Revision of "MESMAR v1: A new regional coupled climate model for downscaling, predictability, and data assimilation studies in the Mediterranean region" by Storto et al.(2023)

REVIEWER 3

We thank the reviewer for the encouraging comments and for the suggestions to improve the quality of the manuscript. Below, we provide a point-by-point reply (reviewer in bold, our answer in light font). All coauthors concur with the proposed changes. We refer to the revised version of the manuscript in answering the questions.

Minor Points:

In agreement with another reviewer, I would like some more discussion (in the Intro.) on why the system is different from previous regional models, and its specific advantages. There is a brief discussion at lines 379-383, but I would like it more explicit in the Introduction. For example, to what extent do other systems have data assimilation?
We have added a sentence on the specificities of our system, according also to Reviewer #1 question.

Line 17. It would help to specify the grid spacing for ocean and atmosphere (not just ocean) using the same units.
Corrected, thanks, as requested also by Reviewer #1

The paper has a slight bias towards the oceans (with less discussion of atmosphere processes). This is OK, but lines 39-43 ignore the fact that enhanced resolution may improve representation of atmosphere dynamics, not just ocean. Is one of the aims of your paper to identify when full coupling provides better results? You could make this explicit.
We reformulate this sentence according to Reviewer 1, pointing out that RCMs were originally only atmospheric models, and when coupled to the ocean a better representation of the air-sea fluxes is generally provided. So, we believe the reviewer's comment is important, but the new version of the paragraph overcomes this point.

Lines 56-57 and 66-67 both introduce medicanes. I suggest to delete the first example.
Yes, deleted, thanks

Line 101. Are the lateral bcs applied as some kind of nudging?
Exactly, added

Line 108 "partial bottom steps"
Corrected.

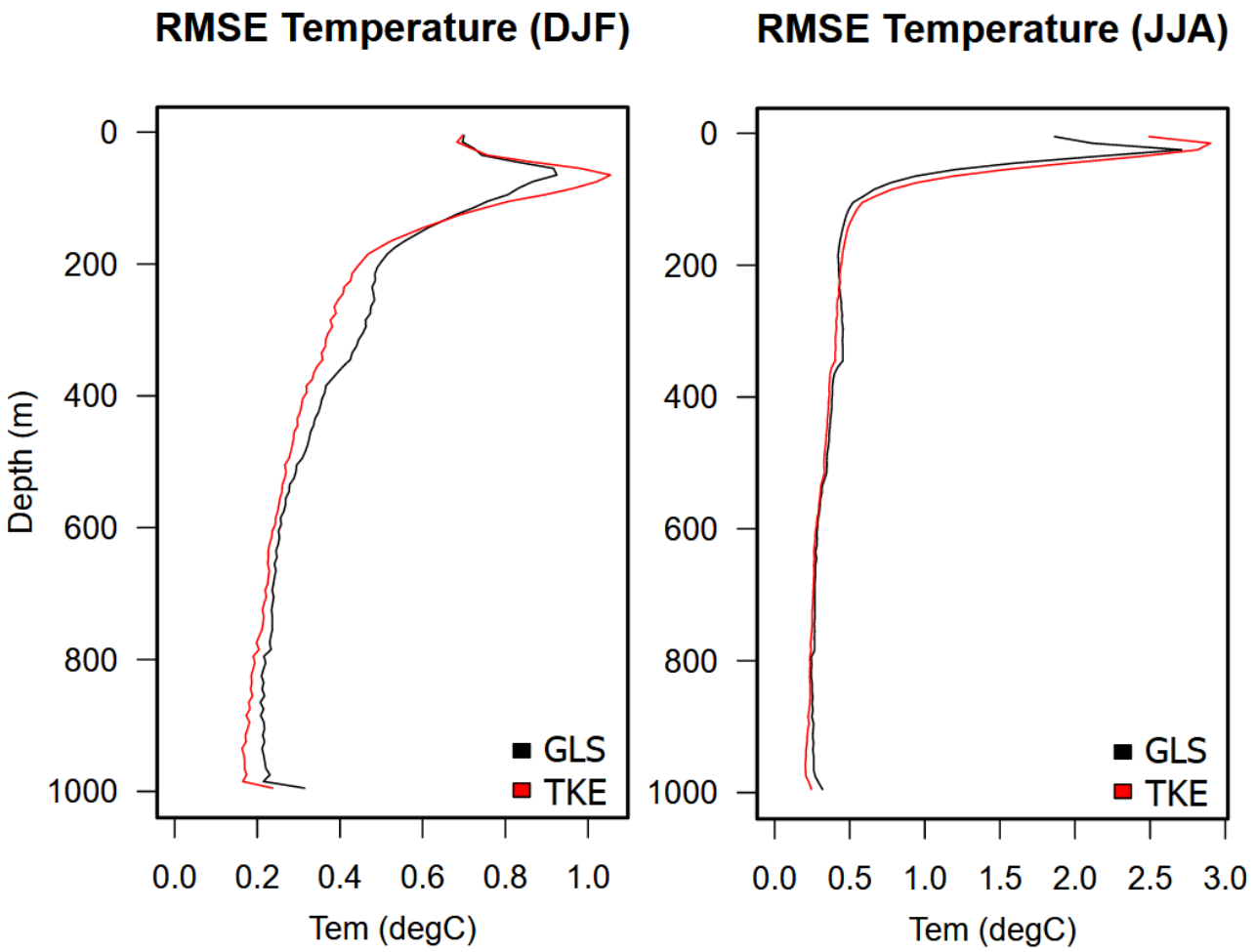
Lines 114-116. Is this the same as the GOTM (https://gotm.net/portfolio/) ?
Yes, however, we are not fully sure that the technical implementation of GLS in NEMO is exactly as in GOTM, so we prefer not to mention GOTM.

