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Ocean Science Discussions



# *Interactive comment on* "TOPAZ4: an ocean-sea ice data assimilation system for the North Atlantic and Arctic" *by* P. Sakov et al.

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Response to interactive comment C456 by A. Srinivasan (Referee # 1) to manuscript OS-2012-33 "TOPAZ4: an ocean-sea ice data assimilation system for the North Atlantic and Arctic" by P. Sakov et al.

We thank the reviewer for a constructive review. Following are our responses to the issues and questions raised in the review.

1) The system is based on a 100-member ensemble. It will be useful to know how/why this number was chosen. It is nice to have state dependent covariance - is there is a way to decide on the number of members in an adaptive fashion depending on the state?

REPLY: There is no simple answer to this question. For a large-scale systems it is impossible to match the number of growing modes by the ensemble size. The common approach is to use the maximal ensemble size possible, and then to tune the localisation and inflation to the best performance. Larger ensembles require weaker localisation and therefore are able to obtain more dynamically balanced updates. Geir Evensen believes that 100 is a good ensemble size for an oceanographic data assimilation system. We also view this size as sufficient for avoiding the vertical localisation, which would substantially complicate the design of the system.

2) In page 1522, ln 11 the statement "the correlation field is strongly anisotropic, with positive correlations in the ice covered areas corresponding to the fresher melted ice and negative correlations in the ice-free areas where warm and saline water melts the ice" could use rewriting to make it clearer.

REPLY: We rewrote this sentence as follows: "The correlation field is strongly anisotropic. It is positive in the ice covered areas, corresponding to the freshening of the water as the ice melts; but it is negative in the ice-free areas, where the state of the the ice is driven by the advection of warm and saline Atlantic water."

3) In page 1523, ln 13 the authors briefly mention the tuning of model parameters for viscosity and diffusion. The values chosen for momentum, thickness and TS diffusion velocities for the grid scale dependent parametrization will be useful information here. Is the diffusion velocity constant or spatially varying? Such information is always of interest to modelers.

REPLY: We added the following text: "The model uses biharmonic viscosity (0.2 kg/m.s), biharmonic velocity diffusion (0.06 m/s), and spatially varying layer thickness diffusion (of 0.06 m/s in the Gulf Stream region and smooth transit to 0.01 elsewhere)."

4) Page 1523, ln 17 in the paragraph where details on HYCOM are presented, it will be good to complete the description with additional info. I presume 2nd order scheme was used for momentum as 4th order is really only beneficial where the resolution is

greater than the Rossby radius? Also the reasons for choosing GISS mixed layer model chosen over the other available models such as KPP and the actual Jerlov water type could be provided.

REPLY: It is correct that the 2nd order scheme is used for this set up, but we did not feel necessary to describe this as it is a standard setting in HYCOM. We have chosen the GISS mixed layer depth model as it provides a better match with MLD (de Boyer Montégut et al. 2005). In particular, it reduces very large values simulated in the Subpolar Gyre and South of Svalbard. The Jerlov formulation had no strong impact but was included for consistency with the ecosystem version of TOPAZ system.

de Boyer Montégut, C., G. Mardec, A. S. Fischer, A. Lazar, and D. Iudicone (2004), Mixed layer depth over the global ocean: An examination of profile data and a profile based climatology, J. Geophys. Res., 109, C12003, doi:10.1029/2004JC002378.

5) Page 1524: The model uses Sigma0 vertical coordinate for target densities. Although, the domain does not include Antarctic Bottom Water, sigma2 vertical coordinate was shown to be more accurate for pressure gradient calculations over the entire water column by Chassignet et al 2003 (JPO). Can the authors please comment on their choice of sigma0? Also the actual model domain bounding latitude at the southern boundary will be useful it the equator?

REPLY: Better water masses representation at depth can be achieved using Sigma-2, but no single reference state is appropriate for the global ocean (Hallberg - Ocean Modelling, 2005). An addhoc approach is to use 3 reference states (Atlantic, Arctic/Antarctic and Mediterranean). The impact at transition is still unclear, and we find it worrying having a transition in our primary area of interest. Therefore, we decided to maintain Sigma-0 reference level - this has been well tested - for this version of TOPAZ, and run parallel test to implement sigma-2* in the next version of TOPAZ.

The model domain was extended further south in order to avoid interaction of the equatorial current with the model boundary.

