Reply to Anonymous Referee #1

[Reviewer comment]
The paper describes a technical tool/technique to perform a dynamical down-scaling for the Mediterranean from a simulation with a global climate model. This is applied to a preindustrial/historical simulation and an early Holocene climate state. Whereas some aspects might be useful for other model systems as well, the focus lies on the models used at LMD: LMD atmosphere (global and regionally zoomed) and the Mediterranean setup of NEMO. The usefulness of the technique is mostly demonstrated for the early Holocene. The general approach (global AOGCM/ESM -> global AGCM driven with SST and SIC (sometimes with flux/bias corrections) -> regional ARCM -> regional Mediterranean OGCM) is fairly standard for evaluating future (and recent) climate changes for a regional ocean domain. However, this typically involves quite some handwork. The new aspect here is that there is an automatic procedure that simplifies the handling of this model chain. The authors apply this model chain also to the early Holocene, where a downscaling using a regional ARCM to my knowledge has not been attempted before. In general, the text reads well, there are, however, some problems with the figures, where a more thorough proofreading would have been useful.

[Reply]
We thank the reviewer for the careful and detailed review, and also for the numerous constructive advices. All of them were carefully implemented in the revised manuscript.

[Reviewer comment]
The nomenclature should be unified as well. As an example, in the text and the figures/captions sometimes LMD-global/regional, sometimes AGCM/ARCM (e.g. figs. 8/9 and 7) is used.

[Reply]
We agree that the initial manuscript was confusing for this aspect. In fact, we use AGCM / ARCM when we describe the general aspect of our approach, and we use LMDZ4-global / LMDZ4-regional when evoking the actual implementation of the modelling chain. We improved this aspect throughout the manuscript, including main text and figure captions.

[Reviewer comment]
From the description of the set up, it is not clear, whether the upper boundary conditions for the OGCM does include some restoring-term to a prescribed SST field in addition to the prescribed heat fluxes. This is important, as this seriously affects the interpretation of simulated SST signals. This needs to be clarified in the ms.

[Reply]
When the oceanic model NEMO is used alone, with prescribed surface fluxes, it is indispensable to implement a restoring term with a constant coefficient of 40 W/m2/K. This is a standard procedure for NEMO to prevent eventual run-away cases. In our modelling chain, the target temperature for the restoration is the surface air temperature from the regional atmospheric model.

[Reviewer comment]
The analysis of the Early Holocene simulation is a bit superficial, but this simulation acts rather as a proof of concept, so this is not a major problem.

[Reply]
Thanks, indeed we chose to publish our platform in GMD and to provide just an illustration. This is why we do not emphasize too much on this case study. Nevertheless, as also suggested by the second reviewer, we provide more information on the improvements obtained going from global to regional scales.

[Reviewer comment]
In some plots I had troubles to find the signals the authors were mentioning, some plots might even be wrong. In general, I believe that quite some revisions are necessary before the paper can reach a state sufficient for publication in GMD.

[Reply]
We believe that the revised manuscript is improved for this aspect. Many thanks to the reviewer for his/her careful reading of the manuscript We improved the plots, clarified most of them and corrected the errors raised by the reviewer.

[Reviewer comment]
Detailed comments:
Abstract Please explain to what extent this paper is useful to readers not using the LMD model system.

[Reply]

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

This manuscript is a compromise between a general concept of a sequential modelling platform from global to regional and an actual implementation with numerical tools available in IPSL. We hope that our manuscript can help to promote such an approach in dealing regional climate issues. The concept that we proposed can be easily extended to other groups with a similar background and a focus on high-resolution climate modelling

[Reviewer comment]
Line 32 'it' obviously is supposed to refer to Mediterranean basin, but this seems not to be backed by the structure of the sentence. ?seat? of civilizations.

[Reply]
We rephrased this sentence. By "seat of civilizations", we meant that the Mediterranean basin played an important historic role for human civilizations.