Line 177. 2 years seems a bit short for this comparison.
We agree and have made explicit in the manuscript that the results are partial.

Line 183. For Fig. 4, add a map of model minus EN4 surface salinity (probably annual mean).
Thanks for the suggestion, we added a panel accordingly (in Figure 3).

Line 193 introduces the TKE scheme in the wrong place. It should be described more fully in section 2.2.
Done.

Lines 205-210. Fig. 5 shows a strong seasonal dependence of bias, but Fig. 6 includes all seasons. Consider making the equivalent of Fig. 6 for winter and summer.
Thanks for the suggestion. We show here the figure and we comment it in the text, but the number of figures is already very large so we prefer not to add more figures.



As for Figure 6, but for the RMSE of Temperature in DJF and JJA.

Lines 270-275. Does the net air-sea heat flux difference between MESMAR and reanalysis relate to SST difference and turbulent fluxes, or to radiative fluxes?
We detail this at the end of the section: roughly 50% is due to turbulent fluxes (overestimated in MESMAR, ie greater heat loss) and 50% to underestimated incoming solar radiation.

Line 288. "climatological anomly"-> "anomalies from climatology"?
Corrected

Line 305. More definition of full-field nudging - I assume this means no length-scale filter, and similar timescales?
Added, thanks

Line 320. "SST skill scores" - SST is nudged to analysis on 15-day timescales, so i don't know how to interpret "SST skill"!

The effect of SST scores is discussed when commenting on Figure 14. Maybe it was not clear in the text: the SST nudging is switched on only in the “OC1” experiments, ie only when also variational assimilation of profiles is switched on. We clarified this in the text and table.

Section 5.2. I assume that the verification data was not assimilated. (Please state this explicitly.)
We added this in section 2.5

Fig. 14. As above, the system is nudged to SST, so how to interpret MESMAR minus satellite SST?
Not really, the experiments OC0 have no data assimilation so the comparison in the top panel has no nudging. In the bottom panel, instead, we want to evaluate the combined effect of all ocean observations (nudging and variational assimilation). We have clarified this in the text.

Line 357. CTL has Medicanes due to bcs?
They are not shown because the track is very far and the intensity is much smaller than in the other experiments. Please note, boundary conditions are far from the tracks, and with no data assimilation nor cycled initialization, CTL can represent medicanes only “statistically”.

Lines 362, 375 quote percentage improvements for ocean assimilation, how do they compare to **atmosphere assimilation? I understand that we expect ocean assimilation to have a lesser effect than atmosphere assimilation for atmospheric weather systems, but it might be useful to compare.**
AT0OC1 has indeed a similar representation as CTL. With no atmospheric data assimilation, there is no way to correctly capture the location of the medicane. This is already stated in the text; the question we wanted to investigate is whether the ocean DA adds value on top of the atmospheric DA. This is the case at least for the medicane intensity. We better clarified this in the text.

Fig. 11, consider adding two more panels of the differences: model minus reanalysis.
Added and commented on the text. Thanks

Fig. 12 - I think a lot of lines overly each other, e.g. BIAS of humidity, perhaps mention this in the text. i.e. the lack of sensitivity to changing configuration.
We now mention it in line 347.