Ocean Sci. Discuss., 11, C1174–C1185, 2014
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Ocean Science
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Interactive Comment

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

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Received and published: 17 December 2014

General comments:

The authors would like to thank Reviewer 2 for her/his time dedicated to offer important comments and suggestions that will certainly help improving the paper. In our answers, we have adopted the page number and line as in the pdf that is published in OSD to refer to the text.

1) Line 118: I'm not sure whether it is necessary to say T,S(z) is the observation space, as opposed to Delta p. The floats will be measuring pressure p, T, and Conductivity simultaneously so in fact whether you regard T(p) or p(T) or indeed Conductivity (T,p) all can be regarded as legitimate observation spaces. Of course projecting to a particular

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model set of isopycnals will be moving away from Observation space. Just a point for clarity.

We totally agree with the Reviewer comment. We have deleted the reference to z as the domain of the observational space and rephrased the text accordingly. For instance, on P1737, L6 the new phrase is:

"Xie and Zhu (2010, afterwards XZ) used an EnOI scheme to assimilate Argo data, and showed that the TE approach produced significant improvements in relation to simpler schemes, in which the innovation is calculated in observational space."

On P1737, L4 and P1741, L22 of the revised manuscript, we also changed the text and removed the reference to z as the observational space.

2) Line 165: When model is introduced the boundary conditions in the north and south should be stated. What happens at 50N.

The boundary conditions in the north and south were stated on P1739, L9 of the submitted manuscript.

"On the lateral boundaries, relaxation to climatological temperature and salinity from Levitus (1982) was applied considering the outermost 10 grid cells and the time scale of 30 days. Constant barotropic volume fluxes were imposed: zero flux in the north; eastward flux of 110 Sv in the Drake passage; westward flux of 10 Sv in 12 grid points south of South Africa along 20oE; and eastward flux of 120 Sv from the latter region until Antarctica."

However, for completion, we have included more information about the relaxation strategy on the lateral boundaries as below.

"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

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and Chassignet, 2001; Gabioux et al., 2013). Constant barotropic volume fluxes were imposed: zero flux in the north; eastward flux of 110 Sv in the Drake passage; westward flux of 10 Sv in 12 grid points adjacent to South Africa along 20oE; and eastward flux of 120 Sv further south to Antarctica."

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), 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 text has been improved on P1738, L25 to clarify why we have made this decision to cut the grid at 50oN.

"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 resolutions 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."

3) Why is the horizontal influence of 150km chosen in line 351? This is a big decision and a reference should be provided preferably. Does this vary with latitude. Does it change near coastlines? I would expect one of the advantages of using HYCOM would be that larger covariances could be used for thickness decorrelations than for z level correlations? Comment?

The horizontal influence was chosen to be 150 km since this number roughly matches the scales of boundary currents (Ezer e Mellor, 1997; Carton and Giese, 2008). In our case, the formula forces the model error co-variance matrix B to decrease to zero when the distance l_ij between any two arbitrary points reaches 300 km. When l_ij reaches 150 km, the weights of the horizontal correlation matrix decrease to approximately 20%.

This horizontal scale of influence was fixed for the entire model domain and for all depths. The present work was a first effort by our group to implement and validate an Argo data assimilation scheme into HYCOM in the Atlantic Ocean. Of course, in new versions of this assimilation code we will attempt to perform investigations and tests regarding different horizontal influence scales, including its dependence on latitude. For instance, a possible approach is to use the first baroclinic Rossby radius of deformation. In the South Atlantic, our experience (Lima and Tanajura, Brazilian Journal of Geophysics, v.31, 271-288, 2013) showed that the horizontal scale of influence for SLA varied from 120 km in the Brazil-Malvinas Confluence region to 440 km in the central equatorial Atlantic. In the present work, we believe the choice of 150 km was conservative and avoided capturing co-variances that could damage the analysis in the high-variability regions, such as the Gulf Stream and the Brazil-Malvinas Confluence.

We think that the isopycnic coordinate models produce larger horizontal correlation scales than z-coordinate models because they better preserve water masses and the flow tends to occur along the isopycnals. However, our model free run contains substantial biases and inaccurate covariances that limit the use of large horizontal and vertical localization radius. Again, we were conservative to use a relatively small horizontal scale of influence. Today, we are implementing new grids with higher resolution and working towards a more accurate free run that will enable us to realize better analyses.

To justify the choice of the horizontal scale of influence, a new paragraph before the subsection 3.4.2 (P1745) has been included as below.

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“The horizontal scale of influence was chosen to be 150 km because this number roughly matches the scales of boundary currents (Ezer and Mellor, 1994; Carton and Giese, 2008). Since the present work was the first REMO effort to implement an EnOI scheme to assimilate Argo data into HYCOM, the horizontal scale of influence was fixed for the entire model domain and for all depths. In this case, the choice of 150 km was conservative in order to avoid capturing co-variances that could damage the analysis in high-variability regions, such as the Gulf Stream and the Brazil-Malvinas Confluence.”

