Ocean Sci. Discuss., 11, C1163–C1173, 2014
www.ocean-sci-discuss.net/11/C1163/2014/

Ocean Science
Discussions

OSD

11, C1163–C1173, 2014

Interactive
Comment

Interactive comment on "Argo data assimilation into HYCOM with an EnOI method in the Atlantic Ocean" *by* D. Mignac et al.

D. Mignac et al.

davi.mignac@gmail.com

Received and published: 17 December 2014

General comments:

The authors would like to thank Reviewer 1 for the comments and suggestions that will certainly help improving the overall quality of the paper.

1) The technique of Ensemble Kalman Filter is adequately described, and the results from assimilation using this method using two different datasets (NCODA and ARGO) are clearly summarised.

We would like to mention that results from the HYCOM+NCODA system were not assimilated in the experiments. Only Argo T/S data were assimilated, and the outputs of the HYCOM+NCODA system were used as an independent source for comparison of

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

the subsurface velocities in a section at 25oW (Fig. 13). In the submitted manuscript (P1749, L3) we mentioned:

“Outputs from the HYCOM-Navy Coupled Ocean Data Assimilation (HYCOM+NCODA) (Chassignet et al., 2007; Chassignet et al., 2009) system available in z-levels and fields from the Ocean Surface Current Analyses – Real Time (OSCAR) (Johnson et al., 2007) were employed to compare the velocity fields produced by the assimilation runs.”

We have decided to use the HYCOM+NCODA analysis to compare the subsurface velocities with our assimilation experiments because this system has a mature multivariate assimilation scheme and it also uses HYCOM. In the surface, OSCAR data were also used to investigate the impact of Argo data assimilation on the velocity field by the calculation of the RMSDs for the u and v components.

2) The result that the assimilation did not significantly improve the circulation in the model is a notable negative result, and in my opinion needs more discussion.

We agree with the Reviewer that the Argo data assimilation did not significantly improve the circulation in general. We discussed in the paper - based on previous references on Argo data assimilation with HYCOM (e.g. Xie and Zhu, 2010) - that the model bias could be the main cause for the lack of substantial improvement of the velocity field by assimilation. In the present work, the ensemble members that come from a free model run had large biases with respect to climatology and observations. These biases were completely ignored in the assimilation scheme. Also, in certain regions such as the Gulf Stream and the Brazil-Malvinas Confluence the model variability and therefore the co-variances among the model variables were not so accurate. In order to overcome these limitations, new ensemble members were recently produced by long data assimilation runs. These analyses replaced the free run ensemble members and contained substantially smaller biases and more accurate co-variances.

We have slightly reorganized the text (P1759, L18) to better explain the limitations of our free run ensemble members in improving the large-scale circulation. We have split

OSD
11, C1163–C1173, 2014

the content in two paragraphs and in the end of the second paragraph, we have added a phrase as below.

“In the present work, the discrepancies of the model free run and its variability with respect to climatology and observations are an important limitation for the assimilation to produce the correct analysis increments, particularly for the large-scale circulation. For this reason, many works point out that the model biases should be considered during the assimilation process (Reynolds et al., 1996; Dee and Silva, 1998; Bell et al., 2004; Dee, 2005; Xie and Zhu, 2010). Also, improving the co-variances of the ensemble members may lead to more accurate analyses (Oke et al., 2008; Xie and Zhu, 2010).”

3) Indeed, the assimilation seems to make the equatorial circulation less realistic than in the control, both with NCODA and ARGO data. The authors should add a sentence or two of discussion of why this might be the case, and preferably relating it to changes (or lack of change) in the density structure close to the Equator. It may be that the model has insufficient resolution to simulate this circulation, or perhaps the equatorial flow features are more directly controlled by the wind stress and basic fluid continuity than by the geostrophic balance that holds over most of the ocean interior, so are less sensitive to adjustments in the temperature and salinity fields.

