**Ocean Science
Discussions**

Interactive comment on "A nested Atlantic-Mediterranean Sea general circulation model for operational forecasting" *by* P. Oddo et al.

P. Oddo et al.

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Anonymous Referee 2
Manuscript "A nested Atlantic-Mediterranean Sea general circulation model for operation forecasting" investigates an effect of Atlantic Ocean boundary conditions on the circulation in the Mediterranean Sea. Two different boundary conditions for the regional Mediterranean model are considered. In the first, temperature and salinity are relaxed to Levitus climatology using nudging and sponge layers. In the second, Orlanski type radiation boundary conditions are used to nest the Mediterranean model within the climatological global model. Careful analysis of the effects of boundary conditions

on temperature, salinity and surface elevation is presented. Model results are then compared to the observations obtained from ARGO floats, satellite altimeter and tidal gauges. Results indicate that the second set of boundary condition is superior in terms of salinity and surface elevation, both in mean and seasonal variability. Overall, the paper reads well, it constitutes a solid body of work, and it deserves being published with minor revisions.

In particular,

1. Page 1094: in addition to the description of the regional dynamics it would be good to a have a schematic of it. It makes it easier to follow the consequent analysis.

We agree with the Reviewer that could be good to have a picture with a sketch of the Mediterranean circulation. We have tried to add this features in Fig01 but then it was confusing. Since most of the papers referenced in the introduction have similar figures and the number of figures in the present manuscript is already large we preferred to omit this info.

2. Chapter 2: Please indicate if there are any tides in the model. I presume not, but it would be good to confirm it, especially in view to the future comparison with tidal gauges.

The following sentence has been added at 1098:11

"tidal dynamics is not implemented in both MFS-V1 and MFS-V2"

3. Chapter 2: Description of the advection scheme in the new model is a bit confusing. Is it upstream at the passages and MUSCL elsewhere?

We disagree with the reviewer, the manuscript states:

"The up-stream scheme is used in proximity of the river mouths, in the Gibraltar Strait and close to the Atlantic lateral boundaries."

What is the reason for switching from simple "centered 2nd order" scheme to the elaborate mixed scheme?

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The following sentence has been added to justify the choice of the new advection scheme:
"This flux-limiting scheme is particularly suitable for operational purposes not only because it is able to preserve gradients without significant numerical noise, but also because it has the capability to switch, without additional computational cost, to a simple up-stream scheme in areas where numerical instabilities can occur."

4. Page 1097 line 2 has a typo. Should be "centered" instead of "cantered"
Modified.

5. Page 1097 line 15: please specify whether yearly or monthly Levitus climatology is used in MFS-V1. A reader is left wondering about that until you mention it in the Conclusion.
Text modified (monthly).

6. Please specify how many years of MERCATOR output are used to construct model monthly climatology that is used as boundary conditions in MFS-V2.
Information added.

7. Chapter 3: Can you elaborate a little about why temperature is less sensitive to boundary conditions than salinity.
In Chapter 3 we have analyzed the differences between the two simulations and then, in section 4, investigated about the driving mechanism. In Section 3 pg1100:10 is stated: "The reason for this is clearly connected to the different inflow of Atlantic Water and it is explained below."
And then in Section 4.1 pg 1104:24
"This is due to the fact the water (and salinity) surface fluxes in the two model imple-

mentations are different by the volume preserving correction factor. The correction factor performed to preserve the volume in the closed simulation produces on average a dilution of the surface Atlantic waters."

However, in agreement with Reviewer's comment we modified the sentence on pg1100:10 anticipating the statement then explained as follows:

"The reason for this is clearly connected to the different proprieties of the inflowing Atlantic waters, which are due to the volume preserving factor applied in MFS-V2.1, as explained below."

8. Figure 3 is a little confusing. There are results from two model simulations there, compared to the climatological curves. But which of the curves are obtained from which of the MFS models is unclear.

Reviewers is right, figure caption has been modified as follows:

"Fig03. Top panel: Time series of Total Heat Flux. The grey line indicates climatology from NCEP; solid markers indicate models climatology (averaging 4-years run); solid thin line indicates 10-day average inter-annual values from model simulations. Bottom panel: Time series of Total Water flux (E-P-R). The grey line indicates climatology from Mariotti et al. (2002); solid markers indicate models climatology (averaging 4-years run); solid thin line indicates 10-day average inter-annual values from model simulations. In both panels, climatological and inter-annual values from MFS-V2.1 and MFS-V2.2 overlap."

9. Page 1100 line 1: a typo. Instead of "that" should be "than"
Text modified accordingly.

10. Please mark months on Figure 6, it is difficult to follow your analysis of month-by-month changes without it.
Labels of the X axis modified.

11. Chapter 4.1:
although MFS-V2 shows an improvement over MFS-V1 in temperature and salt at the surface, the skill of MFS-V2 deteriorates with depth, and is worse than MFS-V1 below 500-600 m. Could that be because of bias in climatological MERCATOR fields used in MFS-V2, as opposed to a bias-free boundary conditions in MFS-V1?
Data used for the nudging in MFS-V2.1 and for lateral open boundary condition in MFS-V2.2 are the same as explained in the text and in table1 and we make this point clearer in Table1. (See also answer to the following Reviewer comment)

12. Page 1104, line 10: I do not buy an argument that the lack of significant improvement in pattern correlation is due to a fact that it is already good in MFS-V1. There are quite a bit of analysis of vertical structure of PCC curves that can be added. PCC is a good indicator of how well mesoscale and small scale activity is simulated. For example, no improvement at the surface can indicate that all the patterns there are locally forced and do not depend on the boundary conditions. Improvement at 100-200 m – does it mean that MFS-V2 is better in dealing with thermocline?
We thank the Reviewer for this important comment. Comment on PCC has been modified accordingly.

Again, deeper portion in MFS-V2 is worse, leading me to suspect that MERCATOR seasonal cycle is worse than that of Levitus. It is something which is worse investigating.

We disagree with this possible explanation because data used to drive both model configurations are the same (see answer and action to previous comment 11). It is possible that different treatments of lateral boundary conditions (relaxation or the

more complex used in the V2.2) have some effects in the Mediterranean deep layers. Another possible explanation of the observed worsening in the deeper layers could be related to the vertical mixing parameterization, which maybe requires further tuning having now better reproduction of water masses characteristics. Future investigations will try to answer this question.

13. Chapter 4.2: please show locations of Envisat and Jason-1 altimeter tracks that are used in the analysis. Are you using all of the altimeter data, or you discard the data that is contaminated by the proximity to the land?
Due to the period spanned in the analysis (4 years) plotting along-track sampling points will results in figures difficult to be understood. So we prefer to do not add new figure in the manuscript. Data close to the land are rejected or corrected before the final product we used is delivered. In order to answer the Reviewer's question the reference to "Pujol and Larnicol 2005", where a brief explanation on the data quality control is given, has been added in the text.

14. Please specify what do you do with the tidal signal in tidal gauge data? This also relates to the issue raised in 2.
"As further evaluation of the surface elevation, model results have been compared with available tide gauges (cyan dots in Fig.01) data; observations have been averaged in time in order to remove tidal signal, model results have been sampled on the tide gauges positions."

Interactive comment on Ocean Sci. Discuss., 6, 1093, 2009.