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Review of "An ensemble Kalman filter system with the Stony Brook Parallel Ocean Model v1.0" by Shun Ohishi et al.

Anonymous Referee #2

Referee comment on "An ensemble Kalman filter system with the Stony Brook Parallel Ocean Model v1.0" by Shun Ohishi et al., Geosci. Model Dev. Discuss.,
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The manuscript discusses a setup of an ocean circulation model (the Stony Brook Parallel Ocean Model, sbPOM) for the north-western Pacific region combined with an LETKF data assimilation step. Daily assimilation of satellite and in situ observations is applied and sensitivity experiments are performed with and without incremental analysis updates (IAU) in which the parameters of different covariance inflation methods (in particular RTPP and RTPS) are varied. In addition, a multiplicative inflation is tested with a single fixed inflation value. The study finds that IAU improves the balance of the model increments while the inflation schemes disturb the balance. In contrast IAU leads to higher estimation errors and less ensemble spread than the inflation methods. The multiplicative inflation is found to be failing by not reducing error enough. Parameter ranges are described in which the different methods yield the best assimilation results (low imbalance combined with low estimation errors) and the overall conclusion is that IAU in combination with RTPP with a parameter value of 0.8-0.9 provides the best configuration.

Overall, I have large problems to find what is actually new in this study and what are relevant research results. Actually, while the authors write 'This study develops an ... (EnKF)-based regional ocean data assimilation system' (Abstract line 12), this system is certainly not new. Actually, Miyazawa et al. (2012) already described an LETKF in combination with the sbPOM model. This earlier publication did not use the same model configuration, but this implies that an actual LETKF-sbPOM DA system already exists for 10 years and this leaves the impression that in the manuscript the authors (Y. Miyazawa is one of the co-authors) merely present some new model configuration. Even more, the applied methods IAU, RTPS and RTPP are established standard methods for ensemble data assimilation. Thus, it is unclear what new insight the experiments described in the manuscript actually provide. The given numbers like 'RTPP with the parameter of 0.8-0.9' (Abstract line 26) are not at all generalizable to other model configurations or other models. Further, the authors do not show any attempt to actually find explanations for their findings. As such it remains that they describe the behavior of a single data assimilation application when parameters of established standard methods are varied. For me, this is insufficient for a scientific publication. To this end, I can only recommend to reject the manuscript. Perhaps, the authors can then find a proper scientific question to

assess with this ocean DA system and submit a new study that provides general insights.

Apart from the aspect of novelty and relevance, I have a few major comments:

1. The manuscript is submitted as a 'development and technical paper' and its title suggests that it might document particularities of the EnKF-sbPOM model system.

However, the manuscript is missing detailed descriptions of the actual system.

2. The authors list EnKF-based ocean data assimilation systems in Table 1. Unfortunately, this list is very incomplete. E.g. there are EnKF-based systems run operationally by the Copernicus Marine Service (CMEMS) for the global ocean and for the Baltic Sea (It is easy to find these systems via the CMEMS website marine.copernicus.eu). From the operational CMEMS systems, Table 1 only lists the TOPAZ4 system. There is also an operational EnKF-based system in Germany (the latest article about it is Bruening et al, 2021, but there are several publications about earlier versions dating back to the year 2012. This system uses 12-hourly analysis, thus even shorter than what is pointed out in the manuscript). Also there is an EnKF-based coupled system which focuses on the ocean (e.g. Tang et al. 2020). Overall the authors should perform a much more careful research on current systems. Publications dating back to 2011 or 2012 do most likely not describe the current status.

3. The authors express that their data assimilation setup is particular because of daily assimilation. However, when one has a sufficiently complete overview one sees that short assimilation cycles like daily are not that special. On the other hand there are good reasons for longer cycles. One particular reason is the repeat cycle of the altimetry satellite data. Further, while applying e.g. weekly analyses steps, systems like TOPAZ4 use asynchronous filtering, e.g. for SST. Thus, the system is able to also take some of the faster variability into account. The authors should take such characteristics of the DA systems into account to provide a sound overview of EnKF-based ocean DA systems.

4. As mentioned above, IAU, RTPP and RTPS are standard methods in DA already for quite some years. As such it is surprising to still see a manuscript submission about these schemes. Unfortunately, the authors also miss to take into account the study by Yan et al. (2014), which discusses IAU in ocean data assimilation. However, also the CMEMS system for the global ocean uses IAU. Given that these methods are well established and well studied, I am quite skeptical that it is possible to find new general insights by just using standard methods and varying their parameters.

5. The authors use a model spin-up of 4.5 years from an ocean in rest. This spin-up period looks far too short for properly spinning up the ocean unless one only looks at the upper layers.

6. The observation errors of 1.5degC for satellite SST and in situ temperature and of 0.2m for SSH are very large compared to what is commonly used today.

7. In lines 220-221 it is described that the localization settings are chosen following the studies by Miyazawa et al. (2021) and Penny et al., (2013). However, in these studies other model configurations with different resolutions are used and both use different localization radii. It is known that localization settings depend also on the model configuration. To this end, just selecting some settings from model configurations at other resolutions is not a reasonable approach. One can use values from other studies as a starting point for one's own tuning, but this tuning will be required as otherwise, there is a high risk that the DA system is suboptimal. Thus sub-optimality then also influences other DA parameters like those for the inflation.

8. In line 60 the authors describe the TOPAZ4 system with 'but with inflation of observation errors'. I'm unsure what the authors intend to express by 'but'. However, when the authors look carefully, the 'moderation of observation errors' used in TOPAZ4 is in fact a careful inflation that should have similar effect as a carefully tuning multiplicative inflation scheme.

9. The multiplicative inflation scheme is described as 'not demonstrate sufficient skill'. This description is actually misleading and invalid. The authors only run a single experiment with a fixed inflation of 5%. Thus, any sensitivity assessment is missing. Actually, the data assimilation process in the system of the manuscript runs already stable with successful assimilation even without inflation as the figures show. This is a clear indication that 5% multiplicative inflation is too large.

10. The residual of the nonlinear balance equation $\{\Delta\}$ NBE (Eq. 8) is not normalized. As such it is unclear whether any of the values shown in Fig. 1 and described in the text (like 2.11×10^{-10} for MULT+IAU in line 249) is actually significant.

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