We respectfully disagree with the Reviewer regarding the negative impact of the equatorial circulation produced by Argo data assimilation. We understand that the assimilation impact in the equatorial currents was very small and difficult to characterize. The SEC, EUC and NECC in the assimilation runs are similar to the control run despite the differences they all show with respect to the HYCOM+NCODA analysis, which contains stronger NECC for instance. Unfortunately, there are not in situ measurements of currents in the equatorial Atlantic from 2010 until 2012 publicly available to better identify the quality of the results. As the Reviewer mentioned, close to the equator the geostrophic balance looses importance and the density distribution will adjust to the currents and not the other way around. The small changes in the equatorial current system could also be attributed to remote impacts from the assimilation in extra-equatorial regions. In order to better discuss this point, the text on P1759, L18 was completely rewritten as below.

“XZ could observe modifications in the equatorial Pacific current system due to Argo data assimilation, but they were not always positive when compared to the Tropical Atmosphere Ocean (TAO) array data. For instance, a too deep undercurrent maximum in the east and a too thick and strong westward current in the west were produced. In the present work, a few modifications were observed in the equatorial Atlantic region as well, but with smaller intensity than in the mid-latitudes. It should be noted that the geostrophic balance that holds in most of the ocean interior looses importance close to the equator. Therefore, the equatorial current system should be less sensitive to analysis increments by Argo data assimilation, so that changes in this region may be mostly attributed to remote impacts from the assimilation in extra-equatorial regions.”

Specific comments:

2. HYCOM and its configuration

P1738.L26: Why stop at 50oN? The phrase “. . .almost all the Atlantic Ocean. . .” doesn’t seem to be justified here! I think the authors should explain why they have excluded the climatically important subpolar region from this study.

A justification should also be included in this section for the decision to use surface pressure here as the reference for potential density. A pressure of 2,000 dbar (sigma-2) is the de facto standard these days in isopycnic models, for the chief reasons that it distinguishes AABW and NADW, as well as giving smaller pressure gradient errors in the ocean interior.

The decision to cut the grid at 50oN considered the fact that the surface currents are primarily zonal in this latitude (Gabioux et al., Brazilian Journal of Geophysics, 31, 229-242, 2013). Also, as we mentioned in the last part of the introduction (P1737, L24),

OSD

11, C1163–C1173, 2014

this grid was conceived and configured to provide reasonable boundary conditions to higher resolution grids that are under development over the South Atlantic (from 35.5oS to 7oN, west of 20oW until Brazilian coast). The Atlantic grid represented the first effort of our group to develop a nested system with data assimilation.

The reason for initially using surface pressure as reference for potential density is that we wanted to focus on the improved representation of near surface fields (Chassignet et al., Journal of Physical Oceanography, 33, 2504-2526, 2003). This configuration was recently revisited and a new grid with higher horizontal and vertical resolution has been proposed in which sigma-2 was adopted as reference for potential density.

The original text starting on P1738, L25 has been improved to include these justifications as below.

"The computational model domain covered almost all the Atlantic Ocean from 78oS to 50oN and from 100oW to 20oE, excluding the Pacific Ocean, the Mediterranean Sea and the North Atlantic subpolar region. The choice for the northern limit at 50oN was based on two facts. First, the surface currents are primarily zonal at this latitude (Gabioux et al., 2013). Second, the purpose of this grid is to provide reasonable boundary conditions to higher resolution grids that will soon be configured along the Brazilian coast, which is the area of main interest to REMO and far away from the northern boundary."

"The model was configured to use surface pressure as reference for potential density, aiming for improved representation of near surface fields, at the cost of not representing accurately the Antarctic Bottom Water (Chassignet et al., 2003)."

P1739.L10: Are you relaxing to monthly or annual mean lateral boundary fields? I assume the former, but this need to be stated explicitly.

We are relaxing to monthly mean lateral boundary fields and this has been stated in the revised manuscript as below.

OSD

11, C1163–C1173, 2014

Interactive Comment

“On the lateral boundaries, relaxation to monthly climatological temperature and salinity from Levitus (1982) was applied considering the outermost 10 grid cells and the time scale of 30 days. This approach attempts to preserve climatological shear through geostrophic adjustment and has been successfully used in previous works (e.g. Paiva and Chassignet, 2001; Gabioux et al., 2013).”