[Reviewer comment]
l55 'In this paper, we developed' -> Here we describe

[Reply]

We rephrased this sentence and a related sentence later in the paragraph.

[Reviewer comment]
l94 'surface fluxes and wind stresses from observations'
I am not aware of daily observational data for fluxes. You probably refer to reanalysis products. With respect to fluxes, these include the use of a model and its parametrisations. Please be correct. same in l115.

[Reply]
Yes, we agree, it is almost impossible to have good observation of fluxes at air-sea interface for daily frequency and for large extension in space. Such fluxes can only be obtained within a model (for climate simulation or meteorological analysis and re-analysis). We made the necessary modification accordingly (l100).

[Reviewer comment]
l101
'the method is not well adapted'
In fact it is, and superior to what you propose, the only problem is the computational effort for long simulations.
l120
it is possible but rather expensive. Please be more specific.

[Reply]
Yes, we agree. We recognize that our general framework of a sequential modelling chain is also a way to remediate the issue of computational resources. We made the necessary modification accordingly (l130).

[Reviewer comment]
l152 An alternative could be to rerun the coupled model with high frequency output for 30 years rather than to rerun an AGCM. please discuss.

[Reply]
Yes, we agree. The best way is to save the high-frequency 3-D outputs when running (or re-running) the paleo applications in their fully-coupled configuration. But this is not always possible or easily feasible. Our proposition of running only AGCM (the same one as in the coupled-mode application, or another independent one) is based on pragmatic consideration. Previous studies (references cited in the main manuscript), including some of our own studies, seem to validate this approach using AGCM with climate signals from SST and SIC in ocean-atmosphere coupled models.

[Reviewer comment]
l191 1.875◦x1.25◦ with 96x72 grid points (from Fig.1) does not result in a global domain! Please correct.

[Reply]
We apologize for the mistake. That should be 3.75 in longitude and 2.5 in latitude (l197).

[Reviewer comment]
l197 Please specify the frequency of the required AGCM output l201

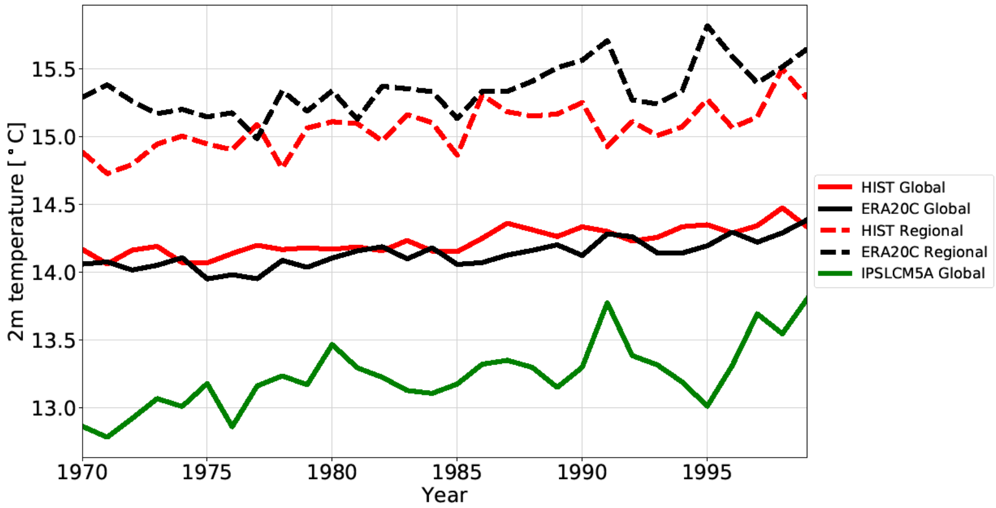
[Reply]
We now provided these parameters: AGCM to ARCM every 2 hours; Fluxes from ARCM to ORCM every day; Atlantic buffer zone and discharges from rivers updated every month (l203).

