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Interactive comment on "Forecast skill score assessment of a relocatable ocean prediction system, using a simplified objective analysis method" *by* Reiner Onken

Anonymous Referee #2

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The author presents a numerical study, based on a Relocatable Ocean Prediction System (ROPS) and in situ hydrological measurements west of Sardinia. The data assimilation method is Optimal Interpolation, the assimilated data are temperature (T) and salinity (S) The main goal is to assess the global forecast skill for T and S at time scales of several days. Another objective is to evaluate the sensibility of the forecast skill to parameters of the assimilation system.

I appreciate the very didactic way the assimilation platform is described which allows a clear understanding of most of the system components and of the implementation efficiency . The emphasis is put on pragmatic issues (relocatability, calculation possible

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Discussion paper

on a laptop, etc.) and this provides a clear and consistent conducting line through the paper. The experimental protocol for each series of tests is well explained and justified. The paper is well written.

My main concerns relate to the two following issues:

1/ the analysis of the results lacks from physical interpretation in terms of circulation processes. The latter would allow a better understanding of 'what the assimilation is effectively doing' and therefore of the results of the sensitivity tests. Even though the goal is to evaluate the performance of a relocatable system, I believe the evaluation process cannot be done without considering the specific dynamics of the study area.

2/ I do not understand how the vertical grid is handled. On which vertical levels is the OA performed: on the ROMS grid levels or on constant depth levels ? The verification method is based on spatial averages of the RMSE at different levels (eg figures 8 to 11): are these levels the ROMS levels (implying that the RMSE for different depths are spatially averaged) ? There is an absolute need to clarify these points; I believe a graph would greatly help.

The paper can be published provided that the two issues above are addressed. No extra calculation is required. I therefore recommend the author to address these issues (and see other remarks below) and then to resubmit the manuscript.

For issue 1, I suggest the following:

- make clear in your introduction what are the specifics space and time scales of variability that this study is targeting.

- add a short paragraph introducing the main circulation patterns of the study area over June 2014

- use this information to discuss or justify some choices or hypothesis: for instance synopticity is assumed for the ScanFish observations over 60 hours while it is found in series C results that data within a 42 and 48h window are too old or too far in the future

to be consistent with the model forecast at the central time.

- add a comment in the discussion on the fact that you do not distinguish the shelf and deep region, although these areas are characterized a priori by different scales of variability. If this is not the case in this specific Mediterranean area in June 2014, it needs to be stated. The differences in dynamical regimes are likely to explain some results on the behavior of the assimilation.

Other remarks:

- Series D: as the distribution of the assimilated data is not stationary in time, can this influence the results? For instance, leg 1 and leg 2 do not have exactly the same sampling pattern nor the same density of observations at the same location (if I understand well figure 3).

- Series D: the skill is relatively low for short forecast range with respect to longer forecast range in both assimilated and free runs: could this be due to errors at short time scales on the atmospheric forcing at the period of the verification (around June 22)?

- As T and S are assimilated independently from each other and since the assimilation is performed independently at each level (as far as I understood) there is no constraint on the water masses. A T/S diagram, for the free run versus the obs and versus the assimilation run would allow to check that new unrealistic water masses are not created by the assimilation

- Section 5 (p14, l22-23) 'it is the massive amount of assimilation data which desequilibrates the terms of governing equations of ROMS ..'. The errors on observations are supposed uncorrelated: is this hypothesis valid with such a number of data ?

Minor revision points

About the choice for the correlation: Please indicate the correlation function for the spatial correlation.

Section 3: please indicate the max depths of the profiles from CTD, gliders and Scan-Fish measurements.

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