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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.

In contrast to reviewer 1, I'm a modeler and I don't work directly with proxies. So, my review will mainly dealing with the model and results section. The authors study the differences between present-day climate of the Eastern Mediterranean Sea between present-day and 9 ka by using a regional Ocean model that uses boundary conditions from a global coupled climate model. They perform two simulations for 9 ka: One with only orbital induced insolation differences (9k1) and also with additional lower CO2 and changed topography (9k2). The early Holocene is of course an important period due to due to the formation of sapropel 1 and therefore this study is very interesting and worthwhile reading. I especially liked the separation of orbital induced climate changes and the additional influence of 'glacial' boundary conditions clearly showing that insolation alone is not sufficient to explain the climate at 9 ka.. The manuscript is very well written and the mechanisms are clearly explained and thoroughly studied.

General comment:

My biggest concern is the detailed focus on SST while vertical (overturning) circulation and density are not shown. In the introduction the authors mention the role of density in sapropel formation but in the remainder of the article density is not discussed. Numerous articles emphasize that salinity (rather than temperature) induced density changes are at least equally, and probably even more, important for the stagnation of deepwater formation and consequently for the formation of sapropels. The authors' focus on temperature and mechanisms to explain the temperature pattern is more suited for an ocean (modeling) journal while I think that readers of Climate of the Past would be more interested in the influence of SST and SSS on sapropel formation. I'm aware that the authors might focus on stagnation and sapropels in future studies as mentioned in the last section. Nevertheless, I strongly suggest that the authors' add pictures and some discussion of density (at least at the surface) and of the overturning circulation (both north-south and east-west) in this paper. This gives the reader an idea of a (possible) influence of SST and SSS on deep water formation and the associated depletion of oxygen. To my opinion this would make this study more appropriate for Climate of the Past.

We agree that 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 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.

We totally agree with the referee that SSS changes are essential for inducing state with stagnating deep water. 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, increased 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

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and their influence on the vertical stratification and thus on the deeper water ventilation (section 4.2).

Specific comments:

P1459 Line 7: “Coinciding”. The authors could add here a short discussion about the lag between minimum precession (i.e., strongest summer insolation) at 11 ka and the midpoint of sapropel 1 at about 8 ka. What could explain this lag and why did the authors choose 9 ka instead of 11 ka for their simulations? To my opinion, 11 ka is really the ‘Holocene Insolation Maximum’.

The referee is correct in pointing out that the highest summer insolation in the NH occurred in the earliest Holocene. We here use the term HIM to clearly distinguish the time slice we use (note that we are actually using proxy data from a 1000-years slice of 8.5 - 9.5 ka) from the much more commonly used Holocene thermal optimum at 6 ka.

The interval of 9.5-8.5 ka BP has been selected to avoid influences of the cold and dry climate anomaly of 8.5-8 ka BP. It comprises the first and most intense peak of the Holocene monsoon maximum that marks the onset of the vegetation-determined Holocene climate optimum (10-6 ka BP), and spans the regional climatic optimum reflected in the speleothem record of Soreq Cave in northern Israel.

Taking an earlier time slice would bring us closer to the actual insolation maximum (however, the difference in insolation between 10.5 ka and 9 ka is negligible), but this happened too close to the Holocene/YD boundary and the region at that time was too much affected by the residual ice sheets and low sea level.

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We don't see the point for adding an explanation about the lag between minimum precession and the mid-point of sapropel, since we do not deal with the sapropel here (see the answer to the general comment). This issue has been dealt with in detail in numerous publication, most recently by Ziegler et al. (2010).

Ziegler, M., Tuenter, E., and Lourens, L.J., 2010, The precession phase of the boreal summer monsoon as viewed from the eastern Mediterranean (ODP Site 968): Quaternary Science Reviews, v. 29, p. 1481-1490

P1461 Line 2/3. Here a short summary of the physics in the model would be welcome. Only a reference to Marsland et al. is not sufficient.

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

P1462 Line 15: year 1950 is not pre-industrial. Shouldn't that be 1850? Or do the authors assume that insolation in 1950 is similar to 1850?

Our experimental design follows 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.

P1463 Lines 3-17: I don't fully understand the role of the NCEP reanalysis. Do you

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also use NCEP for the downscaling at 9 ka? If so, how can NCEP be suited for that? If not, how do you downscale for 9 ka? This should be more explained.

NCEP is used to obtain the regression matrix between the principal component time series of EOFs of (1) SLP anomalies and (2) Wind stress anomalies (both from NCEP, anomalies mean that the long term mean of the model SLP and wind stress has been substracted). Then for each of the 3 experiments, we project the SLP anomalies (with respect to the climate of our 'preindustrial' control run) from the global model onto the EOFs. Then we use the (NCEP derived) regression matrix to estimate wind stress anomalies from the obtained loadings. The wind stress anomalies are then added to the long term mean of the NCEP reanalysis.

As this procedure essentially is used to estimate a relation between SLP and wind stress, which is dynamically determined, the method is limited by 2 things:

1) A general change in topography. The changes at 9 ka are approximately 20m in height, should not be important. For other time slices like LGM this could be a problem.

2) A general change in the circulation, involving circulation patterns not present in the NCEP reanalysis. As we used daily data, the sample size is >9000 and thus the phase space of possible circulation patterns is very well sampled. The principal circulation pattern has not changed that much between 9 ka and 0 ka, but the likelihood of individual patterns has. But this is not a problem for the method.

This method has for sure a higher reliability than the direct use of the wind stress from a T31 atmosphere model with a resolution of 3.75 deg for a model with 20km resolution.

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The general changes in the circulation pattern, e.g. the amplification of the Etesian winds at 9 ka is simulated by the global model as well.