[Reviewer comment]
L201 please specify in this section, whether any restoring of SST to prescribed values is involved. Is there any flux correction for P-E?

[Reply]
There is no flux correction in running our oceanic model NEMO: neither heat fluxes, nor water fluxes (P and E), nor wind stress. However, when the oceanic model NEMO is used alone, with prescribed surface fluxes, it is indispensable to implement a restoring term with a constant coefficient of 40 W/m2/K. This is a standard procedure for NEMO to prevent eventual run-away cases. In our modelling chain, the target temperature for the restoration is the surface air temperature from the regional atmospheric model. We made the necessary modification accordingly (l222).

[Reviewer comment]
l275 /Fig.2 caption define Mediterranean region/ Mediterranean-only. Does this mean only over the ocean?
l278 to what extent is the the response in 2m-air temperature over the ocean surprising if the SST is prescribed? Would you have gotten the same trend from the global AOGCM?

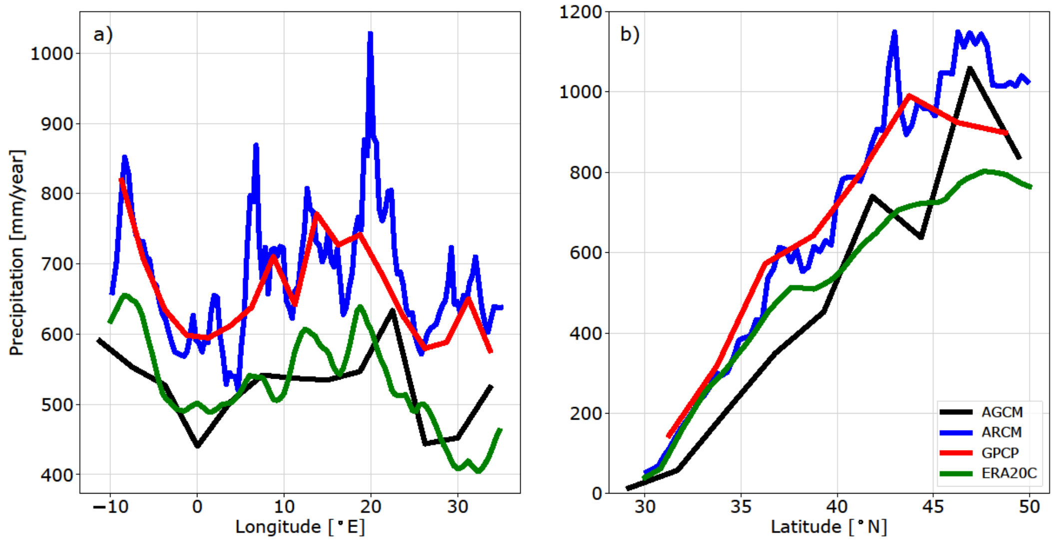
[Reply]
We apologize for the confusion. In fact, for both global average or Mediterranean average, we used surface air temperature at 2 m from land and water bodies. The Mediterranean average corresponds to the regional domain of LMDZ4-regional. We improved the caption to avoid any confusion. During the revision, 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 made in our system (Figure 2 and l298).



New figure 2: **Time series of annual mean surface air temperatures at 2 m in HIST (red) and ERA20C (black) and IPSLCM5A (green) for global average (solid lines) and Mediterranean-region (ocean and continent) average (dashed lines).**

[Reviewer comment]
Fig. 3b It seems that the anomalies HIST-OBS in panel b are not anomalies but the same fields as in 3a except with a different colour bar and masking over the ocean. Here you use HIST-OBs as anomalies, in Fig. 2 obviously as absolute values. Please stick to one definition. l293 BASIN MEAN 'P and E over the Med Sea ARE very close ...' please correct!

