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Ocean Science Discussions

Interactive comment on “Implementation of position assimilation for ARGO floats in a realistic Mediterranean Sea OPA model and twin experiment testing” *by* V. Taillandier and A. Griffa

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We thank the reviewers for their thorough reading of the manuscript and for their constructive comments. We have followed their suggestions and we think that the manuscript is significantly improved thanks to them.

Notice that the figures mentioned hereafter cannot be displayed in this interactive mode. They are available in the document 'letter to the editor'.

Referee #1:

“section 2.1: The assimilation method produces a velocity correction "Delta u(z_rho)" at the parking depth at the initial time (state of the art) or at the final time (operational

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mode). This correction is then projected on the vertical axis to provide a correction over the whole water column. An operator is applied in order for this velocity correction to: - conserve the basin volume - be non-divergent. Reading (page 261;lines 9-11), the depth integrated velocity correction "Delta U" is changed from equation 2 to equation 3: "..."Delta U" is further imposed to be non-divergent and re-computed from the diagnostic stream function...". For the sake of reproduction/traceability of results some technical details should appear concerning the way the basin volume is enforced and how the flow stays non-divergent."

The projection of the velocity correction is based on statistical information (vertical correlations) and model constraints (basin volume conservation). So a consistent 3D velocity correction is provided first by a consistent barotropic flow correction Delta_U, second by a baroclinic structure around Delta_U. For model consistency in rigid lid approximation, Delta_U is stated non-divergent. In numerical practice, the divergent part of Delta_U is removed by computing the associated stream function. This operation has been clarified in the text (last paragraph of section 2.1).

"section 3.2: This should be actually a minor comment (but since it is just a guess...): there is no information about what is the "wrong" initial condition. My guess is that all the model state variables (temperature, salinity, meridional and zonal velocities) are perturbed ("wrong"). If only velocities were perturbed, the study would lose most of its interest. So provide some details."

The reviewer is right: the whole model state is perturbed in model consistency by taking velocity and mass fields one year after the initial time of the experiment. This is now clarified in the text (second paragraph of section 3.2).

"section 3.2: Is the "background" integrated from the initial conditions of 1 March 2000 AND forced by fluxes corresponding to the month of March 1999 ? ("...evolve according to forcing and dynamics" is not accurate enough)."

The three model simulations are performed with the same forcing functions corresponding to March 1999. As also clarified in the text, the background circulation evolves freely, i.e. without model state correction, from the wrong initial conditions.

“section 3.2: I am not sure to clearly understand what is a "perfect" data compared to the "realistic" data; especially when looking at figure 2. As I understood from the text, a perfect data position is given by: - the float position at which a float starts drifting at its parking depth after its dive - the float position at which a float stops drifting at its parking depth before starting its upcast. In realistic condition, those positions are not known. An Argo float is positioned after it surfaces (say typically a couple of hours between it surfaces and the first satellite localisation). Then the float is advected by surface currents for 4-6 hours to transmit its data; it is still positioned. The last position is known a couple of hours before the dive starts. Hence "shear errors" is composed with: the accumulated drift from the parking depth to the surface + surface drift before the first position AND surface drift after the last position + accumulated drift during the descent to the parking depth. Looking at Fig. 2, we get the impression that the "realistic" position is taken in the middle of the surface drift, which probably overestimates the "shear error", especially because surface currents are more intense than subsurface ones. Some clarifications on this "shear error" should be given on Figure 2 and the related text. Also a quantification of the "shear error" should appear somewhere in the text (depending on the kinetic energy of the region).”

According to the reviewer’s comment, we have clarified the definition of the observational error handled in this study (first paragraph of section 3.2). Notice that in comparison with in-situ Argo floats, surface drifts have not been taken into account in these experiments, nor the time lag for communication. Our observational errors should then be considered as lower bounds with respect to in-situ errors. A quantification of our observational error has been realised on the basis of the whole trajectory set (182 floats). The ratio between an average distance (xe) of the shear drift and an average distance (xd) of the drift at 350m is equal to 19%. The value of the ratio has been indicated in the text (fourth paragraph of section 3.2). Notice that this value is close to the one obtained from simulated Argo floats launched every month along VOS tracks during the period 1998-2002 (cf figure hereafter).

“section 3.2, page 266 line 20: I do not understand the following: "In order to minimize the effects of initial conditions, in all cases the homogeneous release is maintained". What is the "homogeneous release" ?”

The reviewer wisely underlines this sentence which might be misleading. Simulated floats are released on the basin according to homogeneous spatial distribution. This procedure disables possible strategies of float deployment to improve observational information. Such clarification has been done in the text (fourth paragraph of section 3.2).

“section 4.1, Fig. 4: Why does the adjustment error do not start at 100% with the procedure "state of the art" ? Why is the correction done at days 2.5, 7.5, 12.5, ... ? I expected a first correction at day 5, and then at days 10, 15, 20, 25, 30 since a sequence is 5-day long (starting at 100% at day 0, that is day 1, 00h". Did I miss anything ? Clarify.”