We noticed that the description of the method was not well-explained. We tried to improve it and make it clearer in the new version of the manuscript.

P1466 First the authors discuss winter (lines 7-12), after that the discuss summer (lines 12-18) and finally they return to winter (lines 18-24). It would be clearer if the third part would be added to the first part (so, first winter and then summer).

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

P1467 Lines 6-9. I don't understand why smoothing could explain the discrepancy. This needs more explanation.

A non-negligible amount of smoothing is introduced in the generation of the raw climatological data. A compromise is made, to keep some structure but avoid the noise. However, for the coastal regions, where there is quite a lot of gradient (strong structures, fronts created by inflow/outflow ...etc...), this results in a smoothing, which removes a big part of the structure.

Therefore, in these regions, the comparison of (1) the raw climatological data with low structure (interpolated on our model grid) and (2) the much higher resolution model data which shows strong gradients in the coastal regions can become critical and lead to stronger discrepancies when we plot the differences between model and interpolated climatological data.

This problem is not encountered in low gradient regions, but in regions with strong fronts such as the Adriatic or the Aegean Seas.

P1469 Lines 9-10: Why do cyclones propagate more southerly during increased seasonal cycle?

In the HIM global simulations, there is a strengthened SST gradient in winter over the North Atlantic. This leads to a shift of the cyclones to the south, with increased precipitation S of 40°N and decreased precipitation N of 40°N.

P1469 Lines 10-11: I don't see why the intensified North African monsoon causes more precipitation over the Mediterranean Sea. I'm sure that the extent of the African monsoon at 9 ka is not that large that the Mediterranean is directly influenced by monsoonal precipitation. Is it an indirect effect or just an error?

In the new version of the manuscript, we included 4 new diagrams to Fig. 6 with annual mean anomalies of P and E.

In fact, the modelled increased precipitation over North Africa almost reaches the Northeastern African coast, this is especially true for summer. However, over the Mediterranean Sea, the summer P increase is due to an enhanced recycling of the precipitated water.

This statement has been revised in the new version.

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P1470 Lines 5-6 The river Nile overcompensates the missing water from the Black Sea. I don't see that. If it overcompensates you should see changed in the freshwater budget, don't you mean 'compensate'?

This is right, we meant 'partially compensate'. This has been corrected in the new manuscript version.

Furthermore, as you mention in the introduction, it is not clear whether there was some in input from the Black Sea at 9 ka. If the fresh water budget with the missing Black Sea does not show large changes in the fresh water budget, the role of the Black Sea could be crucial. In a ideal scenario, you should run a 9 ka simulation with input from the Black Sea. However, I realize this might take a lot of (computer)time. In any case, you could at least discuss this matter in the discussion sector (which is to my opinion section 4, but this section is not called discussion). If you would include the Black Sea at 9k, what could happen with the freshwater budget and SSS?

This is exactly correct, but testing hypothesis for sapropel formation was not the goal of the present study. This will be done in the next study where we will test diverse fresh water perturbation which could lead to a stagnation of the deep water (the opening of the Black Sea is one of the perturbation we will test).

Section 4.3.2. As already said, although the explanation is very clear, this section is very detailed and maybe of less interest for the readers of Climate of the Past. I suggest that the authors shorten this section and also remove some figures (suggestion: remove figures 14, 15 and 17 and discussion about the difference 'wind stress' and 'wind speed' and strongly shorten the discussion about the role of the atmospheric heating).

In the new version, we removed Fig. 15, Fig. 17 and reduced the number of panels in Fig. 16 (please note, that the figures number refer of course to the former manuscript version). We also strongly reduced the discussion about the difference between 'wind stress' and 'wind speed'.

However, we believe that this section is of interest for the readers because the processes described allow explaining the signal detected by the proxies. It also deals with the full understanding of an important process which explains a feature of the Eastern Med. Sea for present and past climates. To our knowledge, it has never been studied before.

Concerning changes in the figures, we also removed Fig. 18 and Fig. 20 in the section dealing with the model-proxy comparison; and we included cross-plots instead. 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. This new material supplies an easier estimation of the added-value of the new comparative method.

Section 4.3.2, Figures 16i and 16m: There is still a insolation induced warming in the far north in JAS while for the 'open basin' the winter cooling dominates. Why is there no winter signal in the far north?

This is an artefact of the coarse resolution global model forcing, which has a strong "land signal" in the coastal regions.

Typographical comments:

P1467 Line 25: Add “is” between ‘models’ and ‘only’.

Done!

P1472 Line 9: I don’t exactly know to what “This” refers to.

It refers to “The reduced vertical near-surface temperature gradient in the western Levantine”. Corrected!

P1476 Line 5: Add “looking the effect” is not right.

Changed!

Figures

Figures 1 and/or 2: For the readers convenience the authors could add some labels at the locations of the seas they mention (e.g., Marmaran, Levantice, Adriatic, Aegean and so on). This would be easier for readers that don’t know the map of the Mediterranean Sea by heart.

We created a new Figure for this.

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Figure 5: There is a pronounced “warm anomaly” centered at 25N;30E in all diagrams. Why is that?

This warm anomaly corresponds to the response to the lower albedo in this region in comparison with CTRL (up to -0.18).

In both 9K global simulations, a shift of the vegetation cover of North Africa is simulated toward the North, explaining the lower albedo values (vegetation being darker than desert). This shift is particularly obvious in the region centered at 25N;30E, where grass vegetation is replacing desert (sand). This explains the stronger warm anomalies simulated there.

Figure 6: Differences in P-E between two runs are always hard to interpret. Maybe you could add 4 diagrams with annual mean anomalies of P and E.

4 new diagrams with annual mean anomalies of P and E are now included in Fig. 6. The text which refers to these plots has also been modified.

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