P1739, L27 and foll.: in the context of this paper some brief speculation might be appropriate as to the reason for the model discrepancies. Are these typical of comparable non-assimilating implementations of HYCOM? Might they be related to the forcing, to the resolution, or to a generic limitation of HYCOM itself?

We think that several factors contribute to the model discrepancies with respect to the climatology and observations in the North Atlantic. For example, the spatial resolution of 1/4o is not enough to solve the Gulf Stream in our free run. Hulburt and Hogan (Dynamics of Atmospheres and Oceans, 32, 283-329, 2000) have investigated the impact of the horizontal resolution in the Gulf Stream using HYCOM as reference. They showed that only with 1/16o of spatial resolution or higher the results were realistic, and substantial improvements were found when the resolution increased from 1/16o to 1/32o. Also, our vertical resolution is relatively poor, with only 21 layers. In addition, the Mediterranean Sea is not simulated in this grid and is imposed as a relaxation of T and S at the boundary without mass flux. We know that Mediterranean Sea plays a key role in the water masses distribution until 2000 m in the mid-latitudes of the North Atlantic. Finally, we used the NCEP/NCAR reanalysis version 1 forcing that has much lower resolution than other forcings currently available. In our opinion, all these factors, especially the first two, will certainly contribute to the development of substantial model biases in the mid-latitudes of the North Atlantic. We are working to improve the atmospheric forcing and to increase the model horizontal resolution from 1/4o to 1/12o and the vertical resolution from 21 to 32 layers with sigma-2 as reference for potential density. This will allow improving the representation of the Mediterranean Sea and Gulf Stream. We expect that these model biases in the North Atlantic will

OSD
11, C1163–C1173, 2014

decrease substantially in future simulations.

The text starting on P1739, L29 has been improved to include these speculations as below. We would like to call attention that WOA09 climatology was replaced by WOA13 climatology in the revised manuscript, which has gridded fields of T and S in 1/4o of spatial resolution.

"In the North Atlantic large differences are seen between 25oN and 50oN below 400 m. The Mediterranean Water (MW) is more saline, warmer and found further north in comparison with WOA13. The Mediterranean Sea is not simulated in this grid and is imposed as a relaxation towards monthly climatological temperature and salinity at the boundary without mass flux. In addition, the simulated temperature is higher than WOA13 in the upper 300 m of the equatorial region, while the values of high-salinity cores in the subtropical gyres are smaller than the values found in the climatology. It is worth noting that this resolution of 1/4o is not enough for HYCOM to properly solve the Gulf Stream and its associated dynamics (Hulburt and Hogan, 2000), which may also contribute to some of the model discrepancies in comparison to WOA13 in the mid-latitudes of the North Atlantic."

3.1 Calculation of the innovation vector

Figure 2: I'm not convinced that the "light black dashed lines" to show the HYCOM target densities are a good idea: they don't appear correctly on my printed hardcopy of the PDF, and will probably be redundant anyway when the figure is reduced for the final typeset paper.

The light black dashed lines from the density profile in Fig. 2 are extremely important to show that the Argo T/S profiles were successfully projected into the model vertical space with the creation of the pseudo-observed model layer thicknesses. These lines indicate that for each model target density there is a pseudo-observed model layer thickness represented by the step function (blue curve) that crosses the continuous density profile produced by the original Argo T/S data. In order to improve the visual-

OSD
11, C1163–C1173, 2014

ization of these vertical lines, they were substituted by black solid lines in the density profile and all the other vertical lines were eliminated in the temperature and salinity profiles.

The caption of Fig. 2 (P1770) has been changed to include this clarification:

”Figure 2: Profile of potential temperature (oC), salinity (psu) and potential density (kg/m^3) from an Argo float located at 4.04oN and 23oW on 1 January 2010 plotted against its approximation for HYCOM as layer averages. The model target densities are indicated by the solid black vertical lines.”

5.1 Comparison of mean states

P1749, L18 and foll.: Why is the absence of mass flux at the northern boundary relevant or interesting in this context?

We have removed the sentence “Also, no mass flux is allowed in the northern boundary at 50oN”. We reconsidered our point of view and we think the absence of mass flux at the northern boundary is not so relevant to explain the substantial model biases around the mid-latitudinal band of the North Atlantic. The causes we may consider as relevant for the model biases were explained above and were associated mostly with model resolution and limited representation of the Mediterranean Water.

