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Climate of the Past Discussions

Interactive comment on "Modeling Mediterranean ocean climate of the Last Glacial Maximum" *by* U. Mikolajewicz

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I would like to thank the reviewer for his/her constructive comments.

The simulations have been continued for several hundreds of years and the plots/numbers have been updated.

This paper is based upon a modelling study of the Mediterranean during the last glacial maximum. It examines the circulation and water properties of the basin using an OGCM driven by a high resolution paleo-climatic simulation. The authors attempt to present their results in terms of the fit with various paleo-reconstructions of relevant fields. The role of sill depth changes at Gibraltar are examined from the model and theoretically.

This is a reasonably well written paper that adds further insight to our understanding of the paleo-Mediterranean. The effort to link the results to various proxy-data means that the work will be of use to people producing and analyzing such data. As the author highlights, the work goes a step beyond previous modelling studies. Thus I recommend acceptance with minor revisions. My specific comments are detailed below. In terms of the introduction and review of previous modelling work, it would be good to discuss some of the recent work that has come out of the Utrecht group, as some of the ideas from their box and gcm studies are relevant to understanding the work done here, as well as the impact of sill depth changes at Gibraltar.

A reference to the Alhammoud et al. (2010) paper has been included.

For the Atlantic box, are the properties modified to glacial conditions, and if so how.

This wasn't clear from my reading of the paper. And if they are not modified, some discussion of the significant of this choice is needed.

Of course LGM hydrography is used (anomalies). This has now been stated in the text.

Page 4,Line23: Might be useful to mention the Rossby radius when stating the model is eddy-permitting.

Modified to state that some eddy variability is simulated.

Page 5, Line 20: Are any of the SST bias corrections referred to here relevant for this modelling study? If so, further discussion is warranted.

It is a standard approach similar to the flux correction in early coupled AOGCMs. For linear problems it is ok, for strongly nonlinear problems it caused problems. In global simulations this occurred mostly at the sea ice edge or at places, where deep convection led to a massive heat release from ocean to the polar atmosphere. In the Mediterranean I would expect a more linear dependence of the heat fluxes on SST.

Page 7, top: If there is more convection, might not one expect a shorter turn-over

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timescale as more new deep water is being produced each year?

The exchange through the Strait of Gibraltar is reduced by roughly a factor of 2.

The word residence time might be less missleading. But this piece of text has been removed anyway, as the simulations have been extended.

Page 9, Line 17: yes the errors are smaller at depth, but so are observed ranges of the variables. So, more relevant is whether the size difference between the variable range and the errors is smaller.

yes, but even if you would choose the variability of the world ocean at these depths, the match is still good.

Page 9, line 24: Using 0.05 kg m**3 to compute MLD seems like a wide threshold to me. Most global climatologies use 0.01 to 0.03 kg m**3 I believe. Is this an indicator that the model stratification is too strong or the surface density in the model is too low?

NO! The standard value in the model is 0.125 which stems from the time, when the simulated MLD was compared directly with the Levitus climatology. This value has not been changed in the standard code of MPIOM.

Page 12 top: There are also estimates of the budgets based on SOC/NOC analysis of Josey that might be useful to compare to here.

Not clear to me which paper is meant. I can either find a paper (JGR 2003) dealing with the Mediterranean focussing on interannual variability (especially winter) or papers dealing with the North Atlantic.

Page 13, first line. Is there river runoff from the Black Sea area through the Dardanelles into the model Mediterranean? If not, might this impact the results and be worth some sensitivity analysis.

No it is not. According to the literature the sea level of the Black Sea was lower than the sea level in the Mediterranean at the LGM. A reference has been added to the text.

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Page 20, line 21-22. Might be useful to clarify that this warm salty inflow through the Strait of Sicily is not LIW, unlike in the present day.

I do not claim that it is LIW. For the western Mediterranean it only matters that it is warm and salty and comes from the eastern Mediterranean.

Page 20, lines 11-12: Sentence needs to be rewritten.

Not clear to me which sentence is meant.

Section 5.4: This section, which is important given the comment on the issues of directly forcing the regional ocean model with the ESM needs greater detail, highlighting the issues and possible ways to improve on this in the future. As is, this section seems like a short late add-on that doesn't really discuss enough the point brought up by the author.

The section has been extended.

For the summary, it might be good to end with some discussion of where to go from here and how to best use the model results. Maybe also some discussion of where it might be useful to focus on for getting new measurements.

I added a bit more discussion..., but I am happy with this part of the paper, so I don"t want to change too much here.

Figure 1. Labelling at least one of the latitude/longitude lines is needed to orient the reader.

done

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