Reply to Anonymous Referee #2

[Reviewer comment]
Review of GMD-2019-196 Vadseria et al. present a sequential modelling tool to investigate (paleo-)climate change effects on Mediterranean Sea circulation. They first explain their set-up and validate for the present-day. Then an example of application, the Early Holocene, is given. It seems like a valid approach that is indeed of use for multiple (paleo-) applications. I would however suggest revision to make the paper clearer, both structurally and with respect to what exactly the added value of their sequential modelling tool is.

[Reply]
We thank the reviewer for his/her constructive comments that help to improve our work. We have implemented all of them in the revised manuscript.

[Reviewer comment]
So my main comments are:
- structurally the paper can improve to clear up some unclarities. For instance, Fig. 2 states "hist-obs" while the text only mentions "hist". I guess you mean the same simulation. Also, many citations seem to be absent from the reference list.

[Reply]
We apologize for such confusions. We made the necessary correction accordingly.

[Reviewer comment]
- content-wise, the authors seem to claim that high-resolution atmospheric forcing is needed to get correct Mediterranean Sea circulation. This needs to be better substantiated by results or discussion. For instance, can you show that your simulation yields better results than, say, a OGCM run forced directly with AGCM forcing rather than ARGCM?

[Reply]
We try to demonstrate this point by using results from literature. Lebeaupin Brossier et al. (2011) showed that high-resolution atmospheric forcing was crucial in triggering the Mediterranean deep-water formation. Increasing the spatial resolution produces finer and more intense wind stress over the north western Mediterranean area. It also slightly modified the precipitating systems representation. Li et al (2006) also showed that the 50-km resolution in the atmosphere seems a threshold to induce the right Mediterranean overturning circulation.

[Reviewer comment]
Please find more detailed comments below, followed by the GMD review criteria.
P2, line 67 "the localization of the ... of debate": true, and actually your set-up would allow for testing separate forcing sources for sapropel formation (i.e. only adding additional freshwater to a certain location, or only precipitation versus only river runoff). This would make your model setup even more useful than using it for overall Med-Sea circulation under paleo-climate-forcings.

[Reply]

(All the lines mentioned hereafter refer to new version of the manuscript)

We agree that we may perform a series of sensitivity experiments to test the response of the Mediterranean overturning circulation to different forcings. Actually, we are working on the impact of different hydrological perturbations during the deglaciation on the Mediterranean oceanic dynamics. We hope to be able to present these new results soon. However, we want to keep our initial objective for this manuscript, to build a coherent modelling chain, able to go to detailed regional oceanic features from simulations with coarser-resolution global models.

[Reviewer comment]
P3 lines 73-77. Please provide section numbers when outlining the paper.

[Reply]
Done

[Reviewer comment]
P4 lines 130-140: how about the exchange with the Black Sea? Is it common to deal with as if a river?

[Reply]
Yes, in most Mediterranean modelling studies, when the Black Sea is not explicitly simulated, it is often treated as a river. It is actually the case for all studies using the NEMO-MED platform.

[Reviewer comment]
P5 section 1.3: in my opinion this fits better in the methods section, where it can be merged with the specific LMDZ-NEMO set-up.

[Reply]
Yes, that's right. The current structure of the manuscript reflects our intellectual confrontation between generality and particularity. Our philosophy was to firstly propose a general concept, and then fill up different boxes by nominative models. So, we want to keep that structure

[Reviewer comment]
P6 lines 188-190: mention where it can derive boundary conditions from (SIC and SST).

[Reply]
Boundary conditions (in particular, SST and SIC) are derived from global coupled models, from IPSL-CM5A in our actual implementation. We detailed this description in the revised manuscript.

[Reviewer comment]
P6 lines 199-200: give a reference for ORCHIDEE and is it run at the same resolution?

[Reply]
Yes, ORCHIDEE (the land surface model) was integrated into LMDZ. The two components work at the same resolution (, reference added l208).

[Reviewer comment]
P6 line 208: which 'first dataset of river discharges' do you refer to? And does this represent the majority of discharge in the 192 ORCHIDEE river mouths?

[Reply]
We apologize for the confusion. We modified the manuscript accordingly (l216). In fact, we had the choice to use a dataset of climatological river discharges. This dataset divided the Mediterranean draining basin into 33 river mouths. However, when the ORCHIDEE model is interactively used to calculate river discharges, there are 192 river mouths. The two approaches are independent, to be actually used by optional choice.

[Reviewer comment]
P7 lines 211-213: how realistic is the assumption that water from the Black Sea is fresh? And does the Q+P-E budget over the Black Sea derive from the AGCM orARCM?