The measure of the adjustment, E defined in equation (8), is computed from velocity fields averaged over successive 5 day sequences. So the sequence number is identified to time axis at 5 days resolution. The evolution of E is represented for each sequence. Its value is marked at day 2.5 for the sequence March 1 - March 5, day 7.5 for March 6 - March 10Ě The definition of the time axis is clarified in the text (last paragraph of section 3.2). In this representation, the correction in the procedure “operational” is provided at the end of each sequence, so its effect is measured only in the E-value of the next sequence. This point is also clarified in the text (second paragraph of section 4.1).

“section 4.1. State in the text that what is shown is the estimate at the end of the last sequence (it is written in the legend only).?”

It is now stated in the text that velocity and mass fields are represented at the end of the assimilation period, i.e. at the last sequence (third paragraph of section 4.1).

“Despite temperature and salinity fields are corrected for thermal wind shear, I guess an adjustment phase still exists between the velocity field and the tracer field at the beginning of the integration starting from corrected fields. Could the authors comment on this since is if often a problem for operational data assimilation ?”

The adjustment phase mentioned by the reviewer does not occur with the present data assimilation method since the corrected fields belong to admissible model solutions. On the other hand, such behaviour can be relevant using a best fit in weak constraint such as OI or Kalman filtering. This point is now mentioned in the first paragraph of section 2.3.

“section 4.2, page 269, line 17: I guess that "the passive relaxation" refers to the natural adjustment between velocity/tracer fields when integrating the model. The term relaxation is often used to relaxation terms added to the model prognostic equations, I was somewhat confused by the terminology.”

This misleading terminology has been changed according to the reviewer’s suggestion (first paragraph of section 4.2).

“section 4.2, page 271: it is stated here (and also in the next sections) that the estimated surface fields are similar to the truth. I was wondering whether the surface forcings (plus relaxation to temperature and salinity at the surface if it exists in this configuration), that are perfect, may by themselves also lead to improvements in the surface properties/circulation. Indeed, "wrong" conditions are those of March 1, 2000. If you apply the true forcing fields (wind, heat, E-P and may be some T&S surface relaxation) that correspond to March 2000 to those "wrong" initial conditions, then the first levels of the model will probably have the tendency to converge to the true state without any assimilation during the time integration.”

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We agree with the reviewer. This comment has been added in the summary of the section 4.

“section 5, p272: Again, a quantification of the "observational error" produced by the "shear drift" (in the model!) would help to convince the reader for the discussion that starts line 21: "This different behaviour..."”

With respect to a previous reviewer’s comment, our observational error has been assessed quantitatively. The discussion is now supported by the mean value of the shear drift introduced in the simulated float trajectories (section 5, third paragraph).

“The exercise is repeated for a month. What happens when the procedure is cycled for a year ? Is there a convergence to the true state ? Some corrections of the model state are due to wave propagation. It may appear that, with time, small scale propagating corrections generated by some data assimilation lead to a sea state that regionally diverges from the true state. Can we expect such behaviour when the process is cycled for a longer time period ?”

The assimilation period is limited to represent the persistence of perturbations in initial conditions. Its duration is then fixed to the order of the time scale for mesoscale circulation patterns. It is seen that with high data coverage, the convergence to the true state is effective in a month. On the other hand, the adjustment rate is low but stationary with low data coverage. In this latter case, the mechanism of spurious error propagation described by the reviewer has been noticed after one month from Figure 10. It would be definitely reinforced with time keeping the same data coverage. This point is now mentioned in the text (third paragraph of section 6).

“More fundamentally, in this manuscript, temperature and salinity corrections are produced from corrections of the velocity fields to obey the thermal wind shear equilibrium. I am somewhat surprised that the author do not use the vertical profile of temperature and salinity measured by ARGO floats (surface-2000m). Why are they not used ? Is there any inconsistency with the thermal wind shear they used ? Does the use of such

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measured T and S profile helps to improve the assimilation of float position ?"

Float positions provide information on horizontal density gradients, which might be at first sight inconsistent with TS profiles. But we believe that simultaneous assimilation of position and TS would be a promising avenue to improve the obtained model state corrections. This comment is obviously placed as the last word of the manuscript (section 6, last paragraph).

"As far as I know, the ARGO network should not exceed (unfortunately) a resolution of one float per 3 x 3 degree (lat x long). That means 3 to 4 floats in the Med Sea region considered here. Furthermore, positions of ARGO floats deployed in the world oceans are known once every 10 days (not once every 5 days). Reducing the number of floats significantly decreases the performance of the method as shown on Figs 9b and 10. We still have a few improvements however and maybe that cycling the assimilation on a longer time would help. But, what would be the impact of reducing the frequency of positions to one every 10 days with such a low float density ? I guess the answer if of interest..."

