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# *Interactive comment on* "A hybrid data assimilation method and its comparison with an Ensemble Optimal Interpolation scheme in conjunction with the numerical ocean model using altimetry data" *by* Konstantin Belyaev et al.

**Anonymous Referee #2**

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This paper presents a practical implementation of a Kalman Filter (KF) Data Assimilation algorithm variant. The method itself, called afterward in the paper as "GKF" for Generalysed Kalman Filter was previously presented by the same author in a previous paper (Belyaev, K. et al., 2018: An optimal data assimilation method and its application to the numerical simulation of the ocean dynamics). Compared to the most common implementations of the KF, an additional constraint on model and observation temporal tendencies is added when estimating the analysis correction (p.2, l.30).

The manuscript focus on the application of the GKF to a basin scale ocean state es-



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timation. It is intended to illustrate the performance of this “modified” KF compared to the more commonly used Ensemble Optimal Interpolation (EnOI). After a short review of the method basis, results of 1-month experiments assimilating along track SLA observations in an Atlantic Ocean configuration at 1/12° are analyzed. The analyzed fields are compared with the ones obtained from an EnOI approach and an experiment without any data assimilated. Observation misfits, as well as physical fields at a given date are taken as a measure of success for the implemented method.

The manuscript suffers from a poor level of English. It has to be reviewed by a fluent/native English speaker.

It also suffers from a lack of explanations and justifications on the meaning of the hypothesis made on the model and observations, their trend and error to derive the GKF from the KF. The validity of those a priori hypotheses has to be discussed in the context of daily basin scale ocean data assimilation, presented here. This could also help justifying why the GKF is best suited here in theory compared to the EnOI. The way the additional EnOI parameters (alpha and R) are chosen for a fair comparison with the GKF is not discussed and could largely affect the observation misfits.

I would recommend major revisions to improve the manuscript readability but also the justification for the use of the method for daily ocean state estimation and analysis of the results.

Title

I would recommend the use of “Generalized” in the title, instead of “Hybrid” not used again in the text to refer to the proposed scheme.

Abstract

- p.1, l.15: “The method is able . . .to produce analysis closer to observations”: closer compared to what? “It also conserves the model balance.” This property should be explained in the manuscript and not only mentioned here.





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- p.1, l.16: "…their errors": the dot at the end of the sentence is missing. The "confidence range of the analysis", mentioned here as an advantage, is neither shown nor discussed later.

The abstract has to be improved to better fit the manuscript content.

Introduction

- p.2, l.5: The constraint on the DA scheme "cost" you mention is mostly relevant in the context of real time production of ocean forecasts.

- p.2, l.10: Some implementations of the 4DVar seek to optimize not only the Initial Conditions but also the boundary conditions (mostly the atmospheric forcing fields).

- p.3, l.4: It the current work…

The assimilation method and the numerical algorithm of its realization

- p.3, l.29: I would suggest following the unified notation introduced by K. Ide et al, 1997 (https://doi.org/10.2151/jmsj1965.75.1B_181), widely used in the atmospheric and oceanographic DA community. The linear dynamical model is then noted M instead of $\Lambda$.

The way you define the observation and model trends has to be better explain and justify.

- P.4, l.10: From my understanding, an observation operator has be applied to x a,n to map the model state into the extended observation space?

Could you explain the use of the ocean model analysis to compute the so called observation trend, since you have access to the observation vector at the analysis step n? And the use of the expectation?

Computational experiments

- p.5, l.18: It is not mentioned in the text that you assimilate "along track" sea level anomaly and not maps, both types of products being produced by AVISO.

- p.5, l.26: “Below we compare the our GKF assimilation . . .”

- p.6, l.4: Do you mean: “An archive with 10 years of completely defined fields. . . ?” Do those fields are instantaneous or daily mean fields?

