

Interactive comment on “A regional coupled ocean—atmosphere modeling framework (MITgcm—WRF) using ESMF/NUOPC: description and preliminary results for the Red Sea” by Rui Sun et al.

Anonymous Referee #2

Received and published: 22 January 2019

Overall comments:

It is neither a technical nor a science paper. It would be beneficial to re-focus the manuscript on one aspect by clearly stating the problem, hypotheses and discuss the findings. Based on a few snippets of the manuscript it comes across that the authors are vaguely familiar with the foundations of numerical modeling in the atmosphere; they got two open source models, coupled them (no small feat!), and ran a test case. What is missing is a critical look at the approach, results, discussion of why things worked and more importantly, why not. I suggest to omit the whole section on scalability. The

C1

experiment design does not support any meaningful conclusions for scaling purposes. I would also recommend proof-reading (not spell checking!) the manuscript.

Specific comments:

Page 6, lines 21-32: Too technical. Page 6, line 32: Why was sequential mode selected? Page 7, lines 17-31: Is it a 30-day long run? How frequently are you forcing lateral boundary conditions? What is the projection? What is the lateral boundary condition type? Why is coupling every 20 minutes? Why 8 km grid spacing? Page 8, lines 13, 16: ‘accessing’, ‘accesses’ should be ‘assessing’, ‘assesses’ Page 9, Table 1: The second ATM.STA should probably be ATM.DYN. Page 9, line 9: Is MERRA-2 really an independent data compared to ERA5? The forecast model is, but the observations do overlap quite a bit. Page 10, line 21-22: Why not use a nest with finer grid spacing to resolve the local topography? Page 10, lines 10-32: When comparing to ERA5 data, how were the statistics computed? Was the model output interpolated onto the observation points in space and time? Page 11, Figure 3: There was a gray stripe at the bottom, making it impossible to read labels of the color bars. Page 11, line 16: Land surface model and PBL model are not microphysics models. Page 13, Figure 5: Are there missing data points for the observed high and low T2, e.g. Mecca and Yanbu 6/21, Yanbu 6/8, 6/10, 6/14...? Page 14, Figure 6: Are model points interpolated to ERA5 points over the Red Sea? Which simulation is ATM.CPL, it has not been introduced in Table 1? How do you explain the drift (blue line)? How can RMSE be negative in the lower right figure?! Page 14, line 4-6: Where there many clouds present during that period? Page 14, line 4-6, and line 12-13: First you state the forcing is different due to ‘uncertainty in cloud modeling’, then you state ‘both simulations are driven by realistic atmospheric forcing’. Which one is correct? Please explain. Page 16, line 13-14: Any cloud comparison? Page 16, Figure 8: Why no time series comparison to MERRA-2 dataset? Page 19, line 6: Which selected micro-physics schemes? Page 20, line 3: 64 /cm/year should read 64 cm/year? Page 21: line 8-9: What does it mean ‘The decrease in parallel efficiency is because when using 256 processors, there are

C2

only 16x16 grid points in the horizontal plane'? Page 21: line 11-13: Please elaborate: 'This may be attributed to the fluctuation of the CPU time when solving the systems of linear equations. When using different number of processors, the decomposition of the domain leads to different linear equation systems, requiring different CPU load and accordingly different convergence time. Page 21-23: Did you try weak or strong scaling? What is the communication cost? I/O cost? How many grid points per core are recommended? Are you reporting average times of multiple simulations in Table 3? How does WRF scale, MITgcm scale – do your results fit? Why did the coupling cost increase when using more cores? Page 23, Table 3: Please use the experiment names consistently throughout the manuscript.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-252>, 2018.