Ocean Science Discussions

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Comment on os-2021-64

Anonymous Referee #1

Referee comment on "Assimilating realistically simulated wide-swath altimeter observations in a high-resolution shelf-seas forecasting system" by Robert R. King and Matthew J. Martin, Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-64-RC1, 2021

The paper untitled « Assimilating realistically simulated wide-swath altimeter observations in a high-resolution shelf-seas forecasting system" present a very interesting and innovative study using state of the art methods and models and clearly presented. Main objectif of the study is to prepare assimilation of future wide-swath altimetry observation from SWOT satellite and to quantify impact and expected improvement of this future mission. Authors address this problem in a realistic operational high resolution ocean forecasting system and using an OSSE protocole perfectly defined and justified in term of ocean processes represented in the model and adequation with the model resolution, observations assimilated in the system complementarity of the observation data set and state of the art data assimilation method fully validated and used in an operational context.

The paper is well written, perfectly understandable, and well presented, plan of the paper is logical and help the reader. The information in each section is at the appropriate level.

The authors present important results obtain in this realistic context that are useful for scientists involved in the SWOT mission, for the developer of data assimilation method applied to oceanography and for operational ocean forecasting centers.

I fully recommend publication of the paper if authors can take into account the following remarks and recommendations.

General comments and questions

- the authors don't provide any figure of innovations or increments, they only show bias and RMSE. That should be justified in the text if analysis increments don't provide more information that the bias or RMSE. But I think that analysis increments provide important information to fully understand how the data assimilation scheme work. I expect that spatial scale of increments should be different and could be illustrated on figure 7 for example. Increments could be also useful to illustrate the discussion in section 5.2.2 and/or 5.2.3 on the improvement/degradation of the solution depending of assimilated observations.
- One of the objectives of assimilating SWOT observations is to constrain small-scale structures in the ocean. This is not address in the paper (expect remark at the end of 5.3 without any illustration or explanation), the authors don't present any results illustrating the impact on meso scale structures in the different simulations or a spectral analysis presenting differences in term of energy between all the experiments. I understand that this is not the aim of the paper which is really focuses on the different sources of errors in the SWOT observations and especially the very important topic related to uncorrelated errors. I recommend adding at least a paragraph in the discussion section on the impact of SWOT on mesoscale structures and a perspective on this topic in the conclusion. Ideally, the authors will add a subsection in chapter 5 for example in section 5.1 about SSH.
- The experimental protocol is well described and fully justified, especially with regard to how SWOT errors are represented in the system and the impact of these errors when the data are assimilated. The authors should provide recommendations in the discussion or conclusion section about how SWOT data should be post processed for an optimized use in data assimilation scheme. How could correlated errors be removed or reduced? Is the HalfSWOT or the 5km and 20km filtering solution a recommendation or a haddock solution? Is it realistic to expect that only kaRIn error will remain?
- In Chapter 1 : Introduction. Authors could add a citation of recent publication Benkiran et al 2021 "Assessing the Impact of the Assimilation of SWOT Observations in a Global High-Resolution Analysis and Forecasting System Part 1: Methods"
- In section 3.1.1. the authors comment on an important point regarding the differences between nature run and free run, in the OSSE protocol it is important to understand these differences and how the data assimilation scheme will move the model on another trajectory. In this section it is not clear why there is systematic cold and fresh bias. Is there a mistake in the explanation "due to broadly similar irradiative fluxes between the atmospheric forcing datasets". Is there a systematic bias between the two atmospheric forcings used in the experiment for the wind? the heat fluxes? The paper doesn't address the question of whether this systematic bias between nature run and free run have an impact on the results? Could you expect different impact on the sea level analysis in a unbiased system? The authors don't provide an OSSE calibration, comparing SLA differences between nature run and free run and what could be obtain in a real case assimilating real data. This is recommended to understand if in the OSSE experiment the data assimilation scheme will work as in a real case. I suggest to provide on fig 2 an additional map showing the classical SLA increment obtained in the operational system.
- In section 4.2. It might be useful to provide a brief definition of each error and comment each figure 5 from a) to f). Could the authors provide more information on the following remark "The length-scale of these correlations can also be of the same order as the size of the domain". Is it something deduced from one of the figures? .
- One important difference between Control run, SWOT and halfSWOT run is the number of sla observations in the system during each data assimilation cycle. The authors don't provide any information on the number of observations assimilated during an assimilation cycle and the expected impact when the data assimilation scheme assimilate half the observations.
- In table 3, it is unclear how RMSE is computed. Is it computed in the observations space or in the model space? Only with the points where there are observations or for the full domain?

- In section 5.1, Authors noted considerable seasonal variation in the off-shelf SSH RMSE, but high frequency variability is even higher and not mentioned.
- Fig 8 : Is the error computed with the same point for different experiments? Is it computed for all the points of the model grid?
- Section 5.1.1. the authors refer to fig 5 to explain that the correlation could be longer than 20km, which is not obvious on fig 5 as no correlation is shown.

Comments on the form

- Figure 2 : add "bottom panel" in the legend.
- Figure 3 : limit of color bar could be change for on-shelf temperature and salinity to highlight more detail on the figure
- Table 2 : provide units
- Chapter 5. Section 5.1. Why don't the authors keep the same section structures with a separation between on and off-shelf for each variables? There is only one subsection in Section 5.1
- Fig 8, 13 : It would be good to keep the same color code for all the figures and experiments. LowerrSWOT in purple for all the figure for example and used other color than blue, yellow for HalfSWOT and HalfSWOT5km.
- Fig 11. HalfSWOT_5Km and SWOT experiments are reverse in the legend.