

## ***Interactive comment on “Observed and Modelled Mixed-Layer Properties on the Continental Shelf of Sardinia (Mediterranean Sea)” by Reiner Onken***

**Anonymous Referee #2**

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### General comments

The paper by Onken is a large sensitivity study for a numerical setup off the Sardinian coasts (Western Mediterranean Sea). Sensitivity is evaluated with respect to initial and boundary conditions, atmospheric forcings and parameterizations for vertical mixing schemes. The paper deserves to be published as the observational effort to validate and assimilate in the model is impressive: a mooring (with an ADCP, a CTD, 4 RBRs and 40 high-resolution temperature recorders), more than one hundred CTD casts taken by two R/V vessels (Alliance and Planet), deployment of 11 gliders and the use of a ScanFish. However, I do have major and minor critical points (see specific comments below) that I believe should be addressed by the author in the review process.

Specific comments: major concerns

C1

a) My first main criticism of the paper is the following. One would expect such a large observational dataset to be used in a state-of-the-art assimilation scheme. As acknowledged by the same author at pag6 (L30-31), ROMS includes a 4DVAR state-of-the-art variational assimilation scheme which can be directly used for sensitivity studies via the analysis of the adjoint of the model. In this work the author uses an Objective Analysis method stating that the variational method is computationally expensive. I believe that at least a quantitative measure of this statement should be provided, considered that in the paper I counted at least 24 different runs: if one run is the same in each subset, we have 3 different initial conditions (A1-A3), 5 different vertical grids (B1-B5), 3 surface conditions (C1-C3), 4 vertical schemes (D1-D4), 14 background vertical diffusivity values (E1-E14). The author should really show that running 24 forward runs is less computationally expensive than running a few simulations with a 4DVAR scheme and perform an analysis of the jacobian matrix for the sensitivity assessment. The author should be quantitative and provide values/estimates (core per hours?) for both the 4DVAR and the OA simulations.

b) My second main point is about the choice of the large horizontal eddy diffusivity and viscosity values in the paper. It is my understanding that a horizontal viscosity value of 50 m<sup>2</sup>/s is used throughout the manuscript while only the diffusivity is reduced from 10 to 1 m<sup>2</sup>/s in the final section 6 of the paper. All these numbers look large values to me especially because ROMS can be run relying on just implicit viscosity/diffusivity and for such a fine grid resolution (1.5km x 1.5 km): what are the estimated grid Reynolds and Peclet numbers? The author should justify this choice because all simulations may be too sluggish and strongly undermine his results.

c) My third main point is that there is no comparison/discussion on other observational experiments performed in the past aimed at assimilating similar fields. Important paragraphs citing previous works in the literature should be inserted both in the introduction and the discussion/conclusion section.

d) My fourth main point is that methodology in general should be better described.

C2

Please refer to specific points 6, 7, 8, 9, 10 and 11 below.

Specific comments: minor concerns

- 1) Pag3, L1: "...west of Sardinia." Maybe here a reference to Fig2 could be provided?
- 2) Pag4, L5-19: Section 3 is methodological while this whole paragraph describes more results. I would move it at the top of section 4. This also helps with the big leap in following Fig.10
- 3) Pag4, L11: ok  $\Delta T = 1 \text{ degC} = 1 \text{ degK}$  but I would stick to Celsius through the whole paper (see also many points below)
- 4) Pag4, L21-22: not clear, maybe rephrase as "here only those casts taken during the 7-11 June period were used".
- 5) Pag4, L24: please calculate and provide an estimate of the Rossby radius from the many CTD casts you have available.
- 6) End of Pag4: there is no description about the CTDs, how they were calibrated, used probes and sensors, etc. The gliders' description (Slocum?), their sensors, is missing as well.
- 7) Pag5, Sec3.2: This section should include the total number of numerical experiments and a reference to a Table were all simulations are summarized.
- 8) Pag5, L22: please provide values for  $r_{x0}$  (h-parameter) after the bathymetry was smoothed. The  $r_{x1}$  parameters for all simulations should also be listed in the table of point 7 above
- 9) Pag6, L27: a discussion about the differences between the two COSMO products is missing. For example, winds from the two products shown in Fig10 are very different after June 14. I am also surprised that winds in the period June 14-20 are less strong in the finer product than in the coarser one.

C3

- 10) Pag7, L1: the description of the OA method is left to these two dated reference. Please provide a better description with formulae
- 11) What about the sensitivity of OA to the parameters  $W$  and  $C$ ? I am asking because the isotropic correlation is a strong assumption especially in areas close to the coast where across and along-shelf dynamics may differ the most.
- 12) Pag7, L23: not clear, maybe rephrase as "The purpose of this section is the investigation of the impacts of:"
- 13) Pag8, L9: not sure I understand, why lack of salinity measurements? Maybe you do not have them at high-resolution as temperature but you do have 108 CTD casts!
- 14) Pag8, L9: Once again set  $\Delta T$  to degC
- 15) Pag8, L9: why did you use this method and not the maximum vertical gradient as for example in Fig10?
- 16) Pag8, L18: a Table summarizing all experiments is definitely needed (see point 7 above)
- 17) Pag8, L25: degC not K
- 18) Pag9, L26-29: not clear and not really able to grasp what the author is trying to say here. Could you rephrase and expand?
- 19) Pag11, L10 and L14: "much better" not so evident to me. Please provide a quantification for this statement. "Qualitative" criteria as at L14 are not acceptable
- 20) Pag12, L7: I read the paper more times but I am missing why the 0.81m depth was chosen wrt other depths
- 21) Pag12, L22: please define the hatted variables via formulae
- 22) pag12: L29 and L32: not sure to follow the argument here, isn't the 18.7h peak the inertial period?

C4

23) Pag13, L24: not clear is this the first or the second vertical gridpoint where scalar (rho-grid) are defined?

24) Pag13, L29: this statement is really important as I am concerned that your simulations are too viscous to be realistic (see main point b above).

25) Pag14, L4: "...almost perfectly". I believe this is an overstatement. What about simply say "reproduced well the observed one"?

#### Figures

26) The meridional and zonal extensions of Figs 2, 3 and 4 should be exactly those of Fig5 to better orient the reader

27) Fig8a, 12a, 14a, 17a and 20a: change label and express deltaT in degC

28) Fig9, 13, 15 and 18: x-axis label, express deltaT in degC

29) Fig19: y-axis label, express deltaT in degC

30) Fig20: panels a1 and a2, change degK into degC.

31) Fig21: please indicate inertial frequency on both a1 and b1 panels

#### Technical corrections

32) Pag4, L2: turnS out

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