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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 #2 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"

1) We could however regret that very little information is given about the way the initial conditions for the assimilation run are obtained. Figure 10 (commented page 139) is indeed the only piece of information. The author should give details about the added noise insofar as the way the mesoscale dynamics is perturbed seems to have a crucial influence on the succss of the assimilation.

* This information was provided at page 134, lines 14-19. For all the runs initial conditions are extracted from an interannual run with XBT assimilation and SST flux correction. The difference between the initial conditions of the free/assimilation run and the control is the date. No noise or other pertubations are involved. 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) The author should also give some hints about the limitations of the approach:

* At the end of Sect. 6 the following text has been added to comment on this problem:

"It should be remarked that the approach followed in this work exhibits some limitations,

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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 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." [Page 15, lines 10-20]

3) We could indeed wonder if the performances of the assimilation scheme are very dependant on the initial conditions. The author should discuss this point and, if possible, give some proof of the generality of its results. If affordable in term of numerical cost, an Ensemble technique such as the one proposed by Evensen (94) could be pertinent: its applicability and/or necessity should be discussed at least for future works.

* The results of this work consist of comparative, not absolute, assessments of different sampling strategies. The effect of different initial conditions was explored. They affect the RMS error, but if results are analysed in terms of standard deviations rather than RMS errors (see answer 5 below) the effect of initial conditions is mostly removed.

Until now, ensemble technique has not been used, mainly due to insufficient resources, therefore, it is difficult to assess possible advantages with respect to the approach followed in this work.

At the end of Sect. 6 the following text has been added to comment on this point:

"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

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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 21-26]

4) The "undesired correction" obtained for tracks 2b and 3 together with the very small impact of the assimilation of track 4 could besurprising:

* Reference to track 2b was erroneous at this point. Only track 3 should be mentioned. Sorry for the mistake. [Page 10, line 9]

4aI) [p 138 lines 17-19]: What is meant by "OI parameters"?

* I agree that "OI parameters" is too generic. The sentence has been modified as:

"A more adequate choice of the forecast error covariance radius, for instance spatially variable, can probably reduce this problem." [Page 10, lines 14-16]

4aII) Are the Eofs used for the order reductions really adapted?

* EOFs depend on the season and the region. In this sense they are adapted. In order to clarify the point, the sentences at page 133, lines 18-21 have been modified as:

"In the TS cycle the assimilation of temperature and salinity profiles is performed by means of vertical bivariate EOFs estimated on regional and seasonal basis from a 1993-1999 GCM run. The technique used for the estimation and the region and season definitions are outlined by Sparnocchia et al. (2003)." [Page 5, lines 21-24]

4aIII) Is their total number (20) sufficient? * The sentence at page 133, line 21 has been modified as: "The first 20 EOFs are used, accounting for more than 99% variance." [Page 5, lines 24-25]

4b) Could it be that another strategy for perturbations would give different results?

* No perturbations have been introduced (see also answer 1 above).

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4c) Which conclusions should be drawn from the present result for these tracks? Does this mean that this observation strategy cannot efficiently correct errors in the characteristics and/or position of the water masses?

* The result is that, under the conditions of the present numerical experiments, the inclusion of track 3, when tracks 2 and 4 are operational, makes the strategy less effective, but see also answer 4aI above.

5) p 131: The author chose to study standard deviation (as for the pilot project OSSE), In a previous paper, the author reported that the evolution of the “RMSm" (for mean RMS) was sensitive to the initial conditions. This point should be discussed in the context of the “multivariate OSSE” by indicating at least if the same comments can be made.

* RMSm (from Raicich and Rampazzo, 2003) is not “mean RMS” but rmsm (see below). The sentence at Page 132, lines 16-19 has been replaced by the following text, to clarify this point:

“If, in a given spatial domain, we consider two n-dimensional samples of the assimilation run (ai, i=1,...,n, the same concept applies to the free run) and the control run (ci), let ma and mc be their arithmetic means over that domain and a’i = ai - ma and c’i = ci - mc the anomalies relative to those means. The relationship between the root-mean-squared difference rms and the standard deviation sigma is given by rmsˆ2 = rmsmˆ2 + sigmaˆ2, where rmsˆ2=1/n*sum(ai-ci)ˆ2 , rmsmˆ2 = (ma - mc)ˆ2 , rmsˆ2=1/n*sum(a’i-c’i)ˆ2 Thus, rmsˆ2 is the sum of a quantity (rmsmˆ2) depending on the means and one (sigmaˆ2) depending on the anomalies. sigma is adopted instead of rms, since, as discussed in Raicich and Rampazzo (2003), it is more sensitive to data assimilation. Clearly, the same concept applies to the free run.” [Page 4, lines 8-17]

6) p 133 : The reasons why the author chose two 7-day assimilation cycles separating T-S and SLA are not clear (to me).

