Weather Clim. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/wcd-2021-46-RC1, 2021

Comment on wcd-2021-46

Anonymous Referee #1

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-RC1, 2021

Review of Simulation of Bay of Bengal tropical cyclones in a regional convection-permitting atmosphere-ocean coupled model by J. Saxby et al 2021.

The paper compares uncoupled and coupled simulations with a UK Met Office limited area modelling setup for 6 tropical cyclones in the Bay of Bengal. The comparison is done against observational data such as best track data and ocean observations and show comparable performance between the coupled and uncoupled models with the main difference being that the coupled model shows slightly weaker tropical cyclones consistent with other studies. One could argue that the comparison between uncoupled and coupled models is not completely fair in the paper, but the details of the comparison are discussed nicely in the paper.
The paper reads well, and the figures are clear and well chosen, so I recommend publication with minor revisions based on my comments below.

General comments:

G1: The description of the coupled modelling system is done by referring to an in-preparation paper (Casillo et al). Some of the details in that paper could be important for the interpretation of the coupled results but since it is not available yet then these details are obviously missing. I would like to see a bit more details of ocean model like for instance the vertical resolution in the upper ocean.
G2: There is also very little information on ocean spin-up used to produce the ocean initial conditions besides the reference to the in-preparation paper. Since we are looking at tropical cyclones then the quality of the winds used as forcing for this spin-up run could influence the ocean stratification in ocean initial conditions leading to differences in coupled model performance.
G3: It is not clear to me what the SST used in the coupled model actually is. Is it the top layer from the ocean model, the mean of the levels in the first 10 m or something else? Is there a diurnal cycle in the coupled SST or not?
G4: Related to G3. The authors chose to compare with ARGO observations rather than

surface drifters which are reporting more frequently. I accept the argument that the profiles used have almost constant temperature in the uppermost 10 meters so it fit well with the prescribed SST used for the ATM runs, but how does it relate to the SST of the CPL runs?
G5: I think that the authors could have compared the ocean stratification of the ARGO floats and the RAMA moorings to both the ocean initial conditions and coupled integrations to gain some insight on the ocean performance of their system, but maybe that is a separate paper?

Minor comments:

M1 Page 5 line 114: It is not completely obvious to me that degrading the OSTIA SST using in the ATM runs with 1 day is a "fair" benchmark to compare with the CPL runs, since this could not be done in an operational settings. Maybe this could be rephrased?
M2 Page 5 line 121: "Madec Gurvan et al, 2019" should be "Madec et al, 2019".
M3 Page 5 lines 123-124: What about the SST received from the ocean model? Is that also a hourly mean?
M4 Page 6 table 1: There is more than one CMEMS global reanalyses, so which one is used for the ocean lateral boundaries?
M5 Page 21. It would be appropriate to include a reference to the ARGO project here and not just in the acknowledgements. Details can be found on https://argo.ucsd.edu/data/acknowledging-argo/
M6 Page 33 Acknowledgements: I think that a link to the CMEMS global ocean analysis used in this study should be included.