Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-8-RC1, 2018

Ocean Science
Discussions

Interactive comment on "Assimilating High-resolution Sea Surface Temperature Data Improves the Ocean Forecast in the Baltic Sea" *by* Ye Liu and Weiwei Fu

Anonymous Referee #1

Received and published: 27 March 2018

The marine status of the Baltic Sea is highly variable and influenced by the forcing from atmosphere and freshwater influx due to shallow topography and semi-enclosed restriction. As a stable observation source, the high-resolution SST from satellite is rather important to improve the ocean operational forecast to serve the Baltic industry needs. The article of "Assimilating High-resolution Sea Surface Temperature Data Improves the Ocean Forecast in the Baltic Sea" use a localized Singular Evolutive Interpolation Kalman (SEIK) filter to assimilate the OSISAF SST during one year of 2010. Compared with dependent and independent observations, the evaluation of the model runs with and without assimilating the SST shows the SST modeling has been improved clearly. This study is suitable for publication in OS, but there are still some

obvious defects like the experimental illustration is not clear, lack of conclusions or analysis methods to inspire the readers.

The main comments are listed as follow:

1) In this study, only to assimilate the OSISAF SST in the Baltic sea. In fact, there are more SST candidates with equivalent high-resolution like OSITA (CMEMS) and RTG_SST_HR (http://polar.ncep.noaa.gov/sst/rtg_high_res/). So if assimilating one or two additional SST products, the related results will be more help the reader to well understand about them. On the other word, the special features about the OSISAF SST in the Baltic Sea have not been highlighted at current, which looks not to support the study focused on it.

2) Lines 163-165, this SST product from AVHRR is available twice daily. It is not clear how to assimilate in the experiment. The assimilation time window is daily? How to calculate the innovation, is it asynchronous?

3) In the first paragraph of 3.1, the assimilated SST has been filtered by the quality. But it is not clear how to consider the sea ice. Do you use the sea ice concentration of OSISAF to mask the SST product, and how to do?

4) The observation error for the OSISAF SST is important for this study, is it a constant of 0.5 degree used? As a good consistence check, some diagnostic about the assimilation stability like Rodwell et al. (2016) is beneficial to understand the system reliability and the observation error.

Rodwell, M. J., Lang, S. T. K., Ingleby, N. B., Bormann, N., Hólm, E., Rabier, F., Richardson, D. S., and Yamaguchi, M.: Reliability in ensemble data assimilation, Q. J. Roy. Meteor. Soc., 142, 443–454, doi:10.1002/qj.2663, 2016.

5) The IceMap has been used for evaluation as one independent SST observation. It is not objective and only twice for one week. In fact, another surface water temperature data set from SMHI collected by Ferry

(http://www.smhi.se/hfa_coord/BOOS/Ferrybox/BSNI/BSNI-Wtemp.png) is more useful and independent for this study.

6) The two in situ observations at Arkona and BY15 is super case to show the impact of assimilating SST only. It is valuable to do more specific analysis by diagnosing dynamic variables. Firstly, investigating the mixed layer depth in the two runs can clearly show the mixing strength for Fig.5 and Fig.6. Secondly, the temp/salinity misfits in vertical can be shown and mutual authentication with the SHARK results.

7) Based on the current results, it indicates the salinity looks no remarkable improvement. However, the salinity peak in Sep 2010 at 7 m can be reduced by assimilation even this model run has an underestimation before. This event is a nice case to explore which factor contributes that positive correction.

8) Fig8 shows the vertical impact for temp/saln. It is better to separate into two parts internal and out of Baltic sea.

9) The impact on SLA looks very small so I suggest replacing the related figure and table by a short paragraph.

10) Fig. 10 shows an improvement by assimilating SST. But the quantitatively comparison with the OSISAF concentration in the time series is helpful to know the impact in different sea ice seasons.

Other small issues:

1) Line 137, the operator of Li in Eq. 3 has no illustration.

2) Line 159, “OSISAF product” is it means more general products or only SST?

3) Line 229, “model layer” replaced by “model level” because the model is not a layered model.

4) Line 233, the forgotten factor is constant, or how to be defined?

5) Line 257, the evolution of SST based on 48-hourly local analysis. Does it mean all the SST comparison afterward use the 48 hourly forecast from the model?

6) Fig 1, the text is hard to identify. It is better to show the rivers involved in the model. The two stations of Arkona and BY15 can be shown in Fig. 1 (or Fig. 7).

7) Fig 6, the observed temperature at 70 m looks missing at Nov 2014, especially compared with other two depths or the salinity.

8) Line 289, the obvious improvement in the Gulf of Finland. However, based on the snapshot of the observed SST distribution in Fig. 2 there are no observations.

9) Line 278, “The model SST forecasts in both winter and summer (Fig2)”. It is not corrected to say SST forecast in Fig. 2 because they only show the analyzed fields and the related increments, which not supports this conclusion.

10) Line 311, “The temperatures differ by about 15-22C between summer and winter” is confused. Does it mean the seasonal variability in observation?

11) Line 314, “The reason perhaps . . .” this kind of illustrations in this study require some proofs like MLD diagnosing or others.

12) Section 5.1, which mean ssh fields are used for tide gauges and the model simulations?

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2018-8, 2018.