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Ocean Science Discussions

Interactive comment on "An eddy resolving tidal-driven model of the South China Sea assimilating along-track SLA data using the EnOI" *by* J. Xie et al.

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Responses to referees' comments

We agree with the central concern raised by the reviewer, and revise the relevant sections with cautions. Again, we thank the referees for the comments.

Major changes in the revised manuscript:

1)According to the referees' suggestions, two additional assimilation runs are supplied as NoFGAT and NoTide.

2)All the temperature profile were use for validating the multivariate impact (as recom-

mended by the reviewer)

3)Small changes were applied in the text in order to address the reviewers comments

The detailed responses to each comment follow:

Referee #1:

1.Page 878: do you use a relaxation towards SST?

RESPONSE: No, just the sea surface salinity is relaxed towards the climatology in the model.

2.Page 879: replace steric height anomaly by sea level anomaly (steric height anomaly is only the part related to temperature and salinity variations).

RESPONSE: Corrected, thanks for that.

3.Page 879: please discuss the impact of using an 11 year averaged of the model simulation. The MDT usually has a large impact on a data assimilation system. In addition, the 11 year mean is not consistent with the mean used for altimeter SLA computation.

RESPONSE: We agree that the choice of the MDT has an impact on the accuracy of the result but it is not obvious what is the best solution- a MDT based on: model, observation or combined. There are often discrepancies between model based MDT and observed one (like here in the Makassar Strait), which most often result from model bias. It might be dangerous to use a different MDT than the model long time average (its equilibrium) because it pushes the model towards a new equilibrium that it cannot necessarily sustain. Here, we do not intend to correct for the bias: observations use an average based on observation (that includes also the observation bias), the model do the same and the anomalies are assimilated. The model and observation MDT are estimated from a 12-year long estimate, but the period is not matching. Ideally, one should use a similar period to limit the impact from interannual variability. However, we couldn't afford computationally to run the model until 2009, and the longest period available was used (12 years).

4.Page 880: You should discuss the tidal model used to correct altimeter data. This part actually would deserve a much better discussion. One would assume that your tidal corrections in the SCS are better that the ones used to correct altimeter data (this needs first to be assessed and discussed). If this proves to be true, one would thus have expected to use this model to correct the altimeter data.

RESPONSE: This might be the case, but we are not able to prove it as we depend on the output from the data provider. The only SLA product available is the one that is geophysically corrected for tides, inverse barometer, tropospheric, and ionospheric signals altogether, but the tidal signal alone is not provided. Therefore, we cannot satisfy your request. In addition, modern tidal models are using finite element grids, which are a more adequate choice for simulating tides than the finite differences in HYCOM.

5.Page 883: Figure 5... This should be better phrased. I cannot figure out what is computed here (what is a running seasonal ensemble from CLS maps?).

RESPONSE: Thank you. The paragraph is now rephrased. "Figure 5 shows examples of standard deviation of SLA obtained in such "running" seasonal ensemble at four different times of the year. In order to ensure that the model variability is realistic, the model-based variability of SLA is compared to the one composed from SLA composite maps produced by CLS (with a similar sampling strategy). As in the observation, the seasonal variability in the model is ..."

6.Page 886: Correlated errors. The (space) error correlation scale (100km) is close to the mesoscale signal scale. Please discuss how this impacts the data assimilation.

RESPONSE: The correlation of errors should in principle act as a filter for scales smaller than 100 km at observation points. We would therefore expect that uncorre-

lated errors lead to artificial sub-mesoscale features in the vicinity of the satellite tracks. Uncorrelated errors on the other hand offer several numerical advantages. These issues would deserve a long discussion but would fall out of the scope of the present paper.

7.Page 886: see also remark 4. Why not computing directly a new tidal solution from your model run?

Response: Running HYCOM as a purely tidal model is practically cumbersome, we do not know of anyone who has attempted this.

8.Page 887: "composing our ensemble from a simulation run with tides...". This is indeed what you should have done. Why is this technically difficult? Please comments. This is a clear limitation of the method. You do not take into account the coupling between tides and circulation (which is actually one of the objectives of the paper).

RESPONSE: This is now addressed in the paper with the following paragraph. "Tidal signal is highly variable with time because of the influence of the sun and the moon. The intensity of the diurnal and semi diurnal oscillations varies strongly with the season and the phase of the moon. The ensemble used for computing our covariance is currently composed from model output taken at similar season, but if the tidal signal is considered, one should also ensure that model outputs are corresponding to a similar moon cycle. This makes the sampling strategy much more complicated, and increases the storing requirement - daily model output should be stored instead of currently every 10 days. Therefore, we decide to correct only the mesoscale dynamics and assume that the influence from tides will adjust quickly."

9.Page 888: What are the RMS differences with respect to assimilated data?