[Reply]
Thanks for your careful reading. Indeed, the initial panels in Fig. 3 were not very relevant for our purpose to illustrate the performance of our platform in simulating the rainfall. We finally changed Fig. 3 to a new illustration in the form of zonal and meridional averages (including more results). We also changed the description in the manuscript, accordingly (see 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]
Table 3 please include an extra column with the total freshwater budget of the Med (saves the reader from doing it him/herself).

[Reply]
Thanks. This is Table 1 now in the revised manuscript. We now completed it, including all terms of the fresh water budget over the Mediterranean Sea.

[Reviewer comment]
Figure 5 MLD averaged over the entire year is not very useful. Rather use annual max MLD or winter (Feb or March) MLD.This would indicate the depth of convection and thus the locations of deep water formation. This would fit to your use of this figure in l336. Table 1 bias of a simulation would be HIST-obs. From Fig. 4, I conclude that the model is too cold and salty. Here you seem to use a different sign for bias, which is confusing for the reader.

[Reply]
We apologize for the confusion. The figure caption was not appropriate. Our diagnostics were indeed the winter maximum value of the mixed layer depth. We corrected it accordingly in the revised manuscript. In Table 2 (initially labelled Table 1), we now corrected the sign of the convention.

[Reviewer comment]
l336 thicker -> deeper Please explain, why the simulated MLD is deeper in the EMed.

[Reply]

We think that a thicker MLD in the eastern basin is due to the salty conditions.

[Reviewer comment]
Fig. 6 why do we see in the ZOF deep cells both in EMed and WMed > 0.2 Sv but no corresponding water mass movement in the Gulf of Lions and the Adriatic? The deep branches seem to be < 0.1 Sv. Please explain this. Specify the longitudinal extent of the domains used to calculated the MOFs. The topography in the Adriatic MOF seems to be pretty deep, please check. You are using rows/columns in a wrong way. Where in Fig. 6 is the 3rd column from left, there are only 2 columns. (should be row from top) Please correct.

[Reply]
We firstly corrected the issue of row/column confusion and we also detailed the domain used for our calculation of the overturning stream function. The Adriatic MOF seems deeper, since our calculation includes the north of the Ionian Sea. But our MOF roughly corresponds to those of other similar studies (e.g. Somot et al. 2006 Fig. 11 and Adloff et al. 2016 Fig. 6). ZOF being integrated from the south coast to the north coast, and MOF from the west coast to the east coast for a particular semi-closed sector, we can observe different deep cells in the ZOF and MOFs. In fact, ZOF includes the circulation near the African coast which is in none of the MOFs

[Reviewer comment]
l348+ There must be more simulations than just the ones using the same ocean model setup. There are more models, e.g. the MIT model. Are there any estimates from observations? Please compare:

[Reply]
Under the Med-CORDEX framework, there are some initiatives for inter-comparison of models over the Mediterranean area. Results and publications are expected soon. In the recent literature, we also found an interesting work of Pinardi et al. (2019) who present ZOF derived from their reanalysis data (1987-2013). It seems that our ZOF in HIST is weaker than that from observation. We updated the text accordingly (l393): “The ZOF depicts in HIST simulation is consistent with the reanalysis (1987-2013) of (Pinardi et al., 2019) over the Western basin, but show a weaker Eastern deep cell compared to the reconstruction.”

