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Interactive comment on "The improvements to the regional South China Sea Operational Oceanography Forecasting System" *by* Xueming Zhu et al.

Anonymous Referee #2

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Zhu, et al, present a look at changes to an existing operational South China Sea regional forecasting system, SCSOFSv1. They examine a number of changes to the system including: i) revision of the model grid and discretization; ii) change to the atmospheric forcing from direct forcing to utilizing the COARE bulk algorithm; iii) changing the advection scheme; and, finally, iv) making improvements to the ensemble optimal interpolation assimilation scheme.

The results show various improvements for the changes that have been made to the operational system. The manuscript is more of a technical report than a research article. While the results are not general, or improve our understanding of the science

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Discussion paper

of predictability in the region (dynamical analyses, etc.), there may be some useful information that would be valid to a specialized reader. So, in the interests of providing information, it is a paper that when in final form could be published in Ocean Science.

That said, the paper is fundamentally lacking in two primary ways. First, to be of any use to that select reader, it must provide details and explanation for the changes and how they would dynamically improve the representation of the region. Secondly, the paper lacks any rigorous analysis of the "improvements." Simply stating that one thing was changed and the summertime surface temperatures are cooler is not particularly useful.

First, I will go through the changes discussed by the authors as topics, then paper comments.

The first change made to the model is changing the model grid representation of the region. The bathymetry is is re-generated, smoothed, and the number of layers increase. The authors state that they smooth the bathymetry to reduce the bathymetric slope to an r-factor under 0.2. However, for an s-level model with various stretching function, the so-called "Haney" number is more valid and it increases with increasing the number of layers. There is no analysis as to whether this grid has improved spurious pressure-gradient flows or can better resolve the complicated bathymetric derived flows of the region. Figure 1 seems to show that the Luzon strait may be far too deep, which would alter the early Kuroshio flow with a more predominant intrusion through the strait. Secondly, the authors remove the island of Guam because it disrupted the flow in the v1 of the system. Guam lies as the southern island in the Mariana island arc that extends from the Mariana trench to the surface to impinge on the westward flow. Guam (as part of the archipelago) does disrupt the flow. If v1 of the system had an incorrect disruption, this implies that the boundary conditions used are not necessarily appropriate or account for the archipelago. More analysis and discussion of these items is needed to show that the new grid better represents the dynamics of the region.

The second change is the switch from direct forcing (momentum, heat, and salt) to utilizing the COARE bulk fluxes algorithm. Of course, the COARE algorithm has been in wide use for two decades in modeling, so the authors don't have anything new to present on the topic; however, the changes and differences seem to suggest not an improvement by the COARE algorithm, but rather a deficiency in the original direct forcing used by the original system. The COARE algorithm produces direct forcing using the atmospheric conditions and the SSS and SST of the model at the time of the forcing. This time dependency is obviously advantageous; however, to see such strong changes in the means shows that there was/is a forcing issue. The direct forcing should be nearly comparable, just lacking the ability to adjust to local intra-seasonal variance.

The third change was to switch advection schemes from a third-order, upstream in the horizontal and a central difference in the vertical to using the Akima scheme for both horizontal and vertical advection. The advection schemes, by design, represent the wavenumber spectrum differently, and in reading this manuscript, it is unclear why one would be preferred over another. Some cursory comparisons are provided, but how is this a result that would be useful to the community? There needs to be some explanation and analysis on how the flows are represented by each scheme. In the third-order scheme, it is far less sensitive to the prescription of explicit horizontal viscosity. What about in the Akima scheme? Did you vary your viscosity for momentum and diffusion for tracers? Is the sub-grid scheme chosen (authors did not mention which scheme, e.g., Mellor-Yamada 2.5, LMD, etc.) sensitive to the advection scheme?