[Reply]
It is a commonly-used treatment when the Mediterranean model doesn't include the Black Sea. The fresh water assumption is entirely justified although the actual water flow from the Black Sea can be salty, since what we evaluated in terms of E, P and Runoff is indeed the fresh water budget. What is important in the model is not the water mass itself, but the salt content. We made some revisions in the new manuscript for this regard.

[Reviewer comment]
P7 line 215 / fig 1: to fit the figure with all your simulations, can you include that boundary conditions can also derive from reanalysis?

[Reply]
It is theoretically possible to include boundary conditions deduced from re-analysis. But our main goal in the platform is to use global coupled simulations as a departure to conduct the whole chain.

[Reviewer comment]
P7 line 229: maybe put the table that shows an overview of experiments in the main text.

[Reply]
We prefer to let that table describing simulation parameters in the Supplementary materials, in order to put the modelling chain and the general concept into a more prominent position.

[Reviewer comment]
P7 line 239: the cited paper is not in the reference list (as are many other citations)

[Reply]
We apologize for this issue. We now double-checked the revised manuscript.

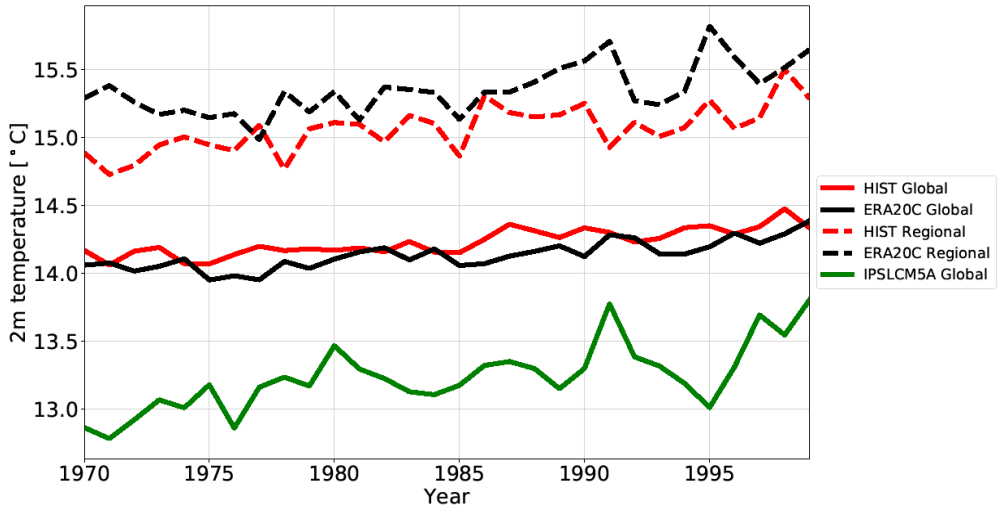
[Reviewer comment]
P8 line 246: "for one period" rather than "for a period"

P8 Fig 1: usually u is zonal wind, v is meridional wind.
P8 line 266: write out WOA

[Reply]
We corrected the manuscript accordingly.

[Reviewer comment]
P9 Fig 2: the legend mentions “HIST-OBS”, I guess you mean experiment “HIST”? Also, why do you use ERA20C here whereas experiment “HIST” is forced with ERA-Interim?

[Reply]
Yes, we corrected the legend and the caption of the graphic. We did not use ERA-interim, since it starts from 1979 only. ERA20C starts from 1970 and is more suitable for our purpose. During the revision, we re-drew the graphic, and improved the description on how different curves were calculated. We also added the global T2m from the ocean-atmosphere coupled model IPSL-CM5A, considered as a baseline, in order to appreciate the improvement that we have in our system.



New Figure 2: Time series of annual mean surface air temperatures at 2 m in HIST (red) and ERA20C (black, ref: Stickler et al., 2014) and IPSLCM5A (green) for global average (solid lines) and Mediterranean-region (ocean and continent) average (dashed lines).

