

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

Anonymous Referee #2

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This paper looks at the development of an idealized (yet still fairly high resolution) configuration for modelling the Mediterranean. The goal of the work is to show that a simplified set up with idealized forcing can mimic the main features of the Mediterranean’s circulation, and thus be useful for paleoclimatic studies when much about the forcing is unknown.

The paper is well written and fairly easy to follow. Ideas, motivation, model details and results are all clearly identified and make sense. As such, I would recommend acceptance with minor revisions.

Details on specific comments are given below.

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- pg 1733, line 6: specify which sea (The Mediterranean), otherwise the statements makes in seem generic (all seas).
- pg 1733, line 17: With respect to the statement that paleoclimatic studies call for a model of relatively coarse resolution that can run for long times - that is true, but one still needs enough resolution to represent the proper and relevant processes for the question being examined.
- pg 1733, line 24: Box models have been applied to the Mediterranean. I'm pretty sure that Korres and Lascaratos did some such work back during the EU Clivamp project. I believe a paper was submitted to JGR. Stephan Matthiesen also did some such work with Keith Haines, although I am not sure what was published out of it, beyond a Ph.D. thesis.
- pg 1733, general: Since the authors are discussing the question of sapropels as an application of their model, it might be good to add a brief introduction to previous sapropel modelling.
- pg 1734, line 15-16: The authors are effectively using reverse mixed boundary conditions. How appropriate are they? Might the type of boundary conditions used effect the results - previous work on the Atlantic (say by Marotzke) has suggested that model equilibria are sensitive to the type of atmospheric boundary condition used.
- pg 1734, lines 20-25: Although I would agree that the paleo-winds would likely have been different than today, I am not sure about the justification used in terms of the controlling nature of the mountains - since their would not have been significant topographic changes over the period of S1, for example, during the Holocene.
- pg 1734, lines 24-25: If you don't have winds, how to you determine evaporation from the appropriate bulk formula. More generally, can you justify that the lack of winds don't affect the long term results and impact on key processes.
- pg 1735, figure 2: What is the spike just after year 400?

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- pg 1735, lines 15-20: Is a lack of river runoffs likely to explain the high salinity. This jumps out especially since the 0.5 m/yr used is small compared to some observational estimates of the net E-P for the basin.
- pg 1736, paragraph 1: The lack of WMDW is a common issue in these type of models, so it might not be significant for the authors purpose, if convection is in the Adriatic. But maybe some more discussion of this topic might be useful.
- pg 1736, lines 20-21: In this in the preceding paragraph, there seems to be discrepancy it what is reported about the Adriatic water - does it sink within this sub-basin, or just south of it in the Ionian. This could be cleared up.
- pg 1737, general: In reality, LIW inflow and salt preconditioning important for Adriatic convection. The wording makes it seem like this isn't the case here. Discuss.
- pg 1738, line 19: decrease in salinity in the deeper layers, or increase?
- pg 1740, line 14-16: In terms of the oxygen content changes, that was examined by Stratford et al. in a modelling exercise, published in Global Biogeochemical Cycles.

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

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