Molcard et al. (2003, JGR) showed how the trajectory sampling influences the skills of Lagrangian data assimilation. The consecutive effects when the sampling is close to the typical Lagrangian time scale (in the order of 5 days for the intermediate Med layer) have been discussed in section 6 (fifth paragraph). One should expect such features to be amplified with a 10 day resolution.

"clearly state in section 2.1 that the velocity correction "Delta u(z_rho)" at the parking depth is done at each (x,y) horizontal grid point no matter a float position is available at (x, y)."

A velocity correction is operated at the mesh points neighbouring the float trajectories. The Eulerian nature of the correction has been reinforced in the first paragraph of section 2.1.

“section 2.2: If I am right, remind the reader that the prior state is either the oceanic state at the initial time (state of the art) or at the final time (operational mode).”

In the two cases, the prior state is described during the same sequence. The velocity estimate (which is time-independent) is provided by the prior trajectories, instead the prior state used for TS estimation (with respect to the time-independent velocity estimate) is a sequence average. This point has been clarified in the first paragraph of section 2.2.

“section 3.1: while referring to the ARGO floats in relation to MFSTEP (page 264; line 11), a useful information would be to give the effective number of floats deployed in the region.”

The number of floats (4) deployed in the region is now indicated in the text (second paragraph of section 3.1).

“section 3.2, Eq. 8: in this equation "t" is the final time-step of a given sequence, right ?”

As detailed in the response of a previous reviewer’s comment, the time resolution is given by the sequence duration, so the variable t is identified to the sequence number on the time dimension. This point has been clarified in the text (last paragraph of section 3.2).

Referee #2:

“Comment 1: Equations (1)-(3) define the method of choice of the function R(z) of vertical distribution of the velocity. The normalization approach is based on the condition for volume conservation. Another possible approach would be just shifting the velocity profile by the difference between the non-divergent and initially divergent barotropic velocities. The latter procedure (shift of the velocity profile) in opposite to relation (3) would not create additional shear in the corrected velocity. This additional shear baroclinic velocity in (3) is imposed only by considerations for corrections needed for

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barotropic velocity. This additional shear will be then projected on T and S changes by (6). I think the authors need to motivate this choice of R(z) normalization."

Shifting the depth integrated velocity, as suggested by the reviewer, has been tested during the development of the method. Numerical instabilities occurred along the coastlines and developed where the intermediate layer intersects the sea bottom. In the present approach, such depth integrated shift is used to modulate the amplitude of the velocity profile. In this way, numerical instabilities are circumvented with a consistent velocity correction at the sea bottom. Considering the vertical shear, its amplitude equal to u(zp) in the first approach is augmented by the depth integrated shift in the second approach. Both approaches are theoretically correct since the separation of single subsurface velocity information into barotropic / baroclinic contributions is an underdetermined problem. So the choice of the second approach is only motivated by numerical modelling considerations. The last paragraph of section 2.1 has been clarified according to the reviewer's comment.

"Comment 2: I think equation (6) for the cost function requires additional discussions. As formulated it means that all elements of the vector of the misfit between the velocity correction and its geostrophic component should have the same weight for all depths. Are there any specific reasons for this choice? Usually realistic ocean models have irregular vertical levels and the contributions to the misfit from each level may not be equivalent. Another question is if there is always a unique solution for the corrections of T and S from (6)? Is M a linear operator or it is a locally linearized operator at points T0(z), S0(z), where T0(z) and S0(z) are the vertical profiles of T and S. How the matrix of M is computed in this work?"

The cost function is defined in equation (6) by choosing vertically independent and homogeneous velocity shear errors. But the contribution of each velocity shear misfit onto TS profile estimates (defined by equation (7)) is weighted by background error variances. In this way, the vertical distribution is intrinsically defined by the matrix B. The reviewer is right to underline that M is a locally linear model. It is governed by

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the linear thermal wind equation and the linearized equation of state around prior TS profiles. The adjoint model MT is built with the associated adjoint equations around the same prior TS profiles. So the optimal solution is searched in the neighborhood of these reference TS profiles. Moreover, it only corresponds to a local minimum of the cost function. The last paragraph of section 2.2 has been clarified according to the reviewer's comments.

"Comment 3: Fig 5 shows the position of hydrographic sections used to estimate the error in the T and S corrections. Though the method of correction of T and S is not in any means a simple one, I would say that the correction along these sections would be "easy" for the method to capture, because there is a strong mesoscale flow variability along them. Did the authors try different sections in their skill scores studies, for instance like the meridional section along 7.5E."

Different sections have been explored in order to focus on several circulation regimes sampled by the simulated trajectories: coastal current fluctuations along the Gulf of Lions shelf, intense eddies on the Catalan sea, weak eddies offshore Sardinia. The choice of this section was motivated to be representative of these features. We agree with the reviewer that the intense mesoscale variability which characterizes this wintertime circulation is helpful and appropriate for testing and validation of the method. However, the following hydrological sections (same as figure 7 but along 7.5E) show significant corrections brought in the part crossed by trajectories.

Interactive comment on Ocean Sci. Discuss., 3, 255, 2006.