it. For example, teh model allows intensity to exceed the observed record of intensity. We note that the short term record (35 years) is a restriction on the actual TC hazard. It does assume stationary statistics, and further validation against statistics such as palaeotempestology to place the ARI wind speeds in a broader context should be considered. Our current intent in this paper is to describe the model and initial validation exercises. The manuscript has been updated to add discussion on uncertainty arising from the short input record, and how this should be considered in longer time scale contexts.

4) Please explain why the time rate-of-change of central pressure is used rather than the absolute value of central pressure?

Pressure tendency is preferred to absolute pressure due to the lower lag-1 autocorrelation in the tendency values. Using absolute values leads to rapid and almost one-way variation (i.e. constant increase or decrease) in the intensity. There remains a strong autocorrelation beyond lag-1 for absolute pressure values, but not for pressure tendency values. Additional figures have been included in the manuscript for autocorrelation of variables (Fig 1).

5) Method: There are a number of regression equations (Equations 8, 9, 12) that appear to be tuned for Australia. Are users to rederive these regression equations for their domain of interest, or are they globally applicable?

Regression equations are based on Australian data, in line with the choice of case study. As the model is free and open source, it is possible for users to modify these regressions for their chosen domain. Supporting software demonstrates how the regression equations were derived, and can be used to determine those regressions for other regions. The manuscript has been updated to expand discussion on the formulation of the regression equations, and highlight these are regionally specific but can be adapted to other regions. Noting the intention is that the model is not intended to be run on a global domain, but on basin-wide domains similar to other track modelling systems (e.g. Hall & Jewson 2007).

6) Method: I don't understand the need for a decay rate model (Equation 10). Isn't the decay rate already included in the input best track (pmin) data?

Using the autoregressive model over land led to an abundance of storms that reintensified, and generated many more unrealistically long-lived tracks. Implementing the decay rate model forces the TCs to decay in line with observed events (but does allow for very occasional over-land intensification).

7) I think it would be useful to mention the option to additionally use local wind multiplication factors to better account for local terrain effects.

Noted. Updated manuscript to include reference to the Krause & Arthur (2018) that describes the approach in detail.

Minor comments: 1) I read that it takes a few minutes to run a single scenario. Can some detail be added on the computational cost of running 1000 years?

The windfield for a single scenario takes about 10 minutes to run, depending on the overall extent of the track. Using multiprocessor systems, a hazard simulation, including track generation and wind fields across the Australian region (100-170E, 5-35S) for a 10,000 year catalogue requires around 3000 hours of CPU time.

2) I may have missed it, but I suggest including a statement that the model can also be used for single event scenario assessments? The model can and is regularly used for single scenario simulation. At GA, we use the model to provide guidance on wind field zones for Emergency Management Authorities based on track information provided by the Bureau of Meteorology (BoM). We can provide the wind fields to EMs on average within 30 minutes of publication of track forecasts from the BoM. See paragraph 5 of the Introduction.

3) Introduction or Conclusion: I suggest adding that the model can be run with any input track data, not just historical best track data. This broadens the applications of the model to be used in conjunction with, for example, TC track data from global climate models to study climate variability and change effects on wind exceedances.

Noted - we are presently working on using other input sources to demonstrate this capability. Manuscript updated to note other data sources can be used.

4) Section 4.5: It is stated that there are differences in the inland decay rates between the East and West coasts. But then a single decay rate model is used. Please justify this decision.

The driver of different decay rates in the east and west is topography. To minimise the data demands (especially with a view to global application), we did not include topography in the regression such that we do not have to source suitable topographic data for all potential basins. This is an area for future development.

5) Section 7.1: The somewhat poor performance of the model over Northwest Australia is explained by the lower genesis probabilities. How is it possible for the model to miss these local genesis patterns if it is sampling from the genesis probability surface?

I am yet to fully explore the causes of this issue in the reported version of the model. Other users have anecdotally noted relatively large number of short-lived weak systems in some basins, and we are now investigating methods to resolve this issue.

6) Conclusion: The introduction mentions the high cost of riverine and coastal surge flooding. Can a brief discussion be added on whether a TCRM-like approach could be used for TC rainfall and/or flooding?

Noted. Manuscript updated with a comment on application to other hazards - for example inclusion of a parameterised rainfall model for rain rates, use as input in surge modelling and wave modelling applications.

7) Figure 5: Please add the units of the genesis probability

Caption updated.

8) Figure 13: What do the colors of the lines represent?

Colours are for clarity only. Noted in caption.

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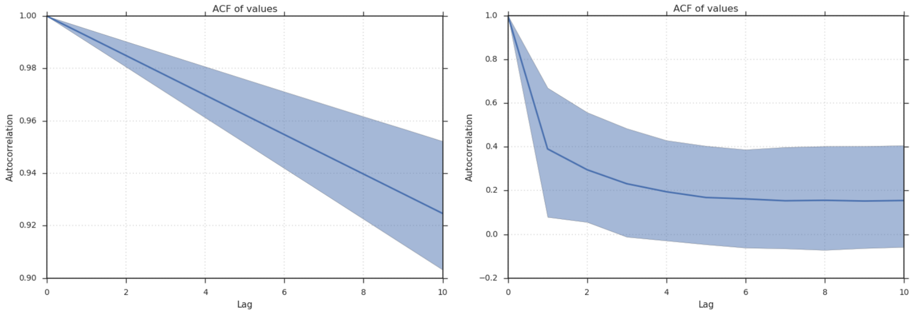

Fig. 1. Autocorrelation of minimum central pressure (left) and pressure rate of change (right) for lagged observations between 1 and 20 steps. See manuscript for more details.

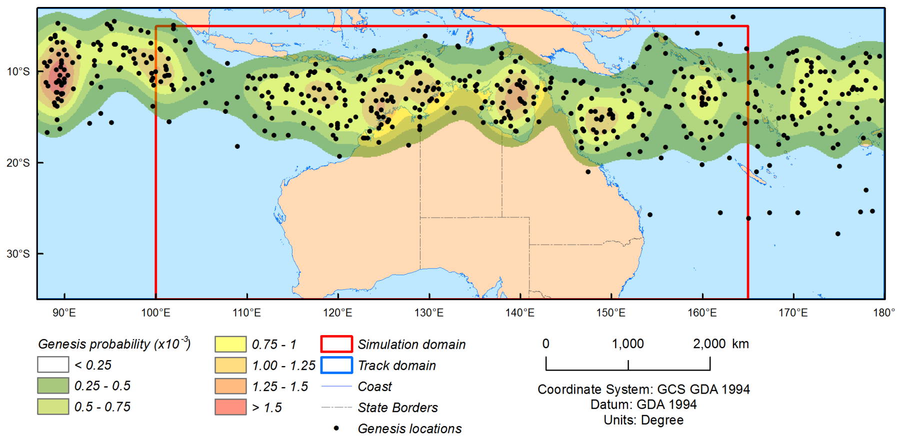

Fig. 2. TC genesis points for historical TC events (1981-2016), and the corresponding probability density function (TCs/year).