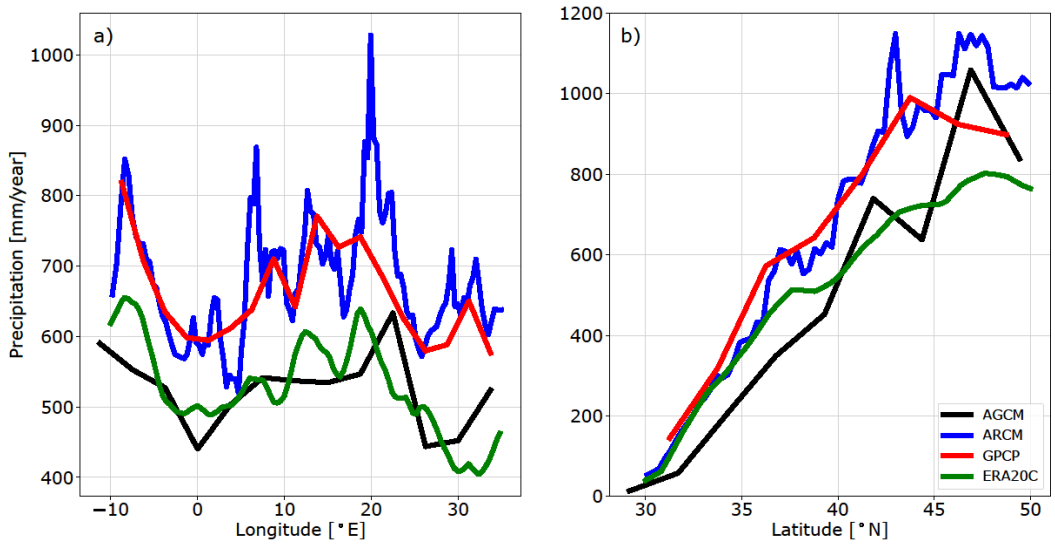
[Reviewer comment]
P10 line 291: Table 2, not 3

[Reply]
Yes, we corrected that. In the revised manuscript, it becomes Table 1 summarizing all components of the fresh water budget.

[Reviewer comment]

P10 Fig 3: again a different dataset is used (CRU), whereas Fig. 2 compares to ERA20C, and "HIST" is forced with ERA-Interim. Why would you use such a range of datasets? And why not use a reanalysis that has values over the sea? Also, looking at the color scales, it seems that the overestimation is as large as the modelled precipitation itself over land. So the relative overestimation there is near 100%?

[Reply]
Thank you for your careful reading. We finally decided to change this plot to curves showing zonal and meridional averages. We also modified the relevant text accordingly in section 3.3.



New Figure 3: Annual mean precipitation, a) meridionally averaged (30 to 50°N), b) zonally averaged (-10 to 35°E), in the historical simulations with AGCM (LMDZ-global) and ARCM (LMDZ-regional). Observation comes from GPCP (Global Precipitation Climatology Project, 1979 to 1999, blue line, ref: Adler et al., 2018). and ERA20C (green line, ref: Stickler et al., 2014).

[Reviewer comment]
P11 Fig 5: in the upper panel it seems like there is a contour overlaying the colours, are those from observations?

[Reply]
No, they are not from observations. Contours in the upper panel are the maximum of MLD (mixed-layer depth) throughout the entire simulation.

[Reviewer comment]
P12 Table 1: provide units and define IS.
P12 line 337: refers to 5b, instead of 5a?
P12 line 340: Figure 6a instead of 7a.

[Reply]
Corrected

[Reviewer comment]
P13 lines 350-352: if there is still a lack of modelling capacity to simulate Med-Sea deep circulation, how can you verify that your study is an improvement?

[Reply]
We now removed this phrase which is not very relevant for our manuscript. There are some uncertainties concerning the changes in deep circulation for the Mediterranean Sea. Our simulation is nevertheless in the range of circulation changes provided by different modeling studies. This is encouraging for us to go a step further and to investigate a larger perturbation, such as the early Holocene. In that context, we added some new text in the revised manuscript (l410):
"The simulation of the thermohaline circulation is well captured by the oceanic model and in the range of the state of the art of existing Mediterranean regional models (compared to the simulations of Adloff et al., 2015 and Somot et al., 2006 for instance). This feature inspires confidence in our modelling platform for the investigations of past climate."

[Reviewer comment]
P14 lines 362-364: Figures 2 and 4 show that your simulation results in significantly lower temperatures than observed, yet here you say they are consistent?

[Reply]
Yes, there are cold biases. We changed the corresponding text in the revised manuscript "The atmospheric simulation is acceptable compared with observations for the air temperature at 2m at both global and regional scales "(l405).

[Reviewer comment]
P14 line 365: How can a model overestimate the precipitation over the surrounding land substantially (fig 3) yet have precipitation over the sea close to observation (Table 2) and have lower river runoff than HIST or PICTL (with overestimation of precipitation over land, why is runoff not overestimated too – is this due to bias correction?)

[Reply]
Yes, it is possible for a model to have roughly right precipitation over the Sea, but too much precipitation over the surrounding land. Our model did show such a feature for its basic climatology, and for changes from PICTRL to EHOL. We now calculated all components of the fresh water budget, and discussed their variation among the three simulations (HIST, PICTRL and EHOL, section 3.3 and 4.4). Rivers discharges increase significantly from PICTL to EHOL, making the fresh water deficit to decrease.