4) Section 4.2. I found the notation of VLOCDP etc.. very cumbersome and not intuitive at all. Could you not choose shorter and easily expt names?

We have changed the name of the experiments with vertical localization in the text and also in the figures of the revised manuscript. The control run (CTL) and the assimilation run without vertical localization (ASSIM) remained the same. However, for the experiments with vertical localization, we agree with the Reviewer that the names could be improved. 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.

5) There is some danger of comparing your results to WOA09 as a reference. The Argo data will show the oceans being warmer than the WOA09 climatology especially in upper levels. WOA09 has lots of older data and the upper ocean has been warming so perhaps something more like an Argo climatology would be a better reference.

We agree with the Reviewer’s concern about a fair comparison between the experiments results and climatology. Therefore, WOA09 climatology was replaced by the recently available WOA13 climatology. In WOA13, the user can choose specific time spans to compose the climatology. This avoids the usage of older datasets considered by WOA09. For this reason, the mean states of the assimilation experiments from 1 January 2010 until 31 December 2012 were compared with WOA13 over the period

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from 2005 until 2012. In this period, the global coverage of Argo floats was used to compose WOA13 climatology. Also, WOA13 has improved spatial resolution (1/4o) in comparison with WOA09 (1o). All figures associated with WOA09 were redone with WOA13.

We would like to call attention to the fact that we have used WOA13 from 1995 until 2012 to compare the mean termohaline state in Fig.1 with our free run from 1997 until 2008.

In general, when we compare WOA09 and WOA13 climatology, the results are similar. However, as the Reviewer has mentioned, the upper ocean is warmer in WOA13 over the period from 2005 until 2012. This can be particularly seen in Fig. 7 of the submitted and revised manuscript that shows sections of temperature along the equator and 30oN using WOA09 and WOA13 as reference, respectively.

The text has been changed on P1739, L25 and P1749, L6 to include new information:

“Figure 1 shows the mean state of temperature and salinity simulated by HYCOM from 1 January 1997 until 31 December 2008 and its comparison with the World Ocean Atlas 2013 Climatology (WOA13) (Boyer and Mishonov, 2013) along 25oW for the upper 1000 m. WOA13 climatology shown in Fig.1 spans the period from 1995 until 2012.”

“Also, WOA13 climatology for the period from 2005 until 2012 was used to evaluate the mean state of T, S and heat content of the upper 300 m (HC300) in all experiments. In this period, WOA13 includes the global coverage of Argo floats to compose the climatological gridded fields of T and S in 1/4o of spatial resolution.”

6) Line 510: It occurs to be it would have been better to make a map of the vertical displacements instead of Fig 6 since Fig 6 everything looks rather similar.

Our intention in presenting Fig. 6 is to show the impact of the assimilation in the density stratification until 300 m. This impact is directly linked to the vertical displacements of each isopycnal layer. If we had chosen a 2-D map of the displacement of a single

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layer interface, many figures would be necessary to characterize the impact of the assimilation in different depths. Also, Fig. 6 is consistent with the changes in T and S shown in Fig. 5 along the same longitude.

To highlight the stratification changes in Fig. 6, we have substituted two black solid lines by two white dashed lines. Now, Fig. 6 has a total of three white dashed lines to improve the visualization. As can been clearly seen, the lowermost white dashed line has dramatic changes in the North Atlantic as well as changes in the equatorial and South Atlantic. The caption of Fig. 6 (P1774) was modified accordingly:

“Figure 6: Potential density (kg/m^3) and the position of the layer interfaces along 25oW in the upper 300 m for the (a) CTL run and the prior state of the (b) ASSIM and (c) VL_DPTS runs from 1 January 2010 until 31 December 2012. The white dashed lines represent the interface between the 5th and the 6th, between the 8th and the 9th, and between the 11th and the 12th isopycnal layers, from top to bottom.”

7) Line 544-45: Some discussion of why this is would be appropriate

The text starting on P1752, L7 has been improved to comment this issue as below.

“Probably, this is because there is a strong vertical correlation among several model layers in the upwelling region that is not considered by the vertical localization of layer thickness in the VL_DPTS run. Similar behaviour was observed in the region of AAIW formation shown in Fig. 5.”

8) Line 563-64: There is no Argo data to the north here because it is a shelf region and much shallower

The text on P1752, L21 was improved to explain the absence of Argo data in this region as mentioned by the Reviewer.

“No Argo data was available in this shelf region - to the north of the observed Gulf Stream - to constrain the model (Fig. 4), so that the assimilation runs could not correct this discrepancy.”

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9) Line 586: One has to be careful of simply increasing alpha as you may overfit the data. If the model cannot represent the eddies then it may not be best to impose very large data driven changes.

We agree with the Reviewer and have considered this important information in the text on P1753, L11.

"This indicates that the analysis error is close to saturation and that changes in the assimilation parameters such as α may be necessary to better constrain the model towards observations. Here, α is equal to 0.3. However, care should be taken when increasing α. The relatively small value chosen here considered two aspects. First, a small value of α helps avoiding abrupt changes in the model state and numerical instability considering the relatively large discrepancies between the model background and observations in certain regions. Also, the model resolution is relatively low and the use of high values of α could overfit the data and try to introduce mesoscale patterns that cannot be resolved by the model."

