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Ocean Science Discussions

Interactive comment on "The assessment of temperature and salinity sampling strategies in the Mediterranean Sea: idealized and real cases" *by* F. Raicich

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Introduction

This document is based on the comments by Referee's #1 published in OSD. Questions raised by the Referee are underlined and labelled with letters or number. Author's replies follow the questions and start with an asterisk; reference is made to pages and lines in the text published in OSD. Pieces of text that have been modified or added are written between quotes; at the end their locations in the revised manuscript are reported in square brackets. A number of points that were probably unclear have been rewritten and completed.

Some Referee's remarks concern the assimilation method. This paper (together with

Raicich and Rampazzo (2003)) describes the first attempt to perform sampling strategy assessment in the Mediterranean Sea, using the same setup adopted in the forecasting system. Therefore, there is space for improvements and tests of other methods. No comments on comparisons could be provided, since no other approaches were tried.

Here follow the answer to Referee's comments.

*** Answers to “General comments”

A) ... However, in the sampling strategy design field, statistical assessment and design methods have also been widely used. It is a regret that the author didn't mention it.

* The sentence at Page 130, line 11-14 has been extended to include also reference to papers dealing with statistical methods:

“OSSEs were also recently applied to observing systems design assessment in the Atlantic Ocean, using statistical methods (Guinehut et al., 2002; Guinehut et al., 2004), in the Mediterranean Sea, using twin experiments (Raicich and Rampazzo, 2003; Griffa et al., 2006; Taillandier et al., 2006) and in the Baltic Sea and North Sea, in the Optimal Design of Observational Networks project (She et al., 2006).” [Page 3, lines 4-9]

The relevant references have been added to the list.

B) Substantial conclusions have been achieved on the assessment of real VOS, SRGO, M3A buoys and Gliders sampling strategies and ideal VOS and M3A sampling strategies. However, these results are limited by the model and assimilation methods and parameters used. The author may discuss more about these limits.

B1) Firstly, the forecast error covariance radius was chosen as a constant (45 km). Such an option actually enhance the importance of the medium-scale phenomena in the forecast. The actual forecasting covariance error is inhomogeneous and may have a large scale pattern.

* The assimilation system was chosen and set up as in the real operation of the Mediter-

OSD

3, S258–S265, 2006

ranean Forecasting System. 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. In fact, this is the first time that such a study is performed in the Mediterranean Sea and a lot of work has still to be done to optimize the system. The magnitude of the forecast error covariance radius is a compromise for the whole Mediterranean basin. A constant value is probably not the optimal choice, since the spatial scales depend on the region and on the season, but it was considered reasonable.

B2) Secondly, the OI method does not fully utilize the temporal evolution information in comparison with adjoint method. For the real sampling strategy, since ARGO and M3A provide more temporal evolution information than the VOS, the real sampling OSSE results may be influenced by using an adjoint method.

* It was a basic choice to perform the OSSEs using the same assimilation method adopted in the operational forecasting system, that was implemented some years ago when VOS were the main in-situ ocean data source. MedARGO started to be deployed in 2004. Other assimilation methods were not implemented yet when this work started.

At the end of Sect. 6 the following text has been added to comment on point B (B1 and B2):

“It should be remarked that the approach followed in this work exhibits some limitations, among which: a) The twin experiments are designed and analysed in order to assess the impact of data that simulate near-real time observations used for assimilation for forecasting purposes. Different objectives, like, for instance, routine monitoring, may not benefit from the assessment performed in this work and may require different approaches. b) The OSSE results depend on the model used to perform the simulations. As an example, the 1/8° Mediterranean GCM used here cannot reproduce small, but highly energetic structures that may be critical in certain areas. c) The synthetic data used in the OSSEs are extracted from the model itself, thus being fully consistent with

OSD

3, S258–S265, 2006

Interactive Comment

it. This may result in an optimistic assessment with respect to the use of real data. d) Data error covariance is kept constant instead of being variable in space and time.

The OSSEs described here and in Raicich and Rampazzo (2003) represent the first experience of this kind in the Mediterranean Sea. They have been performed using data assimilation based on optimal interpolation because that was the choice for the operational forecasting system. As a future perspective, the application of other methodologies for data assimilation, such as, for instance, those based on variational or ensemble techniques, may help to overcome some limitations of the present work." [Page 15, lines 10-26]

C) It is recommended that the paper should provide more information on how good the model works in the Med. Sea (i.e., the model's ability to simulate major features of the Med. Sea).

* The model used in the OSSEs (1/8°, 31 levels MOM-1.1) has been used extensively in the past and it is not possible to fully review its performance in this paper. Beside Korres et al. (2000), other references have been included, and added to the list, that present result obtained with the model. The paragraph at Page 132, lines 3-12 has been extended to comment on this point:

"The model is capable to reproduce the major observed features of the circulation and water mass variability, as shown by Castellari et al. (1998, 2000) and Demirov and Pinardi (2002)." [Page 5, lines 8-10]

The relevant references have been added to the list.

*** Answers to "Specific comments"

1. How is the initial condition of the control run specified, and how is the difference of initial fields between the control run and assimilated runs and free runs generated?

* This information was provided at page 134, lines 14-19. Initial conditions are extracted from an interannual run with XBT assimilation and SST heat flux correction. The text

has been modified in order to make the point clearer:

“In the summer OSSEs the control run is initialized on 1 September 1999, with end on 9 November 1999, while in the winter OSSEs the control run covers the period 1 February - 10 April 2000. Free run and assimilation run are initialized on 1 September 1998 (summer) and 1 February 1999 (winter), that is exactly one year before the control runs. The initial conditions for all the above-mentioned runs are taken from an interannual simulation performed with assimilation of XBT and SST heat flux correction (Demirov et al., 2003), forced by ECMWF 6-hourly reanalyses.” [Page 6, lines 17-23]

2. It is better to distinguish the Western and eastern Med. Sea by using a line in a figure, rather than the sub-regions covered. (p. 131, line 25)

* A line has been introduced in Figure 3 as suggested. The caption has been updated.

