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Climate of the Past Discussions

Interactive comment on "The response of Mediterranean thermohaline circulation to climate change: a minimal model" *by* P. Th. Meijer and H. A. Dijkstra

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Dear Editor, dear Uwe,

I appreciate the idea of Meijer's and Dijkstra's modeling approach using a model utilizing idealized boundary conditions. I myself have done this many times before. I agree that this is a very helpful approach for paleo-simulation where we have limited information to construct detailed boundary conditions. I read the manuscript with great interest and came up with comments, suggestions, and questions. I will try to itemize them.

Major items:

1.) The authors write in their abstract that their model captures "some" important features of the Mediterranean circulation. I assume that is because the authors used idealized boundary conditions from the beginning. If they use a robust model, why wouldn't they first use observed/non-idealized boundary conditions to prove that the model is capable of reproducing the main features of the Mediterranean circulation and water mass distribution? I would like to see at least one result that compares "real" boundary conditions with idealized ones. I would also suggest including two identical experiments, one forced with wind, the other one without in order to see the importance of the wind influence.

2.) The authors state in their abstract that there are only limited atmospheric boundary conditions available. However, nowadays global coupled AOGCM are able to produce even for the past decent atmospheric boundary conditions though it is not feasible to run such a model for twenty or thirty thousand years. Nonetheless, it is possible to produce forcing fields for several key time slices of the whole time interval of interest.

3.) p. 1733, line 18: same as for 2.)

4.) p. 1734, lines 21-25: The authors base the neglect of the wind stress on the difficult to estimate regional influence of the mountains on the wind field. The question that should be answered here is whether the results of the ocean circulation can be improved by using strongly idealized wind stress. So, why not to use for example an interpolated wind field from a global AGCM/AOGCM for a specific time slice rather than dropping it at all. This study is already using idealized values for temperature and freshwater flux at the sea surface. I would like to see model results included that use an observed/realistic wind field as well as on simulation that uses an idealized/smoothed wind.

5.) p. 1735, lines 14-22: The model was initialized with T=16 centigrade and a salinity of 36 and able to produce results that come more or less close to observed values. I wonder why the authors chose for their second case an initial temperature of 10 centigrade rather than a temperature far above the 16 centigrade. The initial combination of T=10 centigrade and S=36 produces a far denser deep water compared to the first combination. Somehow I cannot imagine that the two different experiments would produce a similar deep water mass distribution. Too dense deep water could reside in the deep eastern basin without being exchanged/flushed out within reasonable integration time. The authors should judge their model results rather by looking at the deep basins than at the Atlantic cell which should result in correct values more or less by default.

6.) p. 1736, line 15-16: As previously pointed out, it might be wise to produce one control experiment with observed boundary condition to see if the chosen model setup can be applied to the Mediterranean.

7.) p. 1737, lines 6-10: The authors are testing the model's sensitivity using changed atmospheric forcing in order to find favorable boundary conditions that promote anoxic events/sapropels. Thus, they are reducing the deep water production/overturning. My general question is about the chosen evaporation rate of 0.5 m/yr for their control experiment. The observed evaporation ranges from 0.5-1.3 m/yr. Why don't they test their model within given observed boundaries starting from 1.3 m/yr?

8.) p.1738, lines 11-29 and p1739, lines 1-9: This section should be tightened since it describes expected basic textbook behavior of an ocean basin.

9.) p. 1741, lines 6-7: I would expect this result.

10.) p. 1745, Figure 2: It is difficult to judge but there seems to be still a drift in E_k, T, and S past year 800. Authors may want to show longer integration times, e.g., first 2000 years.

11.) p 1746, Figure 3: It seems that the model is simulating the formation the Gulf of Lyon deep water well as well as to some extend the formation of Adriatic deep water. However, this figure does not really show the formation of Levantine Intermediate Water (LIW) in the eastern Mediterranean.

Minor items:

12.) p. 1733, line 12: Replace “in the high latitudes” with “at high latitudes”

13.) p. 1734, line 3: Replace "Modular . . . Array” with “Modular . . . Array (MOMA)”

14.) p. 1734, lines 17-18: Replace “varies with . . . which” with “varies only with the cosine of latitude, which approximates . . . temperature field observed at present (Fig. 1).”

15.) p. 1734, line 22: Replace “awkward” with “not justified”

16.) p. 1735, line 16: Rewrite “The residence time of the present Mediterranean basin is”. “The residence time of the Mediterranean deep water is . . .” might be what the authors are trying to say.

17.) p. 1744, Figure 1: Spell out ECMWF at least once.

18.) p. 1745, Figure 2: It is difficult to judge but there seems to be still a drift in E_k, T, and S past year 800. Increase font size of salinity and temperature (near solid and dashed lines).

19.) p 1746, Figure 3: Annotation of isolines is difficult to read; negative values show only in one place the minus sign because the numbers are to crowed (this can be fixed in GMT using flag for curvature and spacing); text in white text box is unreadable (lower left plot); same is true for the text under the figure, i.e., longitude [degree]. Negative isolines should be represented as dashed lines. A color bar to the left or right of the graph might be helpful as well.

20.) p. 1747, Figure 4: Panels are either too small or the font size should be increased.

21.) p. 1748, Figure 5: More or less the whole text is messed up; use dashed lines for negative values and color bar; see comments to Figure 3.

22.) p. 1749, Figure 6: Panels are either too small or the font size should be increased.

Final remark: The manuscript should be edited and proofread by a native English speaker. There are too many flaws (only a few are listed above). Note to the authors: This review might look harsh but I believe that additional suggested experiments can be easily managed and improve the manuscript. I would like to see this paper published as previously pointed out. I believe that idealized models can greatly improve our understanding of past climates/oceans. Bringing the figure up to par shouldn't be a problem either. This manuscript definitely requires a language brush up; I may be able to help out here (I am not a native speaker but my family is familiar with scientific editing).

Note to the editor and Uwe: I am not familiar with this journal. Are the authors allowed to share their word document via this thread assumed that they would agree? This would allow me to add my comments directly into the manuscript. If not, that's OK too. I do not want to complicate the process. Thanks. Thanks, Bernd

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Interactive comment on Clim. Past Discuss., 5, 1731, 2009.