RESPONSE: We assume that you refer to the last sentence of page 888: "Their difference is also shown in the ..." It means that the difference is RME-control – RMSE-assim. The sentence is replaced by the following: "Their difference (Ctrl minus Assim) is also shown in the ..." We also corrected a typo in page 887: "-a quantitative routine check against the not-yet assimilated SLA"

10.Comparison to temperature data. You should (must!) include and discuss other (deeper) depths. Please give also the RMS of the observations (with respect to climatology) (are you better than the climatology?).

RESPONSE: Thank you for the suggestion, this is now done. The results are presented both in a table and in a new Figure. It shows that assimilation improve the stratification on average, but not everywhere. Stratification is improved at intermediate depth, but degraded at the surface and at the bottom. Furthermore, comparison to climatology is also added, and it presents RMSE that is lower than in all model runs. This bias is likely to be inherited from the model spinup. This Section is now re-written to present these new results.

Referee #2:

There are two aspects of this paper that make it particularly interesting, and worthy of publish. Firstly, this is an example of a model that explicitly resolves tides, but attempts to only constrain the sub-tidal variability with data assimilation. This approach has been widely discussed in the operational community, but seldom attempted. This paper is the first that I know of. Secondly, the implementation of EnOI uses a modified asynchronous approach, where observations are taken from a long observation window (7 days) and the model is interpolated to the observations at the observation time (so-called FGAT). However, instead of using time-lags in the ensemble, the observations are penalised with an "age error"(the authors describe this as a "new formulation" (P877, L9)- I agree, so they should evaluate it more completely). Both of these factors make this paper relevant and interesting. However, the paper needs two additional experiments before publication. It needs a data assimilating run without explicit tides, so the impact of the explicit tides can be demonstrated and the impact of the method of removing tides from the model can be assessed. The paper also needs a data as-

similating run without the asynchronous implementation, to evaluate the impact of this original implementation. I recommend that the authors perform and describe these runs before publication.

RESPONSE: We have supplied two assimilation runs NoFGAT and NoTide as recommended. The comparison is presented in the new Section 6.

1.Page 875, L25ïijŽThe extensive references to papers authored by Wang, Wu, and Xiao, as examples of papers that assimilate altimetry seems out of place, and is clearly an example of self-promotion. There are many examples of studies that assimilate altimetry in the literature.

RESPONSE: Indeed, there is abundant literature about assimilating altimetry data in global ocean and we do quote both historical references (Cooper and Haines 96, Oke et al 2002) and currently running operational systems (Cummings et al. 2009). However, the studies referred here are the only ones (to the best of our knowledge) that deal with assimilation of altimetry data in the South China Sea. They are therefore necessary for the positioning of the paper and are not a self-promotion (they are not arising from our institutes nor any common projects). Therefore we would like to maintain the references.

2.Page 876, L10: The authors correctly point out that "it is not realistic to run tidal model and eddy resolving model separately because the two can interact". I think the authors are referring top the fact that when tides are included, the sub-tidal variability is impacted, through enhanced mixing, transports etc. However, despite this, the authors use a non-tide resolving model to construct their ensemble (P886, L15). They acknowledge this in the paper (P887, L9), but is represents an in consistency that could be addressed to improve the paper.

RESPONSE: The inconsistency is indeed acknowledged and its reason is purely practical. The article now also suggests a practical solution for those who afford the storage of numerous output files on disk. "Tidal signal is highly variable with time because of the influence of the sun and the moon. The intensity of the diurnal and semi diurnal oscillations varies strongly with the season and the phase of the moon and so does its interaction with the mesoscale dynamic. The ensemble used for computing our covariance is currently composed from model output taken at similar season, but if the interaction with the tidal signal is also considered, one should also ensure that the ensemble is composed from model outputs taken at similar moon cycle. This makes the sampling strategy more complicated, and increases the storing requirement - daily model output should be stored instead of currently every 10 days. Therefore, we decide to correct only the mesoscale dynamics and assume that the influence from tides will adjust quickly."

3.Page 879, L14: Why restrict the mean SSH to 11-years, when a 26-year run is available?

RESPONSE: This was the longest non-tidal model run available. A 26-years run is available only for the coarse resolution outer model. The inner model run starts from January 1978. The inner model is initialized from an interpolated version of the outer model and spin up for 2-years. The mean SSH of the inner model is thus average from 1980 to 1991, which makes a 12-years period (11-years in the text is be corrected by 12-years). The text has been clarified.

4.Page 882, L3: Data assimilation is really only "optimal", when error statistics are well known. I would delete this unnecessary claim.

RESPONSE: It is removed.

5.Page 882: L13:What is mean by "multi-Gaussian"? Do you mean that the errors of the state variables are Gaussian? If so, why not just say that.

RESPONSE: For simplicity the term has been replaced by "Gaussian". Multi-Gaussian is in principle a more correct terminology for the multivariate state variables and corresponds to a more stringent assumption. See Bertino et al. (Int. Stat. Rev. 2003) for a

detailed explanation.

Interactive comment on Ocean Sci. Discuss., 8, 873, 2011.