6) The model bathymetry was interpolated from GEBCO 1 minute. Was it smoothed, were any other modifications done? Why was GEBCO chosen among the available products?

REPLY: GEBCO is the best product available to us at the time the model was developed. It was interpolated linearly to our model grid but was not smooth out. The bathymetry was corrected for an obvious bug in the Nordic Sea. The bug has been reported to GEBCO, and ETOPO5 was used at that place.

7) Page 1524, ln 17: in the description of the model relaxation at the lateral boundaries, additional information on the width of the relaxation zone and the relaxation time scale should be provided.

REPLY: We added: "with the relaxation zone width of 20 grid cells and an e-folding time of 30\,day$\cdot$MLD/15\,m".

8) Page 1525. It appears that Sea level pressure, dew point temperature are used to calculate water vapor mixing ratio. If so, this could be added similar to the description of shortwave radiation from cloud cover and stress from 10 m winds.

REPLY: We replaced the sentence at lines 3-6 page 1525 by the following: "The thermodynamic fluxes are computed as in Drange and Simonsen (1994), but the cloud cover fields are updated every 3 hours in the computation of the shortwave radiation to better represent the diurnal cycle. The momentum flux is computed as in Large and Pond (1981)."

9) Page 1525, ln 10: How is river runoff treated as a mass source? This is of interest since the baroclinic bottom pressure is time independent in hycom formulation. Is the procedure similar to one given in Schiller and Kourafalou (2009)?

REPLY: This was a mistake, only the evaporation and precipitation are treated as mass exchanges. This statement has been removed.

10) Instead of relaxation to surface salinity for handling inaccuracies in freshwater,

evaporation and precipitation balances why not consider a methodology like spectral nudging (Thompson et al., 2006, Ocean Modeling). Perhaps this can be implemented in the existing system by assimilating the innovations provided by (climatology - smoothed model forecasts) with appropriately smoothed ensemble members to spread the corrections.

REPLY: Thank you for this suggestion. We agree that this method is interesting and may allow model to constrain drift/biases efficiently. However, we also see some disadvantages of this method. It may not work well when the interanual variability is not small, such as in the Arctic.

11) Page 1526 ln 5: the description on DenKF can be made a bit easier for the reader? As it stands, one needs to know the details of ESRF, ETKF etc. to understand the explanation given.

REPLY: It is unfortunate, but we believe that it is not possible to expand the paper to include the description of the DEnKF. Should we done so, we would also need to describe other equally important components of the TOPAZ system, such as the localisation scheme and asynchronous data assimilation; as well as the important algorithmic details. Concerning the DEnKF, we direct the reviewer to Sakov and Oke (2008b) that provides a simple and comprehensive description of the scheme.

12) Page 1526 ln 16 mentions that the localization is done by multiplying the local ensemble anomalies by an isotropic function. The use of uniform in space localization radius does not seem to take into full advantage the non stationary and anisotropic nature of the covariance obtained from the ensemble. Can the authors please comment on this aspect?

REPLY: The localisation function is in most cases wider than the characteristic horizontal de-correlation distance of the model. (Except, perhaps, equatorial reqions.) Therefore, the essential (non stationary, anisotropic) covariances between state elements are mainly preserved. If this was not the case, then the analysis would be strongly unbalanced, and we would be able to see strong transients after the restart (and, perhaps, introduce an initilisation procedure).

13) In page 1527, ln 2: some post processing steps are mentioned where instances of negative layer thickness etc. are set to zero. There are approaches put forth to handle inequality constraints such as for layer thickness by Lauvernet et al 2009 and Thacker 2006 (Ocean modeling). Can the authors please comment on the utility of these methods? Is the extra effort to implement such schemes worth it for operational purposes?

REPLY: The methods proposed by Thacker and Lauvernet, although mathematically correct, have not been used for the following reasons. The method by Thacker acts in two steps, the first to detect the negative values, the second to recalculate the optimum under constraints of zero thickness. It therefore requires a second analysis step or even more, since the second step may yield a negative thickness in a different layer (the latter case has been experienced by Jiang Zhu at IAP), the computational performances would seriously be hampered. The method by Lauvernet requires the application of a Gibbs sampler to sample a truncated multi-Gaussian distribution, which is also very costly when applied in high dimensions. The manuscript has been left unchanged.

