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

Anonymous Referee #1

This article describes the progressive developments and improvements to a regional ocean forecast system of the South China Sea. The improvements demonstrated are worth reporting, and will likely be of interest to other researchers. But the level of detail and explanation is insufficient for the paper to really be valuable. With extra detail, the paper will likely be a good contribution to this field.

The authors thank the reviewer for the insightful comments, and we completely agree with all questions and comments raised by the reviewer, which have helped us to improve the quality of the manuscript, especially to achieve the level to be valuable publishing for the paper. We have tried to add more details and explanation of those improvements demonstrated in the revised manuscript.

The main problem with this paper is that essential information is not included. Specific examples of exclusions follow.

Re: Ensemble Optimal Interpolation (EnOI) configuration

An important element of an EnOI system is the construction of the ensemble. This is apparently described between lines 337 and 348. The details presented are unclear and don't sufficiently describe how the ensemble is constructed. After reading this passage a few times, I could speculate a few different ways the ensemble is constructed. This needs to be improved. How exactly were the ensemble members constructed? A clear way to describe this is like: "5-day averaged fields are subtracted from a 10-year average", or "5-day averaged fields are subtracted from 60-day averaged fields", or similar. Then an interpretation could be given. For example, "the spatiotemporal scales represented in the ensemble of anomalies represent intraseasonal variability, or mesoscale processes".

Thanks for pointing this out. The motivation of this paper is to introduce changes and improvements from SCSOFSv1 to SCSOFSv2. Many details about model configurations and EnOI systems were described in our previous papers (Zhu et al.(2016), Ji et al.(2015)). We have revised the whole paragraph to describe the construction of the ensemble in SCSOFSv1 and SCSOFSv2 in detail as reviewer's suggestions between lines 375 and 392 in the revised manuscript.

Modification:

L375-392: revised to "Secondly, we have introduced the method of computing the anomalies of ensemble numbers used for constructing the background error covariance following Lellouche et al. (2013). In SCSOFSv1, the anomalies are computed by subtracting a 10-year average from a long-term (typically 10 years) model free run snapshots with 5-day interval for the ocean state, i.e. sea surface height and three-dimensional temperature, salinity, zonal velocity, and meridional velocity. And the ensemble is selected within a 60 d window around the target assimilation date from each year, adding up to about 130 members in total (Ji et al., 2015; Zhu et al., 2016). However, in SCSOFSv2, a Hanning low-pass filter is employed to create running mean according to Lellouche et al. (2013) in order to get intra-seasonal variability in the ocean state. Thus the anomalies are computed by subtracting the running mean with 20-day time window from a 10-year (2008-2017) free run daily averaged results. Especially, it is pointed out that the daily averaged free run results are selected within 60 d window around the target assimilation date from each year of 2008-2017 and used to compose ensemble members, thus about 590 members totally in SCSOFSv2. It means that the background error covariances rely on a fixed basis and intra-seasonally variable ensemble of anomalies, which improves the dynamic dependency."

The authors also don't describe the ensemble size, or whether covariance localization is used. If localisation is used, what length-scales were chosen?

Thanks for your reminding. We have revised the manuscript and pointed out that the ensemble size is 130 in SCSOFSv1 and 590 in SCSOFSv2. For the covariance localization, since it is mentioned in Ji et al. (2015), and has not been changed in SCSOFSv2, we did not mention in the original manuscript. We have clarified that the localization radius is 150 km in the revised manuscript between lines 409 and 410.

Modification:

L409-410: added "Meanwhile, the localization is still used with the radius set to be 150 km as in SCSOFSv1."

The authors don't say what observation errors are assumed – they merely mention in passing that estimates are made (L335).

Thanks for pointing out this issue. Please see the revised version between lines 371 and 374.

Modification:

L371-374: added "For the observation errors in SCSOFSv2, we simply set those of SLA and SST as constants of 0.09 cm and 0.5 ℃, respectively; as for those of Argo T/S, assuming they are represented as a function of water depth (*D*) following Xie and Zhu (2010) as $ERR_T(D)=0.05+0.45exp(-0.002D)$, $ERR_S(D)=0.02+0.10exp(-0.008D)$."

