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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.

Anonymous Referee #2

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As a heritage from a level-4 product, standard Level-2 SLA products from SSAlto/duacs are still corrected/filtered for tides, inverse barometer, tropospheric, and ionospheric signals. Models that assimilate this data do not include the response of atmospheric pressure in order to be in consistent with observations. A new prototype of SLA product called TAPAS provides in addition to the filtered data, the separate corrections for tides and HF signal. This gives the opportunity to include the inverse barometer in the model and add the correction for inverse barometer and high frequency back in the observations. The authors attempt to quantify the impact of such approach in a realistic forecasting system of the Mediterranean Sea for year 2009.

This study is very interesting as altimetry is the data type that best controls model dynamics. In this respect, I consider this study worthy of being published in Ocean Science but I recommend the authors to respond to my comments before it may so.

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

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

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.

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

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?

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").

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.

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.

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.

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?

Interactive comment on Ocean Sci. Discuss., 9, 1577, 2012.