14) In page 1528, ln 18, it is stated that the perturbation of the model states is done indirectly through the forcing fields to ensure dynamical consistency. In the ocean however most of the variability is due to internal instabilities which are not directly dependent on the forcing. Since this is not an explicitly eddy resolving model it may not be an issue but in general will this approach of perturbing forcing fields alone be sufficient?

REPLY: We agree that the insufficient model resolution does have important implications for the model dynamics and, as a consequence, for the model error. Unfortunately, it is not clear how the resolution induced model error can be estimated in practice. We believe that tuning the ensemble spread by perturbation of forcing is a far better method than adding random or semi-random perturbations.

15) Page 1529, footnote 3: It is stated that the purpose of super observations is to reduce the number of observations. To this it might also be added that superobing prevents amplification of differences between individual observations that fall within a model cell

REPLY: We are not familiar with this problem. Theoretically, assimilating a superobservation and corresponding inidividual observations at the same location yield the same increments.

16) The SST data for assimilation is from OSTIA. There are several Level 4 foundation temperature products. Details on why OSTIA was chosen amongst the several available ones will be of interest. How reliable are the error estimates on the analysis from the data provider? Have the authors looked into the GMPE product from the met office? Comments on this will be useful.

REPLY: We started to use OSTIA because it was supposed to be superior product than the 1-degree Reynolds SST product. We have not tested other products at that moment. Interestingly, we do not see any improvement (and perhaps, some deterioration) after switching from 1-degree Reynolds SST to OSTIA in 1995 during the main reanalysis (1991-2010). We have not formally estimated the error estimates from the provider and do effectively tune them to the performance of the system. It is possible that these estimates are on the low side. In future we plan to move to assimilating L2P rather than L4 SST products.

17) Asynchronous assimilation sounds like First Guess at Appropriate Time (FGAT) common in NWP. Is this correct or is there more to it?

REPLY: There is more to it. Our understanding is that FGAT means using forecast at the time of observation to calculate innovation. The innovation can then be used for assimilation at a later time. The resulting increment is still different from that if the observation was assimilated at the time of observation because of the difference in state error covariance at different times. In contrast, the asynchronous EnKF does implicitly account for the evolution of the state error covariance in time. In the linear case the asynchronous EnKF is fully equivalent to 4D-Var (with some conditions attached, e.g. the same initial state error covariance, sufficient rank of the EnKF ensemble etc.).

18) How much spread is there in the ensemble for assimilating in-situ observations at depth? Are profiles extended all the way to the bottom? If so how is this done? How are multivariate corrections handled with respect to the generalized vertical coordinate system. Details will be useful.

REPLY: The ensemle collapse at depth is of a potential concern; but luckily we do not observe it in TOPAZ. For example, the ensemble spread at the depth of 300-400 m shows no negative trend and is mainly confined to the range of 0.2-0.4 C. We speculate that the model dynamics and, perhapse, the 1-percent inflation are sufficient to counter the reduction of the ensemble spread at depth caused by spurios vertical correlations.

19) More information on the procedure for Lagrangian assimilation of the sea ice drift will also be useful.

REPLY: An observation can be assimilated with the EnKF as soon as the corresponding ensemble of forecast observations is known. It is rather straightforward to calculate the vectors of estimated 3-day ice drift for each ensemble member...

20) The bias estimation procedure provides a Mean SSH that is more similar to MDT from CNES. Why not scale the CNES MDT to HYCOM Mean SSH range and use it in the assimilation?

REPLY: It is true that if the MSSH bias estimate was exactly equal to the difference between the model MSSH and the observed MDT, then one could directly use the observed MDT for calculating model SLA for data assimilation to the same effect. Use of the bias estimation is not an obvious decision; but it does have some benefits, such



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as the possibility to detect the change in MDT over time and shielding from possible errors in CNES MDT.

MDT from CNES is almost certainly more accurate than a model estimate in places where sufficient observations. However, it is much more uncertain in locations closer to the ice edge or in coastal areas. For example, CNES 2009 MDT suggests that the mean circulation along the East Greenland shelf goes inside the coast. This concern was reported to the CLS team, and collaboration during Myocean2 may lead to an updated MDT for the Arctic region that may benefit from our bias estimation. As we need a continuous estimate, we have decided to stick to the model Mean Sea surface height.

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Interactive comment on Ocean Sci. Discuss., 9, 1519, 2012.