The authors report improvements by assimilating more observation types (L332), constructing their ensemble differently (L337 – though, as noted above, the explanation provided is insufficient), introducing FGAT (L360), and by applying increments using IAU (L372). None of these techniques are new or novel. The statement: "Actually, it is close to impossible to calculate the synchronous innovations between the observation and model forecast entirely, since the temporal distributions of SLA and Argo data are irregular and variable at each analysis step", is untrue. Perhaps it's not a convenient calculation. But it is entirely possible, and implemented in many systems that use FGAT. In fact, the authors go on to explain how they did this (from L360).

Thanks for the advice. The motivation of our paper is to demonstrate the technic details of a new forecasting system comparing with its precedent counterpart. We strongly agree with reviewer's point that none of these techniques are new or novel, but they are concurrently implemented for the first time in SCSOFSv2 with respect to its previous version SCSOFSv1. From our point of view, the primary objective of operational oceanography forecasting systems are more about accuracy than innovation, that's why we put our focus on the improvements of forecasting performances, rather than innovate techniques or methods only.

Although we did not develop new algorithm or parameterization, we consider it's a technical innovation by improving our operational forecasting system in the SCS in the way of implementing all those techniques in the new version of SCSOFS and increasing the forecasting accuracy.

For the statement about FGAT, we have revised the manuscript by adding more explanations between lines 404 and 410. We understand the fact that it is entirely possible to calculate by adding codes, but it would bring up the questions of significant increasing compute and storage cost. As for synoptic operational forecasting, we need to reach a balance between forecast accuracy and the computing and storage cost, so we consider 3-hour time slot used for calculating the innovations between the observation and model forecast with 1.5 hours misfit should be enough.

Modification:

L406: "running" revised to "run"; "the previous analysis" revised to " the previous analysis run".

L407: "minus" revised to "subtract".

L408: "time" revised to "temporal"

Re: Model configuration

One of the "solutions" implemented to address a problem with the model's mean circulation (see Figure 2) is to shift the eastern lateral boundary to the west by one degree. This excludes Guam Island from the configuration, and apparently results in an improved mean flow. This approach doesn't seem quite right. The authors have eliminated one element of the system (an Island), by making the model domain smaller. Exclusion of a real physical to improve the model's representation doesn't seem like a step forward. It would be better to understand how the presence of the Island influences the circulation, and then understand how the model can be reconfigured to more faithfully represent this influence.

Thanks for mentioning about this. We completely agree with the reviewer that our approach is simple and not quite right by excluding a real physical in science, instead of analyze how the model represent the island influence. As shown in Figure 2a, the NEC is split into two branches near the east lateral boundary due to the open boundary effects. For our focus domain is the interior of SCS, it's not that difficult to associate the solution of shifting the eastern boundary to the west to exclude Guam Island to get a reasonable large scale background circulation (NEC) for the interior of the SCS. Actually, we had done a serious of tests to decide optimal scheme of the eastern lateral boundary by comparing results of moving which westwards by 0.1°, 0.2°, 0.5°. We found that shifting the eastern lateral boundary westwards by 1° is the best option to get reasonable NEC pattern. This solution might seems simplistic, but is effective to improve the system. We could do more tests to understand how the presence of the Island influences the circulation, but it may beyond of the scope of this paper.

Another change to the system is the adoption of bulk surface fluxes (see Figures 3 and 4, and section 3.1), rather than prescribed fluxes. This is a sensible change, but is not new or novel.

Yes, we agree with the reviewer that the adoption of bulk surface fluxes is not a new or novel technique. But it is also an efficient approach to improve the forecasting accuracy in SCSOFSv2.

The authors refer to advection schemes, UCI and AAG. No reference is given, nor are these schemes described. Yet the authors identify the change from UCI to AAG as a key change that resulted in a substantial improvement in their system. The authors show that they significantly *reduced the temperature and salinity bias in their system by adopting a different advection scheme (re: Figures 5, 6, and 7; and section 3.2). They refer to another study that showed the same result. This is again a good improvement, but again, it's not new or novel.*

In this paper, we use UCI to refer to the schemes combination of third-order upstream horizontal advection (U3H), fourth-order centered vertical advection (C4V) and horizontal mixing on epi-neutral surfaces for tracers(ISO), and AAG refer to the schemes combination of fourth-order Akima scheme for both horizontal and vertical advection (they are denoted as AA), and horizontal mixing on Geopotential surfaces (constant Z) for tracers (it is denoted as G), respectively. It has been described between lines 272 and 275 in the original manuscript. All schemes of U3H, C4V, Akima, mixing on epi-neutral surface, and mixing on geopotential surface, have been implemented in ROMS. We have given mode details about the differences between the Akima and four-order centered schemes between lines 309 and 313 in the revised manuscript. For most of ROMS community, the UCI schemes combination is the default and commonly settings. So the AAG schemes combination is proposed in this paper. We have finished many tests on various model settings to fix the the temperature and salinity bias, just put the optimal settings with the best results into this paper. And we are preparing a separate paper to show details of how tested model settings effect on this problem in order to keep the focus on the improvements of SCSOFS in this paper.