10) Line 617-19. Some more discussion of why vertical localisation for T,S is not so important? Clearly much of what you are seeing is vertical displacements of isopycnals and not changes in the T(S) relation on isopycnals.

The text on P1754, L13 has been improved to include more discussion about why the vertical localization of T and S is not so important. The text was also reorganized and the phrase on P1754, L27 from the original manuscript starting with "Also, the vertical localization..." was deleted to avoid repetition.

"This is mainly because the most important HYCOM variable to determine the termohaline structure is the model layer thickness (Thacker and Esenkov, 2002; Thacker et al., 2004; Xie and Zhu, 2010). For instance, according to HYCOM formulation, if the analysis increments of T and S decrease the density of a specific layer in comparison to its target density, the model attempts to move the lower interface downward, so that the flux of denser water across this interface increases the layer density (Bleck, 2002).

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The opposite happens if there is an increase of the layer density compared to its target density. Therefore, the analysis increments of T and S will try to indirectly change the model layer thicknesses in order to adjust the layer densities to their target densities. However, in the EnOI scheme proposed here, the assimilation of the pseudo-observed layer thicknesses is realized before the assimilation of T and S, and is very effective in changing the model layer thicknesses. This may explain why the model termohaline structure is much more sensitive to the vertical localization of layer thickness rather than to the vertical localization of T and S. The latter was studied by XZ and they also did not find any significant impact in their assimilation experiments."

11) Line 761: HYCOM+NCODA?

HYCOM+NCODA is the abbreviation of the HYCOM-Navy Coupled Ocean Data Assimilation system. The results of this system were taken as reference to compare the sub-surface velocities of the assimilation runs since there is a lack of an in situ subsurface velocity monitoring system. We mentioned that on P1749, L3.

"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 surface velocity field by the calculation of the RMSDs for the u and v components.

12) Lines 783-86: It should be noted that constraining the current systems near the equator is a very different prospect than constraining them away from the equator because of geostrophy. At the equator the density distribution will adjust to the currents and not the other way round. Away from the equator you would hope for a much clearer

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impact on ocean currents from the assimilation of data.

We revisited the results obtained by Xie and Zhu (2010) regarding the impact of the Argo data assimilation on the circulation. They mentioned that they could obtain impact in the equatorial region, but this impact was not always positive when compared to TAO/TRITON array data. For instance, they found that their westward currents in the west equatorial Pacific occupied a thick layer (more than 100 m), and that the maximum of the equatorial undercurrent was too deep in the eastern region. We suspect that these changes are mostly associated with remote impacts of the Argo data assimilation in extra-equatorial regions. They did not elaborate too much about these results, but they attributed the negative impacts on the velocity field to the model biases and inaccurate ensemble co-variances. Therefore, we are not comfortable to discuss very much about their results in our paper. We have modified our text on P1759, L18 to better consider the Reviewer's comments 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."

13) Lines 845-847: The stationarity of the ensemble is a big issue it seems and the real advantages of the ensemble method can only come from allowing time evolution, otherwise the you cannot assess different levels of ensemble spread.

We agree with the Reviewer. In the Ensemble Kalman Filter, matrix B allows time

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evolution and captures the so-called "error of the day." In the EnOI, the stationarity of the ensemble is clearly a disadvantage. For this reason, we have pointed out in the conclusion that EnOI method is also dependent on the quality of the ensemble members, not only on the ensemble size. For instance, if our ensemble members were more accurate, it could be expected that the strong vertical localization of layer thickness would not lead to improvements in the analysis.

In order to overcome this limitation, new ensemble members were recently produced by long data assimilation runs. These analyses replaced the free run ensemble members employed in the present work and contained more accurate co-variances. Preliminary results have already shown improvements in the quality of the analysis. In a near future, a new paper will be written with a focus on the quality of the ensemble members and on the assimilation performance of the EnOI scheme.

In order to explicitly mention the EnKF as a potentially better scheme with respect to the EnOI we have modified the text on P1761, L21 as below.

"This result reinforces how important the quality of the ensemble members is in order to improve the performance of the assimilation, particularly in the EnOI scheme, in which the ensemble is stationary in time and does not evolve with the model integration as in the EnKF (Evensen, 2003; Oke et al., 2005; Oke et al., 2008)."

Technical corrections:

We would like to thank again the Reviewer 2 for the time dedicated to point out some technical corrections that will improve the overall quality of the paper.

All technical corrections proposed by the Reviewer 2 were accepted, except two of them. The first suggestion we did not include was related to the comment 166: and excluding Indian? Or “indo-pacific”. Our domain was from 98oW to 20oE, and from 78oS to 50oN. The Pacific Ocean from 98oW to South America was excluded, so that the western boundary in the southern part of the domain is in the Drake Passage.

Therefore, we don't consider the Indo-Pacific region.

Also we did not change Eq. (8). This equation is written exactly as in the work by Xie and Zhu (2010) and it was included here for completion.

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