[Reviewer comment]
P15 section 3.2: is there any additional ice sheet remaining in the early Holocene in the model?

[Reply]
No, no more remaining ice sheets for the early Holocene, in our model at least. We used a simulation in equilibrium for 9ka using the orbital forcing appropriate for this period with no more Fennoscandian and Laurentide ice sheet (FIC, LIS). Therefore, the sea level also remains unchanged (as to present day)

[Reviewer comment]
P16 line 398-399: “increased Early Holocene summer insolation” or “increased Early Holocene insolation seasonality”.
P16 line 400-404: refer to figures 7c, 7d.
P17 Figure 7: in the caption the “a” after “b) summer temperatures” should be removed

[Reply]
Corrected

[Reviewer comment]
P20 line 494-497: how does the increased Nile runoff in PICTRL (do you mean compared to observations?) compare to the overall lower runoff reported in table 2?

[Reply]
As for HIST, the river runoff for PICTRL is not calculated with the precipitation of the model. PICTRL river runoff is the same as HIST (so prescribed) but with Pre-damming Nile value.

[Reviewer comment]
P21 Fig 11: Especially in late winter and summer, runoff from the Black Sea is decreased by roughly the same order of magnitude as the increase in Nile runoff. Can you reflect on the possible role that the Black Sea runoff alone could have in sapropel formation?

[Reply]
For summer the runoff decrease of the Black Sea is quite “marginal” compared to the Nile increase (-6000/+45000 m3/s). Actually the role of the Black Sea during the Early Holocene is overall quite marginal but some studies pointed out that a freshwater release was likely throughout the deglaciation (as Chepalyga, 2007, Soulet et al., 2011, 2013) , due to the Fennoscandian Ice sheet melting, and thus affect the Aegean Sea and maybe the Eastern Basin during this period.

[Reviewer comment]
P22, lines 522-525: what do you mean by the reference for correction is the preindustrial state? How is river runoff corrected based on pre-industrial climate?

[Reply]
We choose to “correct” the Mediterranean river runoff during the Early Holocene based on the precipitation difference (EHOL – PICTRL) coming from both the ARCM and AGCM and apply it to the PICTRL river runoff (which was prescribed). The procedure of river runoff is detailed in the supplementary material (**Text S2: Bias correction**)

[Reviewer comment]
P22 lines 543-545: I would not say that your simulations show similar changes as Adloff or Bosmans. For instance Adloff (their fig 9) shows strong salinity increases around Greece, and Bosmans (their fig 11) do not show a decreased mixed layer depth in the Ionian sea.

[Reply]

The reviewer is right, we modify the text l598: “Our oceanic simulation depicts these behaviours well and is overall similar”

[Reviewer comment]
P24 Fig 13: add to caption that this can be compared to Fig 6 (PICTRL).

[Reply]
Done

[Reviewer comment]
P24 line 562: “for the first time” – this you could mention more clearly in the introduction.

[Reply]
Thanks. We added a new phrase for this regard: “To tackle this issue, a sequential architecture of a global-regional modelling platform has been developed for the first time and is described in detail in this paper” (l22).

[Reviewer comment]
P25 lines 571-579: this is not a section that should be in the Conclusions. It is more fitting for a discussion section. It also makes me wonder if there is anything known of the effect of keeping the Bosphorus exchange as it is today.

[Reply]
It is not easy to conclude on the role of the Bosphorus during the S1 period. According to the review and the synthetic work of Rohling et al., 2015, it is quite established that the Black Sea, through the Bosphorus, was not a major freshwater source during S1, so that is why we remain that parameter as it was set in HIST and PICTRL. As the reviewer suggested we moved this paragraph at the end of the 4.5 section

[Reviewer comment]
P26 References: make sure all cited literature is in the reference list.

[Reply]
We apologize for this issue concerning the cited references. We double-checked it during the revision.

Main changes

Article:

Figure 2: new curves (t2m IPSLCM5A).

Section 3.3: new descriptions of the new figure 3.

Figure 3: Annual mean precipitation, a) meridionally averaged (30 to 50°N), b) zonally averaged (-10 to 35°E), in the historical simulations.

Table 1: (former table 2) new column with the Black Sea values and the budget.

Figure 7: fix the contour/shading issues.

Figure 9: remove the difference (EHOL vs PICTRL)

Section 4.4: new description of the new figure 11

Figure 11: move the monthly Nile climatology to the supplements

Section 4.5: move the first paragraph of the conclusion to 4.5

SOM:

Addition of figure 1: climatological runoff of the Nile River

Figure S2: addition of IPSLCM5 SST (raw and corrected)

Figure S8: Interannual evolution of the sea surface salinity (EHOL and PICTRL)

Table S1: (former figure S1)