As we mentioned in previous replies, the motivation of our paper is to demonstrate the technic details of a new forecasting system compared with its precedent counterpart. The techniques and parameterization scheme are not new, but they are concurrently implemented by our forecasting system and perform better than previous version. In recent years, we found that many operational oceanographers mainly focus on how to improve the data assimilation technology when they want to improve the forecasting skills of the forecasting system, but pay less attention on the performance of physical model itself. Some forecasting systems are overly reliant on data assimilation while their pure physical models cant simulate reasonable large-scale circulation even. However, we think differently. Data assimilation should be adopted as a last resort under the premise that physical models cannot improve their forecasting skills. For example, the forecasting skill of sea surface temperature can be greatly improved by changing the surface forcing mode from prescribed fluxes to bulk surface fluxes (although it is not a very new or novel technique) as described in this paper, instead of increasing the amount of calculation as the introduction of data assimilation. Therefore, another main purpose of this paper is to tell the reader (operational oceanographer) that it will be once and for all to improve the forecasting skills of the system by improving the configuration of physical models, thereby to improve the prediction performance of the system in essence.

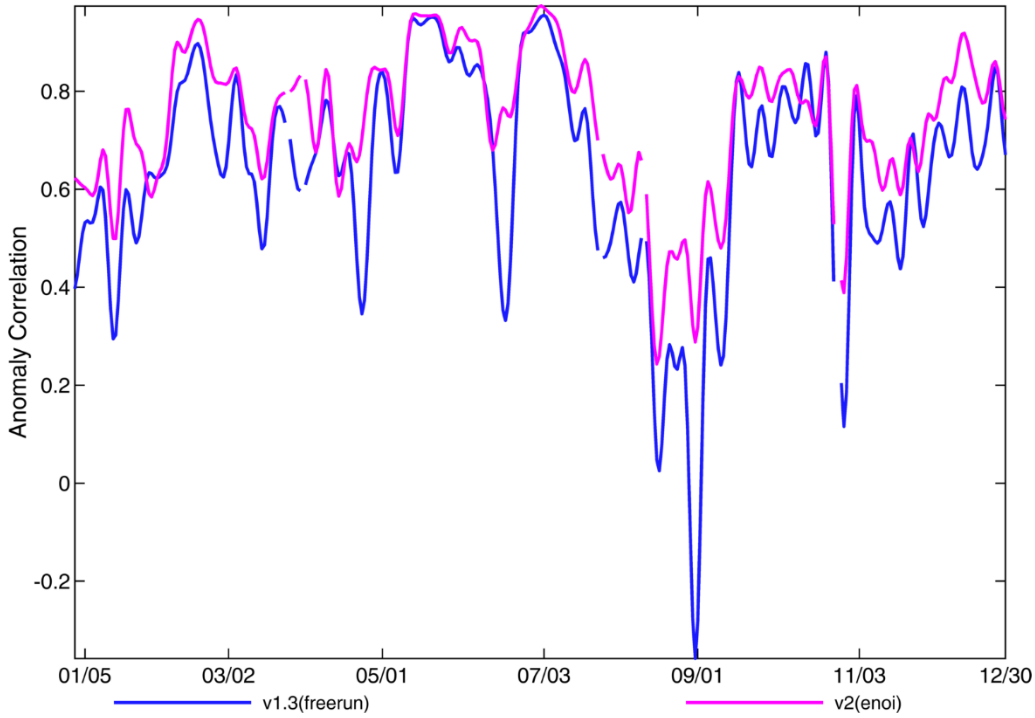
Modification:

L309-313: added "The fourth-order Akima scheme is a little different from the fourth-order centered scheme by replacing the simple mid-point average with harmonic averaging in the calculation of curvature term. Since the time stepping is done independently from spatial discretization in ROMS, the Akima scheme represents its advantage of reducing spurious oscillations, which arises with nonsmoothed advected fields, with respect to the fourth-order centered (Shchepetkin and McWilliams, 2003, 2005)."