- p.6, l25: Could you explain what means Cn+1=0 and x ÌĚ n+1= xa n when applied to daily ocean state data assimilation and to which extend this approximation is valuable or not for the solved problem here? This can also help in understanding why the GKF approach is more appropriate to the type of problem solved here than the EnOI.

Results of the experiments and their analysis

- P.6, l28-29: Reformulate the 1st sentence in a better english. The values you compute are Sea Level Anomalies OR Sea Level “height” (SLA+MDT)? How do you compute the model counterpart to estimate the innovations: at the exact time of the observation from instantaneous model forecast field?

- p.7, figure 1: In the legend of figure 1, could you tell which variable is shown and give its unit?

- P.7, l.9: The term “moment” n is confusing: does it refer to the model time step the closer to the observations OR to the assimilation “step” (in day here)?

- P.7, l.10: “with the total amount of observations equaled to N”: the notation N was already used for the number of analysis time step; you cannot used it for the number of observations.

- P.7, l.12-13: SLAf and SLAa values used for the skill computation are instantaneous model counterpart of SLAo values at different model time steps between 0 to 24h when data are available OR do you compare the observations with daily mean model outputs? Does it differ from the way it is done within the assimilation process to compute SLA innovations?





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- P.7, Figures 2 and 3: You should add the variable name you show and its unit in the legend. The axis units are also missing.

- P.8, l.12: What is the temporal frequency of the SSH nudging?

- P.8, l.14-15: You should show the full experiment period on figures 2 and 3 to better illustrate/justify your assertion: "the major deviation between both of DA methods and control occurs near the day 27, after this day all curves become practically steady".

- P.8, l.18: For the SST, does the January 27, 2010 coincide with the day with a nudging?

- P.9-10, figure 4: The units in the legend are missing. The values on the isolines are difficult to read, what is the contour interval? A zoom (on the Gulf Stream for example) with the observation positions/values could help to see eddy position/shape differences for the three simulations, compared to the observation values.

- P.11, l.5-8: Could you tell if the SLA features you mention are realistic? Seen in the assimilated observations?

- P.11, l.15: The warm eddies only seen in the GKF analysis are also seen in the SST fields used for the nudging?

- P.12, figure 5: It seems that the colobars differ between the plots c and d. The contour labels in the Gulf Stream area restrict the visibility of the SST fields. A zoom could be useful to highlight smaller/regional differences. The SST difference between the GKF and the EnOI SST analysis (panel d) shows very few "round" shaped patterns with high values. Could you explain that? It looks like there were few very high isolated SLA innovations and you can "see" the signature of the prescribed correlation radius in the one of the analysis correction? Does the SSH field differences has the same kind of round shaped patterns at the same locations?

5 Comparison with independent data





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- P.13, l.9: It is unclear how you compute your diagnostic: the model average values are daily mean values of the model fields? Do you have also computed daily mean observations from the instantaneous measurements you downloaded (one measure each 15 min)?

- P.13, l.15: The color of the lines on the figure are green, red and blue, there is no “gray” line as stated in the text. P.14, figure 6: the legend do not mentioned the variables that are shown on the figure and the line corresponding to the different experiments.

- P.14, l.11: This section is very short. As its purpose is to show that the GKF SST analysis is able to capture synoptic variability compared to EnOI analysis, I would suggest moving this comparison to the OSTIA SST where the analysed SST fields are compared (figure 5). A zoom on eddies can be done as the basin map do not allow to see the mentioned eddies (p.15, l.3-4). How does OSTIA compare to the nudged SST fields: the NCEP/NCAR SST do not have such eddies at the same date?

6 Conclusions and outlook

The conclusion is very short and remains very general. It should contain more precise outcomes of the study.

The reference to the “Comparison of Data Assimilation Methods in Hydrodynamics Ocean Circulation Models” by Belyaev K. et al. just published in Mathematical Models and Computer Simulations in July 2019 could be added to the list of reference. I found the method presentation clearer in this previous paper.

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