3. p. 132, line 10. does the model use relative humidity and cloud cover from ECMWF forcing? What about river runoff?

* Concerning ECMWF forcing some information was actually missing. The text has been modified as:

“The model is forced by ECMWF 6-hourly operational analyses, namely 10-m meridional and zonal wind components, 2-m air temperature, relative humidity and cloud cover. These variables are used interactively in the surface heat flux calculation. Precipitation and river runoff are not explicitly included, but implicitly taken into account, on average, by relaxing surface salinity to the MED6 climatology (Brasseur et al, 1996; Fichaut et al., 1998; Brankart and Pinardi, 2001).” [Page 5, lines 3-8]

The relevant reference has been added to the list.

4. There is a miss-match between Fig. 1 and Fig. 13b. Is this due to that Fig.1 assimilates observations while Fig. 13b is an OSSE? If so, this means OSSE results on the salinity in wintertime may only be trusted to a certain extend. Please give more explanation on this.

OSD

3, S258–S265, 2006

Interactive Comment

* Figures 1 (I am considering curve BTS only) and 13b are both OSSEs but represent quite different experiments: a) The assimilation involves different data sets (MFSPP data for Fig. 1 vs. MFSTEP data for Fig. 13b); b) assimilation is made in filter mode (Fig.1) vs. smoother mode (Fig.13b); c) Fig. 1 refers to the whole Mediterranean Sea, while Fig. 13b to the eastern basin only. The description of what Fig. 1 displays could be found at Page 132, lines 23-29, but, perhaps, is not clear enough.

In order to make the difference clearer, Section 2 (“Methods”) has been split into two sections, namely 2.1 “Twin experiments setup” and 2.2 “From univariate to bivariate data assimilation”. Section 2.2 contains the paragraph from Page 132, line 21 to Page 133, line 14, with few changes based also on a comment by another Referee, consisting on the definition of sig_a and sig_f, that was erroneously provided later in the paper:

“The application of multivariate data assimilation enables to overcome a limitation that affected the sampling strategy assessment performed in the MFSPP (Raicich and Rampazzo, 2003), where only univariate temperature assimilation was available. Figure 1 displays a comparison of daily winter salinity relative errors, i.e. the ratios between the assimilation run error (“sig_a” in the figures) and the free run error (“sig_f”), for the whole Mediterranean Sea obtained in runs with univariate temperature assimilation (UT), bivariate temperature assimilation (BT) and bivariate temperature and salinity assimilation (BTS). For this comparison the assimilation setup is different from that described in Sect. 2.1: a) The data set consists of profiles along all the VOS tracks of the network adopted during the MFSPP, extensively described in Raicich and Rampazzo (2003); b) data assimilation involves T and S only and is performed with a 7-day cycle using only past data (filter mode). In the UT run salinity changes are the result of the model rearrangement induced by temperature correction. In the BT run temperature is still the only observable available for assimilation, but salinity is also assimilated, after being estimated by means of the vertical EOFs, and corrected. Finally, in the BTS run both temperature and salinity are explicitly assimilated and corrected. From Fig. 1 it is

OSD

3, S258–S265, 2006

Interactive Comment

evident that BTS performs much better than UT and BT (that are similar) in both L1 and L2, with a relative error reduction of about 20%. In L3 (not shown) the relative error reductions for UT and BTS are similar and both less than 10%, while BT does not exhibit any improvements over the free run. The latter fact may be due to an inadequate EOF estimate of salinity, which is statistical and may not represent well the water column conditions of the specific time when the experiment is performed. The same analysis for temperature shows that, although the relative error reductions in the three runs do not differ much from each other, temperature benefits from the explicit assimilation of salinity, particularly in the western basin." [Page 6, line 25 - page 7, line 16]

5. Method BT may not be a good technique to be used in OSSEs, since in Fig.1, BT results are worse than UT in L1 and L3.

* Although the problem has not been fully understood, a possible explanations was already suggested at Page 133, lines 9-11: "The latter fact may be due to an inadequate EOF estimate of salinity, which is statistical and may not represent well the water column condition of the specific time when the experiment is performed." The text has not been changed.

6. In Fig. 3, it is not easy to distinguish tracks 5b and 6,7a,7b.

* Captions already included this information. However, to make figures better readable, the information on parameters, regions and seasons has been written above the upper panels in figures 8-9, and also Fig. 12-13.

7. The captions of Fig. 8-9 should mention if the figures is for temperature or salinity.

* Captions already included this information. However, to make figures better readable, the information on parameters, regions and seasons has been written above the upper panels in figures 8-9, and also Fig. 12-13.

8. p. 140. line5, LIW, please give its full name.

* LIW was intended to be defined at Page 132, line 1, but the acronym was missing.

The mistake has been corrected. [Page 4, line 25]

9. Ticks in Fig. 12-13 (i.e., the notation for different types of lines) are wrong, please correct them.

* I do not understand what is wrong in the legends (is this what the Referee means ?), however I realized that the curves can be confused with each other. Colours have been used instead of different line styles in Fig. 12-13, and also Fig. 8-9.

Interactive comment on Ocean Sci. Discuss., 3, 127, 2006.

OSD

3, S258–S265, 2006

Interactive Comment