**Ocean Science
Discussions**

Interactive comment on "Assimilation of SLA along track observations in the Mediterranean with an oceanographic model forced by atmospheric pressure" *by* S. Dobricic et al.

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Reply to anonymous referee #2

We would like to thank the reviewer for his valuable comments. Here are the replies to the comments:

The authors compare ATPR1 versus CONT1 and ATPR2 versus CONT2. It is rather intuitive that ATPR1 is more accurate than CONT1 and that CONT2 is more accurate than ATPR2 because they have better agreement between model and obs. But, the main question is if ATPR1 is superior to CONT2 as most systems nowadays use CONT2. If this were not the case, it would also be a valuable result. This statement

appears in the conclusion (page 1588 line 5-13), but seems to me not to have been demonstrated. The authors should include a summary table where the overall RMSE of each experiments are reported (in units or percent) for SLA, Temperature and Salinity profile together with the significance of the improvements. These numbers should be used in the abstract and conclusion instead of holding vague statements.

Reply: Now we show details of the differences between RMS of residuals and more clearly demonstrate that ATPR1 has the highest accuracy. (Section 3.2, the first two paragraphs)

It may be interesting to include the spatial distribution of the relative RMSE between CONT2 and ATPR1 and compares it with Fig2.

Reply: The revised manuscript includes Fig. 6 that shows the spatial distribution of differences between RMS of SLA residuals. The discussion is given at the end of the first paragraph in Section 3.2.

Page 1587, Line 14 (These SLA difference. . . "). I could not find where this statement has been demonstrated?

Reply: In the revised manuscript we indicate that this statement is demonstrated in Section 3.1. (The first paragraph in the Discussion)

Although Fig 7 is interesting, the authors should focus on the topic of the paper. If differences between ATPR1 and CONT2 are found, this would be relevant for the paper otherwise I am afraid that this is off-topic. The last sentence of the Abstract (From line 10), implicitly suggests that correct spectrum can only be achieved with ATPR1.

Reply: In the revision we try to more clearly justify the inclusion of this figure. It demonstrates that it is not necessary to make the filtering of the observations in the preprocessing, because even condsidering the energy power spectrum the model used in the assimilation is able to correctly filter the information in the observations. (The third paragraph in Section 3.2 and the second paragraph in Discusion)

Page 1579, Line 16, I do not consider the citation to the paper relevant and suggest removing it.

Reply: The citation is included, because it is an in situ estimate of the power density spectrum that corresponds well with the theory. Although it was measured in the atmosphere it should be also relevant for the ocean.

Page 1580 Line 1 More details about the filtering method are needed. Some information are found in Page 1581 Line 5-11. It seems to me that the most problematic aspect of CONT2 is that the model contains the high-frequency response from the wind, while this signal is filtered out in the observation by the "high frequency correction obtained after the application of a barotropic model forced by high frequency winds and atmospheric pressure". This results in some inconsistencies between model and observations, which is not the case in ATPR1. Is this correct?

Reply: We would like to thank the reviewer for this important comment regarding the omission of considering the impact of wind forcing. In the revised manuscript we discuss the impact of this assumption in the third paragraph in Discussion.

I recommend the authors place their study in the broader context. What is the particularity of the domain studied, for the problem of including atmospheric pressure and assimilating non-filtered data? What is the impact of their data assimilation setting on the results (in particular the following adhoc setting: "mean residual is subtracted from the residuals along each satellite track").

Reply: The particularity of the model set-up in the Mediterranean is discussed in Introduction. In the revised manuscript, in the last paragraph of Discussion, we add a short discussion that suggests that the results of the study should be valid mainly for model set-up in semi-enclosed seas like the Mediterranean.

In Fig. 6, it is hard to see anything. I would recommend the authors to zoom in the Figure and add a legend to the Figure.

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Reply: We have added the relative change in the figure in order to make it easier to see the differences.

The paper is not well structured. For example in Section "Data assimilation system", model and data assimilation must at least be separated by a paragraph. Changes of the assimilation method depending on the experiments are described before the experiments are themselves described. I would suggest separating observations, model and data assimilation method.

Reply: We have restructured this section according to the suggestions of the reviewer. We have however left the description of the method in this section before the description of particular experiments, because it is a more general description, while experimental set-up is described in the section that also presents the results.

The data assimilation method is poorly described. What is the e-folding radius of the Gaussian function? A more detailed description is expected. Why the EOF are not used in the horizontal ? Line 11, do you mean the baroclinic velocity ? How many iterations are used or which criteria is used to stop iteration? A scheme would help understanding how the method is working.

Reply: In the revised manuscript we give a more detailed description of the data assimilation scheme with information on the radius of correlation, number of iterations, reason for not using EOFs in the horizontal etc.

Page 1582, Line 17. It seems that the MFS uses a different MDT than the one used in observations (CNES 2009). Why, and what impact does the authors expect?

Reply: We do not expect any impact from the different MDT. It is the same in all experiments. In the revised manuscript we state that all parameters of the data assimilation scheme have the same values in all experiments and we assume that there are no non-linear interactions that could impact the results (the first paragraph in Section 3.1).