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* The choice was motivated by the unavailability of adequate multivariate EOFs for both purposes at the same time. As explained at page 133, lines 26-29, SLA assimilation is performed via a unique trivariate EOF (barotropic streamfunction, T, S) for the whole basin, defined below 120 m and only when the water column is deeper than 1000 m. TS assimilation makes use of EOFs that vary regionally and are defined also in shallower areas. The following comment has been introduced after Page 133, line 29:

"The choice of splitting TS and SLA assimilation is motivated by the fact that SLA is mainly affected by TS variability below the mixed layer, while CTD and XBT profiles provide water column properties also within the mixed layer. This splitting was adopted also by Demirov et al. (2003) in their "analysis" scheme, to which the scheme used in this work is similar." [Page 5, line 33 - page 6, line 3]

7) p 133: Was the sensitivity to the number of Eofs (20) tested? Is this number based on physical background or on numerical cost? Are these Eofs dependant on the region?

* See answers 4aII and 4aIII above.

*** Answers to "Minor comments"

a) Page 131 Line7: the true ocean (...) provides temperature "andsalinity" data...

* The Referee is right: the words were missing. Corrected. [Page 3, line 30]

b) Page 134 Line3: the data "errors" are uncorrelated with each other.

* The Referee is right: the word were missing. Corrected. [Page 6, line 6]

c) Page 137 Lines17-19:

I) the definitions of sig_a and sig_f should be given with the presentation of Figure 1 (page 132). Indeed the ratio sig_a over sig_f is plotted on this figure.

* Concerning the definition of sig_a and sig_f, the Referee is right. The sentence at page 132, lines 23-25, has been modified as:

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"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 ..." [Page 6, lines 27-29]

Note that, following a comment by another Referee, the paragraph has been moved to the end of the section and defined as Subsection 2.2.

II) The author should also briefly explain why temperature is shown for the eastern bassin and salinity for the western bassin.

* As written in the text, those are examples. Moreover, in Sect. 4 comments are also made for the parameter that are not shown. The complete set includes 2 other figures besides Fig. 8-9. My original idea was that 2 other figures are too much, since they do not add crucial information, but I can add and comment them, if the Editor thinks it necessary. By the way, the same concept applies to Fig. 12-13. The text has not been changed.

d) Page 143 lines 20-24: the north-western Mediterranean is also a region with complex dynamics (northern current...) although tracks 2b, 3 or even 4 do not have a positive impact on the assimilation...

* Concerning track 2b, it was mentioned by mistake (see answer 4 above). In each experiment only one track is removed, therefore the results should be interpreted in a relative sense. Tracks 3 and 4 are actually less effective than track 2, which is the most effective one and crosses the Algerian Current region. This does not mean that the Liguro-Provençal-Catalan Current is not a highly variable system in space and time, but its variability is not dominant with respect to the region crossed by Track 2. The text has not been changed.

*** Answers to "Figures"

f1) Figure 1: the label for the vertical coordinate has not been defined. It is defined at the beginning of section 4. The indications for L1 and L2 (as given for Figure 8) should

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be added to the caption.

* Concerning the definition of the vertical axis label, see answer to minor comment c above. The caption has been corrected as suggested.

f2) Figures 3, 4, 5 and 6: labels and ticks are missing for latitude and longitude.

* They have been included.

f3) Figures 8, 9, 12 and 13: the curves are rather difficult to distinguish. The author should use some other plotting methods.

* Curves have been colorized to better distinguish them.

f4) Figures 12 and 13: unlike the previous figures (a) is temperature and (b) salinity... which is misleading as all other figures are the other way round.

* To make figures better readable, the information on parameters, regions and seasons has been written above the upper panels in figures 12-13, and also figures 8-9.

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