Ocean Science

Discussions

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

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This paper describes an experience in assimilating a high-resolution satellite sea surface temperature (SST) product into a NEMO-based numerical model of the Baltic Sea. There is no doubt that the Baltic Community urgently requires a good quality forecast of the Baltic Sea hydrography. And there is no doubt that any forecasting system would definitely benefit from assimilating observational information. These circumstances provide a strong motivation for the study discussed. I am, however, not sure that this data assimilation experience is sufficient enough to be documented in a peer-reviewed publication (at least with respect to improving forecast, as stated). In its present form,

I cannot recommend the manuscript for publication. Below I list my comments the authors might want to consider.

General comments

The Title does not correctly reflect the subject/results of the paper. My concern is the statement "Improves the Ocean Forecast": 1) because of the use of atmospheric reanalysis (not the forecast) as the forcing; and 2) since it is not clear from the text whether the authors evaluate the system state after the LSEIK analysis or in the forecast phase (just before the analysis). Same is to the statements in lines 20 − 21, 86, 278, 440. "...the Ocean Forecast in the Baltic Sea" sounds a bit odd.

There is a lack of detailed information on the data assimilation set up: whether the ensemble error statistics (or ensemble of model trajectories) dynamically evolve(s) in time or there is just one model trajectory and at the analysis step (every 48 hours) a constant (as it looks like given the expression "a stationary ensemble sample" in line 465, the suggestion on "a flow-dependent background error covariance" in line 472) covariance matrix represents the model error statistics... The SEIK and LSEIK are normally considered as ensemble-based data assimilation methods. It would be nice if the authors clearly emphasize what is different/distinct in their application and why they use (L)SEIK for the analysis while they do not use any ensemble at all. Why do the authors not use the flow-dependent background error covariance? Do the authors really "use a localized Singular Evolutive Interpolated Kalman (SEIK) filter" just only "to characterize correlation scales in the coastal regions"? Please describe the model variables used to construct the multivariate error covariance matrix and included in the state vector.

In the present form the conclusions include only general statements on the impact of SST DA, which does not, however, add anything new to what was drawn from previous studies, and there is nothing specific with respect to assimilating the OSISAF SST. More emphasize could be made on benefits due to the resolution of the OSISAF SST

product, then a comparison against similar experiments but assimilating satellite SST data with coarser resolution are required.

Specific comments

Lines 12-13: overall the sentence sounds misleading; moreover, for the localised SEIK you can use the LSEIK abbreviation. Missing reference to Nerger et al. (2006).

Line 20 (also line 453): I am just wondering whether 0.4% difference is a statistically significant in this particular application.

Line 119: please provide a reference to the used "runoff database".

Lines 205, 206: the discussed is the representation error (Janjić et al. 2017, https://doi.org/10.1002/qj.3130).

Part 4, Lines 224-225, 228: Please explicitly determine the state vector – which particular model variables it includes.

Lines 227-228: editing is required for the sentence "There does not exist uniform nature of error covariance for the variables of the model state vector and for the coastal zones . . ."

Line 233: "a forgetting factor" or "the so-called forgetting factor"

Liner 236: missing references to Janjić′ et al. 2011

Lines 247-248: The sentence "The correlation length scale . . .." is a copy-paste from Losa et al. 2012; please rephrase and provide the references, including the references to the original studies by reporting on the estimates of the Rossby radius of deformation (Alenius et al., 2003; Fennel et al., 1991).

Lines 271-276: the discussion on the bias seasonality: while, in general, the statement (l. 271) is true and was also discussed in Losa et al. 2014, it is difficult (if ever necessary) to conclude anything in this respect given just 2 snapshots for the increments

(Figure 2).

Line 457: "significantly improved" – this is not obvious.

Misprints/Typos

Line 14: should it be "improvements of" instead of "improvements on"?

Lines 33-35: please provide references;

Line 38: "a numerical model" instead of "a numeric model";

Line 46: "joint effort" instead of "joints effort";

Line 49: "used for the operational" instead of "used to the operational"?

Line 85: "sea level anomaly" instead of "sea level Anomaly";

Line 271: "model forecast possibility" – please remove "possibility";

Line 308, 333, 335: "Arkona" instead of "Arokna";

Line 329: "The possible reason" not "The possibility reason";

Line 470: "strongly", however the sentence in the lines 470-471 sounds misleading.

References

Janjić′, T., Nerger, L., Albertella, A., Schröter, J., Skachko, S., 2011. On domain localization in ensemble based Kalman filter algorithms. Monthly Weather Review 136 (7), 2046–2060.

Nerger, L., Danilov, S., Hiller, W., Schröter, J., 2006. Using sea level data to constrain a finite-element primitive-equation model with a local SEIK filter. Ocean Dynamics 56, 634–649.