

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 #2

Received and published: 4 November 2019

Review of GMD-2019-196 Vadsaria 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. So my main comments are:

- structurally the paper can improve to clear up some unclarities. For instance, Fig.

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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.

- 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?

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.

P3 lines 73-77. Please provide section numbers when outlining the paper.

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

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.

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

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

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?

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 or

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ARCM?

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

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

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

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

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?

P10 line 291: Table 2, not 3

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%?

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

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.

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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?

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

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?)

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

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

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?

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?

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

P22 lines 543-545: I would not say that your simulations shows 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

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in the Ionian sea.

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

P24 line 562: "for the first time" – this you could mention more clearly in the introduction.

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.

P26 References: make sure all cited literature is in the reference list.

1. Does the paper address relevant scientific modelling questions within the scope of GMD? Does the paper present a model, advances in modelling science, or a modelling protocol that is suitable for addressing relevant scientific questions within the scope of EGU?

Yes. Protocol is suitable for addressing paleo-questions. The models themselves are however not new, and I would like to see more evidence that the modelling chain used in this paper is necessary (e.g. compare to a NEMOMED8 simulation directly forced with AGCM instead of ARCM) and / or can be used to address questions not yet addressed (e.g. separate the freshwater flux changes. Ocean simulations of e.g. Holocene or LGCM are not new.)

2. Does the paper present novel concepts, ideas, tools, or data? The specific modelling chain (AGCM -> ARCM -> OGCM) seems new for the Mediterranean.

3. Does the paper represent a sufficiently substantial advance in modelling science? I think so. I would, however, like to see this more substantiated (see Q1)

4. Are the methods and assumptions valid and clearly outlined? Valid yes. For clarity it would be good to put a table overview of all simulations in the main text (I found it in the supplementary though)

5. Are the results sufficient to support the interpretations and conclusions? The model

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set-up is sufficiently clear, but whether this particular set-up is needed (rather than e.g. just using global GCM directly onto a Med-OGCM) could be more substantiated.

6. Is the description sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? In the case of model description papers, it should in theory be possible for an independent scientist to construct a model that, while not necessarily numerically identical, will produce scientifically equivalent results. Model development papers should be similarly reproducible. For MIP and benchmarking papers, it should be possible for the protocol to be precisely reproduced for an independent model. Descriptions of numerical advances should be precisely reproducible. Yes.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Credit is given (although a lot of references in the text are missing from the reference list!!), and the specific modelling chain seems new for the (paleo-) Mediterranean.

8. Does the title clearly reflect the contents of the paper? The model name and number should be included in papers that deal with only one model. Yes.

9. Does the abstract provide a concise and complete summary? Yes.

10. Is the overall presentation well structured and clear? No, I think the paper could benefit from some clarifications. It is sometimes unclear which simulations are referenced to and what simulations' boundary conditions are (For instance in Figure 2 HIST-OBS is not yet defined). An overview of experiments in the main text (as opposed to only in the supplementary material) would be helpful. Also I feel part 1.3 is too overlapping with the methods, as it essentially a generic version of the methods. The distinction between results-discussion-conclusion also needs to be revised. For instance, the conclusion contains a paragraph on how the Bosphorus was dealt with, which should be in discussion. Furthermore I found that many citations in the text are not present in the reference list.

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11. Is the language fluent and precise? yes.
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? N.a.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? I think section 1.3 could be removed or combined with the methods section (see Q10). There should also be a clearer overview of the simulations.
14. Are the number and quality of references appropriate? Yes, but do make sure all citations are actually present in the reference list.
15. Is the amount and quality of supplementary material appropriate? For model description papers, authors are strongly encouraged to submit supplementary material containing the model code and a user manual. For development, technical, and benchmarking papers, the submission of code to perform calculations described in the text is strongly encouraged. I appreciate the links to the model code as well as the user manual in the supplementary materials.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2019-196>, 2019.