Re: results

The new system seems to be better than all previous versions based on most metrics. Although this is not always the case. Figure 10 shows that for SST there is a period of 2-3 months when the new version has a smaller anomaly correlation compared to other versions (Figure 10). This needs to be explained.

Thanks for pointing this out. Actually, we have illustrated that v1, v1.1, v1.2, v1.3 are without data assimilation in 2013, but v2 is with data assimilation in 2018, in Figure 10's illustration. In order to simplify, we do not show the result from v1.3 without data assimilation in 2018. Now figureR1 shows those results from v1.3 (freerun) and v2 (with EnOI data assimilation) in 2018. It is found that the new system is still better than previous version based on anomaly correlation of SST in 2018.



FigureR1: The anomaly correlation of SST best estimates for v1.3 and v2 in 2018.

Re: data availability

The authors claim that "No datasets were used in this article". This clearly isn't true. Perhaps the authors mis-understood what was expected here.

Thanks for pointing it out. We have added it in the revised manuscript and removed some links in the text.

Modification:

L642-652: revised to "*Data availability.* GEBCO_2014 Grid, https://www.bodc.ac.uk/data/open_download/gebco/GEBCO_30_SEC/zip/, last access 3 January 2021; SODA 3.3.1, https://www2.atmos.umd.edu/~ocean/index_files/soda3.3.1_mn_download.htm, last access 3 January 2021; SODA3.3.2, https://dsrs.atmos.umd.edu/DATA/s oda3.3.2/REGRIDED/ocean/, last access 3 January 2021; CFSR, http://rda.ucar.edu/datasets/ds093.0, last access 3 January 2021; CFSv2, http://rda.ucar.edu/datasets/ds094.0, last access 3 January 2021; NCEP_Reanalysis 2, https://www.psl.noaa.gov/data/gridded/data.ncep.reanalysis2.html, last access 3 January 2021; AVHRR, http://www.ncei.noaa.gov/data/sea-surface-temperature-optimuminterpolation /v2.1/access/avhrr/, last access 3 January 2021; OSTIA, SST of *in-situ* drifting BUOY, AVISO along-track SLA, and Argo temperature and salinity profiles, https://marine.copernicus.eu/, last access 3 January 2021.

Reference:

Beckmann, A., Haidvogel, D.B.: Numerical simulation of flow around a tall isolated seamount. Part I: problem formulation and model accuracy. J. Phys. Oceanogr. 23, 1737-1753, 1993.

Ji, Q., Zhu, X., Wang, H., Liu, G., Gao, S., Ji, X., and Xu, Q.: Assimilating operational SST and sea ice analysis data into an operational circulation model for the coastal seas of China. Acta Oceanol. Sin., 34, 54-64, 10.1007/s13131-015-0691-y, 2015.

Li Ang，Zhang Miaoyin，Zhu Xueming*, et al. A research on the optimal approach of CFSR surface flux data correction based on different surface forcing modes. Haiyang Xuebao，2019, 41(11)：51–63，doi:10.3969/j.issn.0253−4193.2019.11.006. (In Chinese with English abstract)

Marchesiello, P., Debreu, L., and Couvelard, X.: Spurious diapycnal mixing in terrain-following coordinate models: The problem and a solution, Ocean Model., 26, 156-169, 10.1016/j.ocemod.2008.09.004, 2009.

Shchepetkin, A. F., and McWilliams, J. C.: A method for computing horizontal pressure-gradient force in an oceanic model with a nonaligned vertical coordinate, J. Geophys. Res., 108, 3090, 10.1029/2001JC001047, 2003.

Shchepetkin, A. F., and McWilliams, J. C.: The regional oceanic modeling system (ROMS): a split-explicit, free-surface, topography-following-coordinate oceanic model, Ocean Model., 9, 347-404, 10.1016/j.ocemod.2004.08.002, 2005.

Xie, J., Zhu, J.: Ensemble optimal interpolation schemes for assimilating Argo profiles into a hybrid coordinate ocean model, Ocean Modelling, 33(3 – 4): 283 – 298, 10.1016/j.ocemod.2010.03.002.

Zhu, X., Wang, H., Liu, G., Régnier, C., Kuang, X., DakuiWang, Ren, S., Jing, Z., and Drévillon, M.: Comparison and validation of global and regional ocean forecasting systems for the South China Sea, Nat. Hazards Earth Syst. Sci., 16, 1639-1655, 10.5194/nhess-16-1639-2016, 2016.