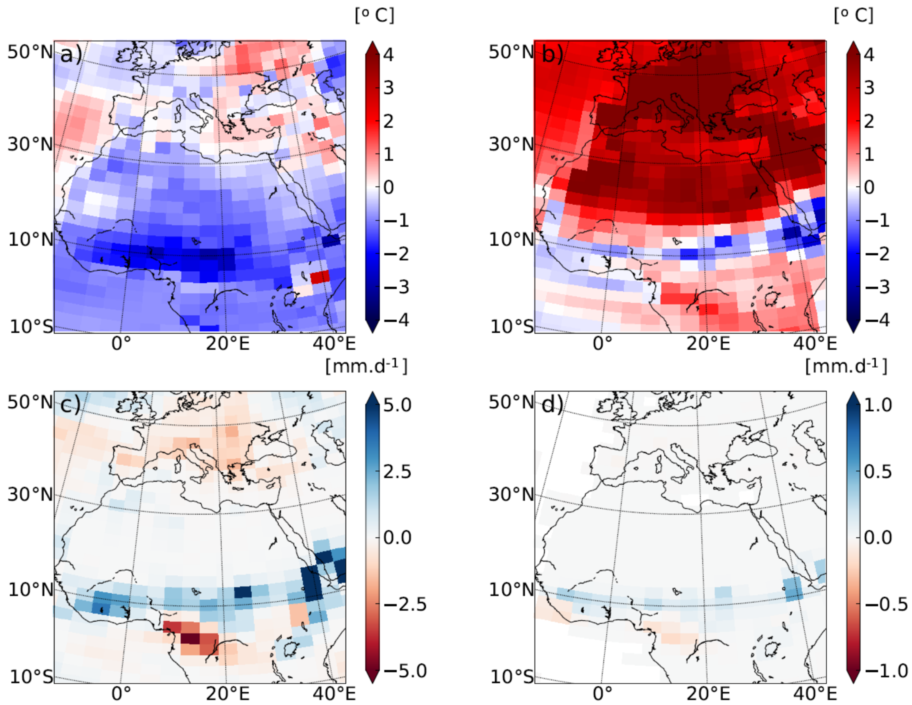
[Reviewer comment]
l350: “A large spread between the models for this pattern indicates that there is still a lack of modelling capacity to simulate the deep circulation of the Mediterranean Sea.”
l367: “The thermohaline circulation is well captured by the oceanic model (compared to the simulations of Adloff et al., 2015 and Somot et al., 2006 for instance), which inspires confidence in our modelling platform for the investigations of past climate.”
For me these two statements do not go together very well.

[Reply]
However, there is 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. Therefore, the sensitivity for historical period is encouraging to go a step further and to investigate a larger

perturbation as the early Holocene one. To remove any confusion, we just deleted the first phrase and made some revisions for the second one (l410).

[Reviewer comment]
Figure 7 Please include labels a), b) etc. The top right panel looks like summer temperatures, but has a colour bar indicating mm/d. Inverse problem in bottom left panel. Please use same colour bar for summer and winter temps.
Compare Figs. 7 and 10! Assuming that LMD-Global is equal AGCM, why is Europe so much drier in Fig. 10 than in Fig. 7? Shouldn't these panels show the same signals? Please explain. Fig. 10 Please use the same colour bar in all panels!

[Reply]
We apologize for the wrong label in Figure 7b. We corrected it now. For the apparent difference between Fig. 7c and Figure 10c, it was mainly due to a small calendar shift, combined with a graphic problem in relation to "contour fill" with python matplotlib. The graphic was now plotted with "shading" option, which seems resolve the problem. We updated both Figs. 7 and 10.



New Figure 7: Deviations between EHOL and PICTRL in the AGCM for a) winter temperatures at 2m, b) summer temperatures a, c) June to August precipitation, and d) July to September surface runoff (averaged over the entire simulation).

[Reviewer comment]
Fig. 9 Please add arrows in AGCM plots.

[Reply]
Ok, arrows added now in the revised plots.

[Reviewer comment]
Fig. 11 A mess! Split it up into 2 figs. and make sure that there is a clear relation between colour labels and displayed data panel. Why is the Nile shown in the west as well? If the Nile is flux corrected in EHOL, how can there be an anomaly of <-3000 during winter. Does this indicate a negative Nile runoff in EHOL winter? Please explain and discuss implications (deep convection in Nile plume?).

