

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

**Anonymous Referee #1**

Received and published: 28 October 2010

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.

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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.

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 significance of this choice is needed.

Page 4, Line 23: Might be useful to mention the Rossby radius when stating the model is eddy-permitting.

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.

Page 7, top: If there is more convection, might not one expect a shorter turn-over timescale as more new deep water is being produced each year?

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.

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

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.

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.

Page 20, line 21-22. Might be useful to clarify that this warm salty inflow through the

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Strait of Sicily is not LIW, unlike in the present day.

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

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.

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.

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

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Interactive comment on Clim. Past Discuss., 6, 2005, 2010.