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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.

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First, I thank the reviewer for his/her constructive comments.

Please, note that the pages and lines references do still refer to the numbering of the first manuscript version.

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 appropriate 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.

Our introduction did not transmit an adequate message about the focus of the present study. For this reason, we revised our introduction trying to state our goals more clearly.

The purpose of the study was not to investigate the sapropel formation but to model the effect of insolation alone on the upper ocean state of the Mediterranean during the early Holocene. This is an essential first step preceding further studies where freshwater forcing could be added in a transient experiment to the insolation-driven model state in an attempt to simulate the inception of sapropel deposition. The investigation of the insolation effect alone proved sufficiently interesting to warrant publication, because it yielded a novel interpretation on proxy-based reconstruction of the basin.

Through the model analysis, we found interesting patterns and their associated processes which were relevant for present as well as for past climates and which had never been suggested in the literature before. These processes could explain the signal detected by the proxies.

The following step to this study will be to test different fresh water perturbations (e.g. opening of the Bosphorus, enhanced Nile runoff, freshening from the Atlantic, increase d precipitation) and investigate how does the deep ventilation react to these changes. However, as we explained above, the present study does not deal with the sapropel formation.

Nevertheless, because a discourse with the sapropel issue is inescapable, we added in the new version, more discussion on the salinity changes displayed in our simulations and their influence on the vertical stratification and thus on the deeper water ventilation (section 4.2).

This might have been not explained clearly enough, so we tried to improve the introduction in the new version to state our goal in a clearer way.



- 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 i f each 5 are 12m thick).

In the ocean model MPIOM, the first 6 layers are centered to the following depths: 6,17,27,37, 48 and 60, so the thickness of the first 5 layers are respectively 12, 10, 10, 11 and 12. The first layer is also subject to sea level variation so the thickness is fluctuating. I inserted a correction "5 levels in the upper 54 meters".

- 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.

In the reality, the narrowest width of the strait is 14.3 km. The resolution of one grid box is roughly 20 km. With the Arakawa C-grid which is used for the horizontal discretization, the use of one grid point is sufficient to obtain a good estimate of the transport. Moreover, we are quite satisfied with the modelled water transport, which falls into the range of the observations, this quantity being fundamental for a good representation of the Mediterranean system. We agree that this simplification could have effects on fine-scale features in the region immediately adjacent to the Strait, but this was not the focus of the present investigation.

Concerning the boundary conditions, it is stated Page 1463, Lines 1-6, that we have a sponge zone at the Atlantic boundary (over 5 grid boxes) where we perform a restoring to climatological hydrography. For the HIM simulation, we use hydrography + global model anomalies 9K vs. CTRL.

- 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.

This will be part of the next study dealing with investigation of the hypothesis for sapropel formation. In this next study, we will test diverse fresh water perturbations which could lead to a stagnation of the deep water (the opening of the Black Sea is one of the perturbation we will test). But to discuss this in the present study would not fit to the goal of the paper and would lead to an exceeding length.

Again, in the new version of the manuscript, we tried to improve the introduction to better explain the goal of the present study, only dealing with a stationary well-ventilated past climate state. The section 4.2 has also been developped, with a focus on changes in vertical stratification resulting from the changes in salinity.

-Which bulk formula are used for the surface fluxes?

*da Silva, A.M., Young, C.C., Levitus, S., 1994. Atlas of surface marine data, Vol. 1–5, NOAA Atlas NESDIS.*

This has been included in the new version of the manuscript.

-I would not call 1950 pre-industrial!

Our experimental design follows the the PMIP2 protocol for setting the pre-industrial global simulation. PMIP2 proposes to use orbital parameters which correspond to



1950 for the pre-industrial simulation, indeed the difference between 1750 and 1950 insolation induced by changes in the orbital parameters is negligible.

This explanation has been now implemented in the new version of the manuscript.

- 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.

We think that 300m is not too deep for the LIW. According to Somot (2005), the LIW is stabilizing in density at around 200/300 m depth and is responsible for a subsurface salinity maximum between 200 and 600 m almost everywhere in the Mediterranean basin. (More references: Ovchinnikov, 1984; Lascaratos et al., 1993, Ozsoy et al., 1993).

In the new version of the manuscript, we added an explanation on how is the mixed layer depth calculated in the ocean model.

- 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.

Yes, it corresponds to pre-Aswan dam runoff values (see Page.1463, Lines 24-25).

- 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.

In the new version of the manuscript, we added a table with standard errors (RMSD) and mean biases for (1) SST comparisons, (2) 0-30 m temperature comparison; each for both experiments 9K1 and 9K2. We also added some text to discuss these values in section 4.4.3.

Additionally, we removed Fig. 18 and Fig. 20 and replaced them by cross-plots. These cross-plots display for each experiment the fit between the reconstructions and the model data at each core location, for the absolute SST and 0-30 m temperature. The discussion in sections 4.4.2 and 4.4.3 is consequently slightly modified.

Both materials are supplying an easier estimation of the added-value of the new comparative method.

- 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.

Following your advice, the new version of the manuscript contains now more highlight and more discussion on the novel comparative approach proposed in our study (in the abstract, in the sections 4.4.2 and 4.4.3 as well as in the conclusion).