[Reply]
We apologize for any confusion in Figure 11, and we recognize that it was not an easy graphic to read and to understand. We entirely revised it and made text revisions in the “hydrological changes” subsection. We also split the graphic into two parts, as suggested. We keep only one part in the main text, and we put the second part into Supplementary materials. When we flux-corrected the river runoff there is no negative values, please see the sub section “River runoff to the Mediterranean Sea” of the section “Text S2 bias correction” in the supplementary. See also our response relative to your comment for the supplementary material.

[Reviewer comment]
l530 and Fig. 12c Please change consistent with Fig. 5!

[Reply]
Yes, we checked the consistency between Figure 12c and Figure 5. They are consistent. We added a phrase in this sense in the revised manuscript (l629).

[Reviewer comment]
l521 Please indicate in this section, how close the surface is to steady state. Please show time series of basin mean SSS during the EHOL and PICTRL simulations. Maybe in the supplement.

[Reply]
We believe that our simulation PICTRL and EHOL reached their stationary state, at least for surface properties. Figures S6 to S8 display the time series for the index of stratification, the zonal overturning stream-function and sea-surface salinity, which allow us to conclude the quasi-stationarity of the simulations. The following panel reproduces Figure S9 showing the evolution of SSS in PICTRL and EHOL.

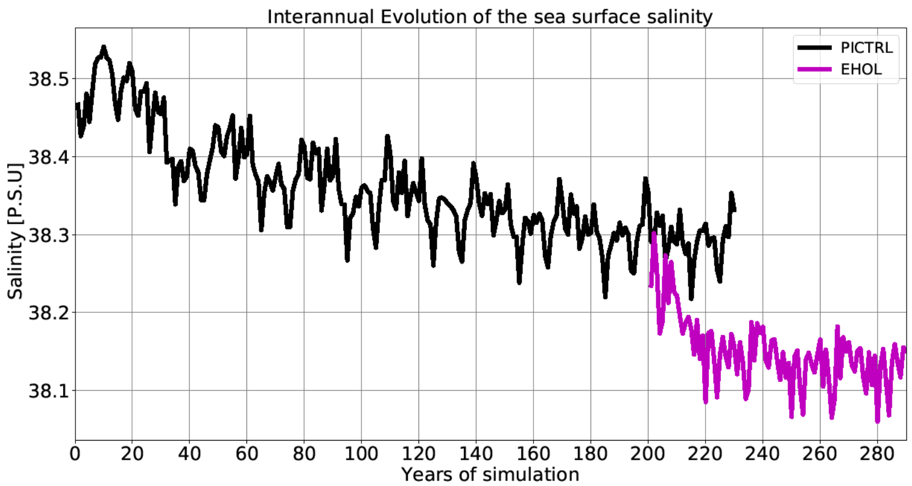


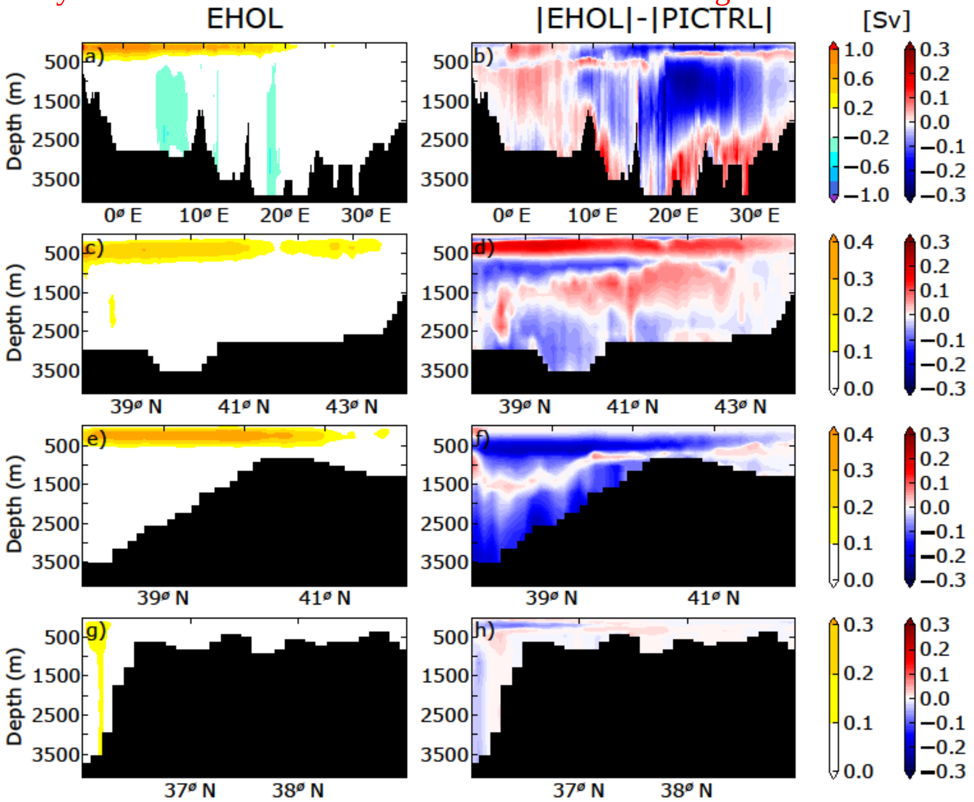
Figure S9: Interannual evolution of the sea surface salinity (SSS) for the Mediterranean Sea for the PICTRL and EHOL simulations (including the PTCRL spin-up phase).

[Reviewer comment]
Fig. 13 Please correct the caption Ionian should be Aegean.

[Reply]
Corrected

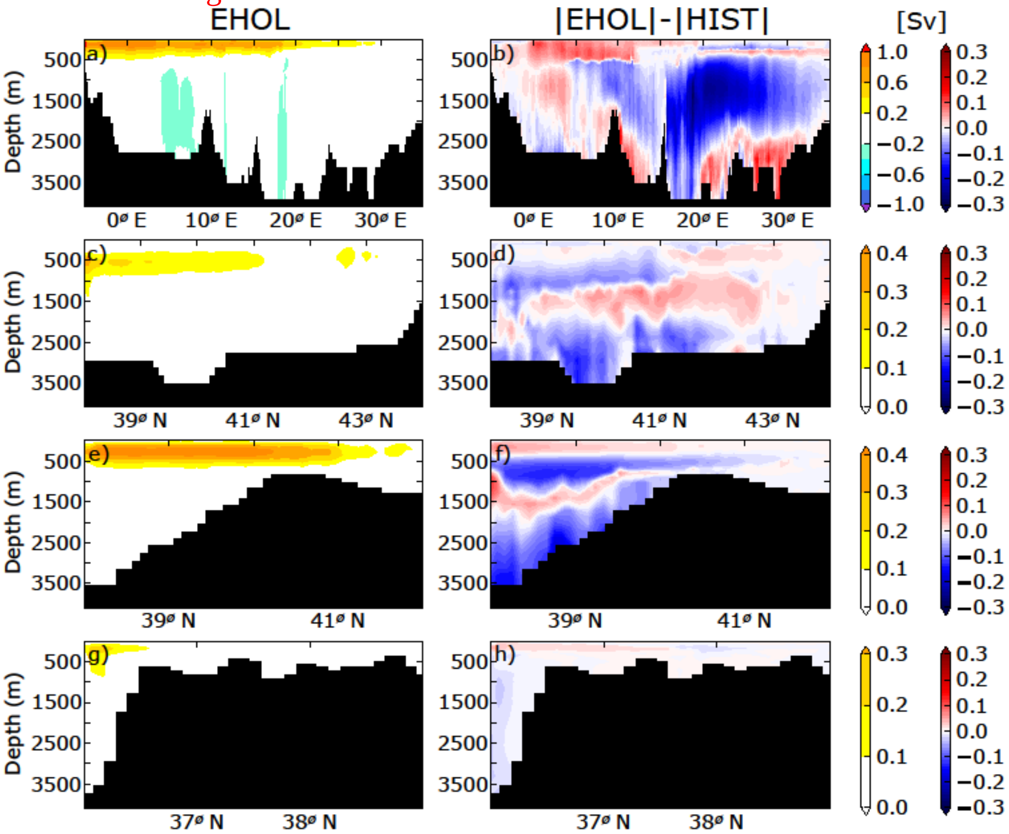
[Reviewer comment]
l530 Comparing Figs 6 and 13 it seems that the ZOF in EHOL is about as strong as in HIST. Compared to PICTRL it is indeed reduced. In the MOFs it is hard to see the reduction which is claimed to be obvious ('is followed by a general reduction in the thermohaline circulation compared to PICTRL'). Please make a careful and more detailed comparison. And include discussion of Fig. S7 which shows only a weak reduction.

