

Interactive comment on “Assessment of the impact of TS assimilation from ARGO floats in the Mediterranean Sea” by A. Griffa et al.

A. Griffa et al.

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Response to reviewers

REVIEWER 1

Major comments:

1) What kind of correlation model are (the authors) assuming?

We have used the standard covariance model adopted in SOFA, that is:

$$C(dr,dt)=(1+dr+dr^{**2}/3)*exp(-dr)*exp(-dt^{**2})$$

Where $dr=2.103803*\sqrt{dx^{**2}+dy^{**2}}$ is the adimensional spatial distance and dt the adimensional time interval between two data, respectively. Let the dimensional x -, y -distances and t -interval be DX , DY and DT , respectively, then:

$$dx=DX/xr, dy=DY/yr, dt=DT/rt$$

where, xr and yr are the covariance radii (in our case $xr=yr$) and rt the e-folding time.

2) Have the authors verified the sensitivity of results to the spatial and temporal correlation scales chosen?

The values of the parameters used in covariance function were chosen to be the same as in the Mediterranean Forecasting System operational code. This was a decision based on the results of a cooperative work of the whole team involved in the MFS. Up to now it has not been the aim of the OSSE exercise to study the sensitivity of the assimilation to parameters, as this is the first time that such studies are performed in the Mediterranean Sea and a lot of work has still to be done to optimize the system. The adopted values are probably not the optimal ones, since, for instance, the spatial scales depend on the region and on the season, but they were considered reasonable for our purposes. This limitation has now been noticed in the paper in the Summary (Section 5) at the end of the fifth paragraph.

- More generally, the results presented in this work are based on a particular configuration of the MOM model, with a particular forcing etc..... Have the authors evaluated the impact of such factors on the methodology procedure?

As stated in the Introduction and Summary, our experiments are focused on the uncertainty deriving from incomplete knowledge of the initial conditions, and we have not directly investigated the impact of model and forcing errors.

3) In Fig 3 (relative errors versus time in days) it is clear that the assimilation is successful in all cases, according to the authors "especially marked during the first two assimilation cycles". Why is not so clear (during the first two assimilation cycles) for salinity variable in winter (at the Eastern basin)?

This is likely to be due to the fact that the floats do not efficiently sample the strong salinity front during the initial cycles. This is now noticed in the text in Section 4.1,

second paragraph.

Minor comments:

All the suggested changes at pg 673, 682 and 686 have been done

REVIEWER 2

Major comments:

1) The launch positions are not very important as there is no control on the trajectories after launch. Rather than to look for optimal launch positions one should look for optimal coverage.

We have clarified this point in the text, stating that the experiments are targeted to test coverage in the Abstract and in the Introduction, and explicitly discussing the issue of the impact of initial launching ' versus float life in Section 4.1, fifth paragraph.

2) ... T and S are assimilated without adjusting the velocity. As shown by Burgers et al. (JPO 32 (2002), 2509-2515) this approach leads to sub-optimal results. A revised version should at least discuss this point.

The reviewer is correct, the TS assimilation is not balanced. We thank the reviewer for pointing out the reference. We have now discussed this point in the Summary, end of the sixth paragraph, and added the reference.

3) In section 4.3 the authors compare the results from assimilating Argo data with those from assimilating XCTD data. This comparison suffers from two flaws, (a) as conceded by the authors XCTDs do not exist, and (b) the assimilation procedures are different. So what is the value of this comparison? Omit in revised version.

We agree with the reviewer that the comparison in Section 4.3 had some serious flaws as performed in the previous version. Nevertheless we think that the comparison is of potential interest since it shows a comparison between two different sampling strategies, i.e. collecting profiles at high resolution fixed points along VOS tracks versus col-

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lecting them at lower resolution following buoy trajectories. For this reason, we have extensively modified the section to address the reviewer concerns, and we have maintained it in the revised version of the paper.

In particular, regarding the reviewer comments :

a) We notice that XCTD actually do exist, and they are presently tested in the framework of MFSTEP and of other operational systems, even though we agree with the referee that assuming such extensive launches as in our OSSE experiment is not realistic. On the other hand, the point here is not to actually test XCTD potential, but rather to test different sampling strategies, as explained above. This point is now more explicitly explained in Section 4.3, first paragraph.

b) To address the correct criticism of the reviewer, we have changed the experiments using exactly the same assimilation procedure for the two data sets (except for the fact that the VOS and MEDARGO experiments have different sampling interval as in the real in-situ observations). We have also modified the VOS data coverage in order to maintain consistency between the amount of data in the eastern and western basin, as for the MEDARGO data. 200 profiles per month are now considered in each sub-basin for the VOS data, corresponding to twice the maximum coverage during MFS. By comparison we recall that the the MEDARGO profiles, corresponding to twice the MFS coverage, are 65 per month As a result of these changes, the entire Section 4.3 has been modified: Fig.10 is new, showing the methodologically consistent results, and the text has been re-written.

The results show that the assimilation of the VOS profiles leads to a maximum improvement of about 10\% with respect to MEDARGO assimilation in the western basin and virtually no improvement in the eastern basin. This is almost surprising given that the assimilated VOS profiles are about three times the MEDARGO profiles, and it suggests that spatially sparser profiles from floats can be more efficient, probably because they approximately follow flow features. Also, it is possible that the VOS horizontal reso-

lution of 12 nautical miles is redundant, in the sense that adjacent profiles might not be independent. This though could be a consequence of the MOM model resolution which does not completely resolve the mesoscale features. Further testing to assess this point are planned using a higher resolution model and appropriate statistical testing to verify data independence. These points are discussed in the last paragraph of Section 4.3.

4) The paper can be considerably shortened by avoiding overlapping information. A lot of information from the Introduction is repeated in the Methodology

We have shortened the paper following the indication of the reviewer. Some of the information on the simulated float cycle have been left in the third paragraph of the Methodology (Section 2) since they are specific to the numerical procedure.

Minor comments:

All the suggested changes in the text at pgs 673, 688, 681, 682, 683, 684, 686 have been made

Figures, general: For differences ,e.g, Fig. 5c, use a centered palette which clearly distinguishes between positive and negative, e.g. light centered palette. Fig. 5c: redundant, same as Fig. 6a. Fig. 6: the panels have different colour bars Fig. 6: "free-assim" as plot title should probably read "assim-free" - cf. lege\ nd and Fig. 7c. Fig. 7b: redundant, same as Fig. 5b. Fig. 7c: redundant, same as Fig. 6b.

We have followed all the reviewer suggestions.

Figure 5 was modified: last panel control-free was replaced by assim

Figure 6 was modified: lower panel was replaced by assim-free

Old figure 7 was removed

We've used light centered palette for differences.

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