The final change was to modify the ensemble optimal interpolation (EnOI). The authors reference an earlier paper, Zhu, et al., 2016, that does not contain details about their EnOI implementation. Oke, 2002, does a good job of explaining the EnOI methodology as a simplification of the ensemble Kalman Filter; however, it has a number of choices that must be made in its implementation. The authors of this paper do not describe any of their choices, their impacts, or reasoning. First, the choice of ensemble members. It is not well described in the manuscript. I will explain my understanding; however, I am unsure how correct this is due to the lack of any detail. The authors break a 7 day assimilation window into 3 hourly periods. All observations are assigned to their nearest 3 hour slot, providing a total of 57 ensemble members. For each member, the innovations are calculated, and used for the EnOI setup. There is no discussion of localization, which is absolutely required in the ensemble scheme increments.

The authors take the increments and turn them into temporal nudging by dividing the EnOI derived increments by 57 and applying them weighted through time. The way it is presented would not seem to make sense. There are 57 innovations relative to the background. However, once an increment is made, the subsequent increments are now invalid because they are now being applied to a field with different innovations. This is why the original EnOI scheme applied the increments at the beginning of the window. Furthermore, localization is required to make this somewhat reasonable.

The final issue with this is that for a forecasting system, the authors never examine the forecasts. Adding forcing through time should reduce your forecasting skill because as soon as the forecast begins, the artificial forcing term disappears and the system is "shocked" back to its original state with the forcing that was "holding" it in place removed. How did these improvements change the forecast ability of the system? Does the EnOI system provide significant improvement over a model with the "best" atmospheric forcing?

* Minor Comments

1. The authors are non-native English speakers, so I give some leeway, but the manuscript is difficult to read and confusing in many places that are too many to list here. Example lines: 68, 120, 123, 189, paragraph at 195, sentence at 210, 229, most of the EnOI description, 426,467, 473, 540, 545)

2. Figure 1: This map ratio is strange. There are roughly 25 deg in the vertical and 45 deg in the horizontal. The Philippine Sea is presented as smaller than the SCS. Likewise Figure 2 doesn't show the impact of changes on the SCS, the entire point of

the paper.

3. Paragraph near line 140: Why would you use a different set of initial conditions from the boundary conditions you are using? What is the persistence of the initial condition information? Over the 16 years or so that you spinup experiments, wouldn't the boundary conditions replace the initial conditions? There is no explanation for why you would use one product to initialize the model and SODA as your lateral boundaries.

4. Line 233 states that coastal sea surface forcing was "heating up the ocean". The region is massive with a deep ocean basin. How would some slight imbalances along a coast heat up the entire ocean of like 124 km2?

5. Line 241: Figure 4 is not a histogram. It is a bar chart.

6. Figure 5 caption, should this be SCSOFSv1 in (c)?

7. Paragraph around 275: You spin up the model until it is "in stable status". What does this mean? How do you determine "stable status"? During the spinup, you say that the temperature increases, but how does SODA compare? As per (3) above, are you not just converging towards the SODA state?

8. Line 360, for the EnOI, you generate seasonal background error covariances. Wouldn't it be more appropriate to have typical 7-day background error covariances? The covariance over a season is very large compared to the variability over 7-days. You should expect that your model is within a 7-day covariance period of the observations?

9. Section 4, "Scientific inter-comparison" I don't really see any scientific comparison, just comparing the bias, RMSE, and AC.

10. Table 2, what is the final row?

11. Line 460, what is "PI"?

12. Figure 11c, there are strong RMSE on either side of the Luzon Strait. The observations capture the surface bounce of the strong internal M2 tides there, resulting in a

25cm "error" that is present in altimetry because the surface bounce of internal tides vary in phase and location, meaning they aren't removed in the sea surface height measurements. So, 11c looks exactly as it should. But, 11d shows significant reduction in something that your model doesn't include. You don't have tidal forcing (at least explained in the manuscript), so you don't have the process present to reduce the error. This means that your EnOI is doing something non-physical to the system by trying to force an SSH expression that is due to internal tides.

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