

Interactive
Comment

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

A. Srinivasan (Referee)

asrinivasan@rsmas.miami.edu

Received and published: 5 June 2012

General Comments:

This paper by Sakov et. al., presents a detailed description and evaluation of a coupled ocean \-sea ice data assimilation system for North Atlantic and the Arctic. The paper is well presented and details will be of interest to researchers setting up ocean data assimilation systems, and for downstream users of the system products.

Specific questions, comments and suggestions:

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



state?

2) In page 1522, In 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.

3) In page 1523, In 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.

4) Page 1523, In 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.

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?

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?

7) Page 1524, In 17: in the description of the model relaxation at the lateral boundaries,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



additional information on the width of the relaxation zone and the relaxation time scale should be provided.

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.

9) Page 1525, In 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)?

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.

11) Page 1526 In 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.

12) Page 1526 In 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?

13) In page 1527, In 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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



these methods? Is the extra effort to implement such schemes worth it for operational purposes?

14) In page 1528, In 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?

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

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.

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

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.

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

20) The bias estimation procedure provides a Mean SSH that is more similar to MDT

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



from CNES. Why not scale the CNES MDT to HYCOM Mean SSH range and use it in the assimilation?

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

OSD

9, C456–C460, 2012

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C460

