

Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2021-46

Anonymous Referee #2

Referee comment on "Simulations of Bay of Bengal tropical cyclones in a regional convection-permitting atmosphere–ocean coupled model" by Jennifer Saxby et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-46-RC2>, 2021

This manuscript documents the performance of the regional configuration of the UK met office model in predicting six tropical cyclones over the Bay of Bengal between 2016-2019. Although it is encouraging to see the authors documenting the model performance beyond simply looking at the track and intensity, and the manuscript is well written, I suspect that there are major issues with the model configuration that may need to be addressed for further improving the model skills. I have some major concerns related to this work hence I am rejecting this work in its current form. I am doing that because to address a few of my concerns the authors may need to re-run several of the cases or make a detailed analysis of why the errors are so huge.

Major concerns:

(1) The model suffers from huge phase errors which could lead to poor predictions of timings of land-fall, critical for forecasting. While those may not be obvious in Figure 1, those errors do show up in several of the other diagnostics. Track errors of Vardah for 05/12 is on the order of 1000 Km and, in general, for Gaja and Phethai are huge as well and may not be useful for forecast purposes (please compare the tracks from the operational models such as WRF or GFS in that region for those cases). Such huge track errors could be due to some large-scale issues which have not been analyzed here. Very likely those are coming from the driving global model. Why not plot the track errors from the driving model? Very likely the regional configuration is so small that such longer range predictions without two-way interactions may not even be possible. Very likely this regional configuration cannot predict tracks beyond 48 hours. Unfortunately, the number of cases isn't statistically significant to prove or disprove that the regional configuration is capable of predicting beyond 48-72 hours.

(2) The problem with the track and landfall timings affects intensity errors as well. If a formal intensity verification is done for Vardha, Gaja, and Roanu, all of them will be

extremely huge for a forecast model. I would encourage the authors to first look at the large scale and look at why there are such huge track and intensity errors. Very likely those are coming from phase errors originating at the boundary of the regional domain. Is the domain too small for forecasting beyond 48-72 hours? I suspect the WRF or other operational models used over that region should have provided much better statistics for all these cases. I think the author needs to make an analysis of why the track and intensity errors are so huge? I doubt if such errors could be useful for the forecasters.

(3) I am also concerned about the model comparisons that have been made between the atmospheric only (one-way interactive, to be precise) and the coupled versions. While the atmosphere only version is run at 4.4 km, the coupled version is run at 2.2 km. While we expect a higher resolution to provide improved intensity predictions, this has been the reverse here. The coupled version is producing excessive cooling. I don't see an analysis of why the ocean model is reducing excessive cooling. Is there a problem with the coupling? That also begs the question about the surface layer parameterization scheme used in the model. What are the Cd and Ck functions used? That could have a huge impact on the pressure/wind relationship as well.

(4) There is no Data Assimilation used in either of the configurations. Whereas time and again it has been proven that for improving regional forecasts of TCs, DA holds the key. Also, the behavior of the cycled system may be very different from the current system because whatever is lost in terms of the size of the domain could at least be nudged with better initialization from one cycle to another. Please note that DA is one major area that the NCMWF is focused on. The lack of DA in this configuration is surprising. Any regional models like the WRF come along with a DA system.

(5) Although, the rainfall evaluation presented here is fairly unique, what else is new in this work? The model lacks DA and produces huge track and intensity errors beyond 1-2 days on several important cases, the coupled configuration produces excessive cooling and the model struggled with all the rapid intensifying cases. In that case, the authors have to think if the regional configuration described in this work is really up to the mark to produce forecast quality outputs? I think this work does not show enough evidence that the regional model is a good forecast model nor I am able to see much scientific value beyond the analysis and rainfall verification.