[Reply]
We were limited to a visual inspection in the manuscript, since this GMD manuscript was mainly devoted to the introduction and presentation of the modelling platform. Detailed diagnostics will come in future works. Nevertheless, if we plot the difference EHOL-PICTRL and EHOL-HIST (as shown here in this review-reply text), we do clearly see the reduction of the Mediterranean overturning circulation.



Additional figure 1: Overturning stream function. First column: EHOL, second column: EHOL minus PICTRL. From top to bottom are ZOF for the entire

Mediterranean, MOF for the Gulf of Lion, MOF for the Adriatic/Ionian Sea, and MOF for the Aegean Sea.



Additional figure 2: The same as in the precedent, but for EHOL-minus-HIST.

[Reviewer comment]
l579 you also used preindustrial pCO2 instead of early Holocene pCO2, which should be about 260 ppm. Please mention.

[Reply]
This comment is perfectly true. We should have changed in our case study the pCO2 value to 260 ppmv, as it is recommended by PMIP for mid-Holocene. Howerver our **goal in this paper was mainly to have a sensitivity to orbital parameters.** This is clearly stated in the supplementary information section Table S2.

[Reviewer comment]
supplement l180 'latest version' not a particular good description, especially in a few years from now. Specify the version.

[Reply]
Yes, that's right. We deleted the irrelevant words in the revised manuscript (l37 and l152).

[Reviewer comment]
supplement l260 Please mention that the method can lead to negative river runoff. Is this then effectively the same as a very strong local evaporation? Does this initiate salt driven convection at the mouth of the Nile?

[Reply]
Theoretically, a negative river runoff can happen with the water budget treatment in our modelling chain. It would be equivalent to a strong evaporation that can eventually induce a salt-driven convection. But in our case, EHOL shows a general increase of fresh water discharge in comparison to PICTRL, which prevents negative runoff from occurring.

[Reviewer comment]
supplement l299 Please compare the results shown in Fig. S2 with the bias corrected SST used to drive the global AGCM. Is there a real improvement or do you get more or less the same results? Compare with similar plots in Mikolajewicz (2011), who got almost no difference in the simulated climate signal.

[Reply]
We understand your concern and our results confirm your guess. What shown in Fig. S2 (now S3 in the revised manuscript) is the SST in the simulation EHOL, with comparison to a few reconstruction data. You asked if it is consistent with the bias-corrected SST (original SST from IPSL-CM5A, but with biases corrected) that was used to drive both AGCM and ARCM. The answer is Yes. The two fields are quite close to each other, although they do have different spatial resolutions and they differ in detailed structures. Uve Mikolajewicz, in 2011, published a similar study on the Mediterranean Sea climate during LGM (Climate of the Past, doi: 10.5194/cp-7-161-2011). He pointed out (Fig. 15, there) that the SST changes obtained in the regional ocean simulation is very close to those from the initial Earth System Model (MPI-ESM) serving as a driver with an AGCM in the intermediate step. We now cited this publication and mentioned the absence of ARCM in his approach.

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)