P1749, L20: “It was already expected that the control run would have larger biases around the middle latitude band. . .”. Are you referring to your comments on Figure 1 in Section 2, or is this an expectation based on previous knowledge? Why would you expect these biases?

In Fig.1, the model free run termohaline state from January 1997 until December 2008 was considered to evaluate the model with respect to WOA13. In this figure, substantial biases in the mid-latitudes of the North Atlantic in sub-surface were observed. Consequently, in Fig. 6 the CTL run from 2010 until 2012 had the largest T and S discrepancies with respect to WOA13 in the mid-latitudes of the North Atlantic, exactly

OSD
11, C1163–C1173, 2014

in the same region that was shown in Fig. 1. In this region, the largest impacts of Argo data assimilation were also attained. We have previously pointed out some reasons that may explain these biases.

We have removed the sentence “It was already expected that the control run would have larger biases around the middle latitude band. . .” to avoid ambiguity. In this paragraph (P1749, L16) the possible causes of the model biases were highlighted in the revised manuscript and the text was changed as below.

“As mentioned before, to simulate the MW in the Atlantic, relaxation of T and S in the boundary condition is imposed without mass flux. Moreover, the resolution of 1/4o could also be a limitation to accurately solve the Gulf Stream and its associated dynamics in the mid-latitudes of the North Atlantic (Hulburt and Hogan, 2000). The main purpose of this grid is to provide boundary conditions to higher resolution grids focusing on the Metarea V.”

P1751, L2: The raising of the layer interfaces is one possible consequence of the changing of density as a result of assimilation; convection resulting from a water column becoming statically unstable with respect to the underlying water masses is another. Can you confirm that the latter does not occur in extreme cases?

We cannot ensure that assimilation of Argo data has not created any type of convection resulting from the water column instability. If this occurred after an assimilation step, the model was able to quickly stabilize the water column due to its isopycnal nature in the deep stratified ocean. In HYCOM, each isopycnal layer has a target density. Due to advection, diffusion and forcing, the model layer density may momentarily create deviations from its target density. However, after the model equations are solved, the hybrid coordinate generator (subroutine hybgen.f) relocates the vertical interfaces by mixing higher or lower density waters from adjacent layers and restores the reference densities in the ocean interior to the greatest possible extent. This process also takes into account the minimum layer thickness requirements.

OSD

11, C1163–C1173, 2014

5.3 Adjustment of the altimetry and velocity fields

As the authors state, Figure 12 confirms that there is a general decrease in sea surface height. It would be helpful if a time series of the individual thermosteric and halosteric tendencies were added as additional panels to this figure to compare the relative impacts of the two contributions.

SSH in HYCOM is calculated through the Montgomery potential as a diagnostic variable. In this formulation, the main components are the model layer thicknesses and the barotropic pressure. The baroclinic component is changed due to assimilation of the pseudo-observed model layer thicknesses calculated with both the Argo T and S data. Changes in SSH are mostly produced by changes in the model layer thicknesses and this corrects most of the background termohaline structure. The assimilation of T and S realized after the assimilation of the pseudo-observed layer thicknesses was done in an univariate way and did not impact too much in SSH. This is indicated in Fig.12 which shows that SSH is quite sensitive to the vertical localization of layer thickness and not to the vertical localization of T and S. Because of this, we did not focus on the individual thermosteric and halosteric components of SSH.

We would like to call attention that we have changed the names of the experiments. The control run (CTL) and the assimilation run without any vertical localization (ASSIM) remained the same. However, for the experiments with vertical localization, the new suggested names are VL_TS, VL_DP and VL_DPTS, in which VL means vertical localization, and DP, T and S represent the model variables over which localization was applied.

Technical corrections:

We would like to thank again Reviewer 1 for the time dedicated to point out some technical corrections that improved the quality of the paper.

All technical corrections proposed by Reviewer 1 were accepted and implemented.

OSD
11, C1163–C1173, 2014

Interactive comment on Ocean Sci. Discuss., 11, 1733, 2014.

OSD
11, C1163–C1173, 2014