

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

Anonymous Referee #1

Received and published: 14 December 2020

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 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
C1

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”.

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

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

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). 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 elim-

inated 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.

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.

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.

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.

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.

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-104>, 2020.