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Interactive comment on "Development of a sequential tool, LMDZ-NEMO-med-V1, to conduct global to regional past climate simulation for the Mediterranean basin: An Early Holocene case study" *by* Tristan Vadsaria et al.

Anonymous Referee #1

Received and published: 26 September 2019

Review of the ms. 'Development of a sequential tool, LMDZ-NEMO-med-V1, to conduct global to regional past climate simulation for the Mediterranean basin: An Early Holocene case study' by Tristan Vadsaria, Laurent Li, Gilles Ramstein and Jean-Claude Dutay submitted to GMD

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

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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.

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.

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.

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. 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.

Detailed comments:

abstract

Please explain to what extent this paper is useful to readers not using the LMD model system.

l32

'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.

l55

'In this paper, we developed' -> Here we describe

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.

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.

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.

l191

1.875°x1.25° with 96x72 grid points (from Fig.1) does not result in a global domain! Please correct.

l197

Please specify the frequency of the required AGCM output

l201

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

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?

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!

Table 3

please include an extra column with the total freshwater budget of the Med (saves the reader from doing it him/herself).

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. give 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.

l336

thicker -> deeper Please explain, why the simulated MLD is deeper in the EMed.

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.

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:

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...

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!

Fig. 9

Please add arrows in AGCM plots.

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?).

l530 and Fig. 12c

Please change consistent with Fig. 5!

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.

Fig. 13

Please correct the caption Ionian should be Aegean.

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.

l579

you also used preindustrial pCO2 instead of early Holocene pCO2, which should about 260 ppm. Please mention.

supplement

l180

'latest version'

not a particular good description, especially in a few years from now. Specify the version.

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?

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.

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