**Climate
of the Past
Discussions**

Interactive comment on "Upper ocean climate of the Eastern Mediterranean Sea during the Holocene Insolation Maximum – a model study" *by* F. Adloff et al.

Anonymous Referee #3

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This paper uses a numerical model to study the circulation of the Mediterranean during the Holocene, during the time of maximum summer insolation. The authors use 3 long simulations (a modern day control and two HIM scenarios) to look at the upper ocean circulation in the eastern basin. Although the authors do present a lot of figures howing temperature (and nothing on salinity), they present an interesting mechanism that links temperature signals with enhanced downwelling and wind mixing from strengthened Etesians.

However, potentially the most interesting aspect to this work is the attempt to use the discrepancies between the model fields and the paleo-proxies to suggest that the ap-

propriate temperature transfer functions should be based on temperatures integrated over a depth range rather than just SSTs. As my expertise is not in the area of proxies, I can't comment on the specific improvement suggested here. But to my mind, this type of result is the way that paleo models should be used...to make concrete suggestions on hypothesis's that can be considered by the observational/proxy community, rather than just using the models to present large number of hard to verify paleo output.

If the proxy idea is valid, then I would recommend this paper for publication with minor revisions, for it is otherwise well written and easy to follow. If the proxy idea is not especially useful to the observational community, then this paper becomes a long modelling paper focussing on one field, temperature...in which case, a more thorough analysis focussing on all model fields and the key model science questions would be needed.

Specific Comments:

- Would like more in the introduction with respect to other ideas for sapropel formation to put the work in better context.

- If the first level is 12 m thick, and is the thinnest, as is normal in models, I don't see how there can be 5 levels in th top 50 m (top 60 m if each 5 are 12m thick).

- Is one grid point for straits like Gibraltar appropriate? There is nothing in the paper on the boundary conditions, and thus whether this will work. Some more discussion of this may be useful.

- I think an experiment with freshwater provided from the Black Sea would be useful (although running another 700 years may be too much). For example, there are some good papers in this area by Lane-Serff et al. and Myers et al., based on simple modelling.

- Which bulk formula are used for the surface fluxes?

- I would not call 1950 pre-industrial!

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- The comment about model MLDs being around 300 m in the Levantine seems much too deep for LIW to my mind. This needs more validation, and if an issue, a discussion of how it may impact the model results/conclusions.

- With respect to the Nile runoff, is it based on pre-Aswan dam numbers for the control? Otherwise, a proper comparison for the right reasons may be hard.

- With respect to figure 18, and the discussion on page 1478, I don't necessarily see that the results with 9K2 are much better than those with 9K1. In fact, I would say I find these figures trying to compare the model fields and the proxies a bit hard to read.

- Given the potential significance of the suggestion to use an upper ocean depth range to examine the proxies (rather than SST), it seems to me that this idea is not highlighted enough in the summary. I would like to see more discussion here, including comments about ways to potentially go back and use this result to improve other relevant studies.

Interactive comment on Clim. Past Discuss., 7, 1457, 2011.