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Geoscientific Model Development Discussions

Interactive comment on "Development of an atmosphere–ocean coupled operational forecast model for the Maritime Continent: Part 1 – Evaluation of ocean forecasts" *by* Bijoy Thompson et al.

Anonymous Referee #1

Received and published: 4 November 2020

General comments

The authors start by presenting the development of an atmosphere-ocean coupled operational forecast model for the Maritime Continent. The authors then evaluate the model's performance to forecast SST, temperature and salinity in the water column, and SSH. The coupled model uses OASIS3-MCT libraries to couple a state-of-the-art atmospheric (MetUM) and ocean (NEMO) model with a 4.5x4.5 km horizontal resolution. Besides, the authors evaluate the ocean-only model (NEMO) in hindcast mode.

Overall, the paper addresses relevant scientific modeling questions within the scope of the Geoscientific Model Development (GMD). The development of a 4.5x4.5 km horizontal resolution ocean model coupled with an atmospheric model for the complex environment of the Maritime Continent can, in the future, be used to address relevant scientific questions within the scope of the European Geosciences Union (EGU).

The paper represents advances in ocean modeling for the Maritime Continent, and its methodology can be adapted to other oceans. This is because the methods and assumptions are valid and clearly outlined by the authors. Overall, the results presented for hindcast and forecast simulations are sufficient to support the authors' interpretations and conclusions. The paper is well structured; the abstract provides a concise and complete summary of the work presented. The language is fluent and precise. Overall, the number and quality of references are appropriate; however, some references are missing in the references list.

Based on this, my opinion is that GMS should publish this paper. However, some contents deserve to be better presented and discussed. The following sections present all the comments and recommendations following my review.

Specific comments

Comment #1 – (Figure 1 and line 149) A reference for GEBCO 2014 should be provided and added to the references list. Are the authors considering updating MCao bathymetry based on the GEBCO 2020 dataset? Although GEBCO 2019 and GEBCO 2020 were not available when the simulations were performed, these datasets were available when the paper was submitted. The authors should then discuss how using a 15 arc-second resolution bathymetry dataset could influence model results.

Comment #2 – (line 80 and 81) Please provide references to support this statement ("For instance,. . . Pacific Ocean").

Comment #3 - (line 81 and 82) Please provide references to support this statement

("The north Pacific Ocean… tropical cyclone annually").

Comment #4 – (line 116 and 117) Why did the authors decide to change the values of background vertical eddy viscosity and eddy diffusivity coefficients?

Comment #5 – (line 167) Could the authors briefly explain how they managed not to have these numerical issues in MCao? This information can be handy for other authors that want to implement similar model systems based on NEMO.

Comment #6 - (line 168) FES2014b is not in the list of references.

Comment #7 - (line 190) What is the external source for MSLP?

Comment #8 – (line 194) Please provide a reference for Mercator global ocean reanalysis.

Comment #9 – (line 254) Please provide a reference for the Operational Sea Surface Temperature and Sea Ice Analysis.

Comment #10 – (Figure 4): Although Figure 3 mentioned that the Bay of Bengal region is excluded from the analysis-domain, results are presented for this region in Figure 4. Could the authors clarify this better in the text? Moreover, I suggest writing the abbreviations of each sub-region in the map of figure 3. This will help readers not familiarized themselves with the Maritime Continent.

Comment #11 (line 295) – What could be the reason for the observed SST bias in the Andaman Sea region?

Comment #12 (line 366 to line 368) – What could explain this?

Comment #13 (Table 4) – Can the author elaborate on the reasons for the decrease of SST Bias overtime (Bias decreases for higher forecast lead times). This is a general trend for all the sub-regions (exception for ASMS). In general, it is expected that the accuracy of SST decreases with increasing lead times. However, it seems that Bias is not showing that.

Comment #14 - In Figure 9a, results are presented for Mooring M1(95E, 8S). However, in Figure 9b, 9c and 9d results are presented by MCO_ao at 95E, 5S. Is this a typo? Based on the text in the manuscript, I believe this is a typo. However, the authors must double-check if they compared observations and model results at the same lat/lon.

Comment #15 (line 424 to line 435, and Table 6) – The authours say "the analysis mainly demonstrates the model performance in the domain excluding the SCS region". This is understandable. However, it is impossible to understand if Argo and XBT profiles available from 1 October to 5 November 2019 represent the other sub-regions. Did the author calculate RMSD for all the sub-regions (excluding SCS)? Does RMSD change from sub-region to sub-region?

Comment #16 (Table 6) The legend of Table 6 indicates "observation during October 2019". However, in line 424 the authors mentioned: "for the period 1 October to 5 November". Please clarify this point.

Comment #17 (table 7) - The 19 tide-gauge location used to evaluate the model performance to simulate SSH should be presented in Figure 3. Although the lat/lon of each tide-gauge is provided in Table 7, its location in a map will make it easier for readers to understand better where the authors evaluated SSH performance.

Technical corrections

Line 117 – In line 111 MC_ao is defined as MCao. Please use only one nomenclature for the MC atmospheric-ocean coupled model.

Line 143 – Do you mean Parallelise instead of PArallelise?

Line 269 – Replace "set of variables an d" by "set of variables and"

Table 5 – Please delete the space in "observation s"

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