Clim. Past Discuss., 7, C1218–C1228, 2011
www.clim-past-discuss.net/7/C1218/2011/

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.

F. Adloff et al.

fanny.adloff@zmaw.de

Received and published: 12 August 2011

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.

Although I do not directly work with (paleo)climate models but rather on proxy based paleoceanographic records I reviewed with interest the manuscript authored by Adloff and co-workers. The manuscript presents and discusses new results from a regional

ocean general circulation model (OGCM) experiment forced by atmospheric input derived from global simulations. The OGCM experiment provides a reconstruction of the â€IJupper ocean climate of the Eastern Mediterranean Sea during the Holocene Insolation Maximum. My limited knowledge of several aspects of the commonly used (paleo)climate models prevents me from commenting on the model setup as well as on the other technical aspects of the model itself. I will rather focus my comments on whether this study is important or not for the "Mediterranean" climate and ocean communities and I will stress on those aspects that will need to be developed and/or improved, especially on those concerning the model-data comparison.

General comments

I think this manuscript investigates an interesting time interval that in the Eastern Mediterranean Sea coincided with the deposition of the sapropel S1. However, the Authors focus only on the temperature changes associated with the Holocene Insolation Maximum and neglect the contemporaneous changes in sea surface salinity that, according to proxy-based recnstructions, played by far a more important role in the deposition of the sapropel layers (including S1). I personally find it hard to argue that SST changes are so important for the water mass dynamics in a basin with a vertical circulation of water masses that is primarily "salt driven". However, if the Authors, as I sense, have a different view they should more clearly put it forward, or else the connection with the sapropel S1 deposition is hard to follow.

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

Contrary to previous studies (e.g., Myers et al., 1998 – Paleoceanography, 13, 586-606; Myers and Rohling, 2002 – Quaternary Research, 53, 98-104) the Authors do

not force their OGCM experiment with sea surface temperatures and salinities derived by early Holocene proxy-based reconstructions but use those reconstructions, which are derived from a manuscript in preparation, to validate their model. I do find this approach interesting but at the same time I think that manuscripts in preparation (i.e., Kucera et al.) – thus not (yet) subject to peer-review – should not be intensively referred to, let alone used to support/validate the Authors – conclusions as it is the case for this manuscript. Furthermore, I think that data-model comparison should be also discussed by means of cross-plots and not simply by means of contour maps as the Authors do. Only by using cross-plots the actual offsets between modeled and reconstructed properties can be visually and quantitatively assessed.

The comparison with assumption-free model output with independent proxy data is indeed the interesting aspect of this paper. Therefore, we cannot avoid citing the study in preparation because the reconstructions we use for the comparison are based on this study. We are aware that this paper has not been subject to peer-review yet, but the methodology has already been validated (Hayes, 2005).

Following your advice, we added a table with error estimation and mean biases and an associated discussion in section 4.4.3. We also included 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. Both materials are supplying an easier estimation of the added-value of the new comparative method.

I think that the sections presenting the results and those dealing with the discussion of the results should be kept separate to avoid confusing the reader. While reading section 4 I was thinking that those were the results, while in the end I realized that it was the discussion as section 5 deals with the conclusions already.

We decided to organize the paper with the discussion inserted into each section. Indeed, the investigation of the processes responsible for the subsurface warming and the proxy-data comparison are two parts quite independent from each other. For this reason, we think that it was better suited to organize it that way than adding a common 'discussion' section at the end, which would rather confuse the reader (to our point of view).

Finally, I note that the figures presented in the manuscript are very many (N = 21) and should be largely reduced (by at least 50-60 %. The same holds for the numerous acronyms that are used throughout the manuscript; they make it really hard to the reader to follow the story at times.

In the new version, we removed Fig. 15 and 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“.

In the part dealing with the model-proxy comparison, we removed Fig. 18 and Fig. 20, and replaced them by cross-plots.

The extensive use of acronyms is justified as it makes the sentence structure easier to follow and reduces the extent of the text significantly. We believe all acronyms have been explained clearly and their number does not exceed what is typically used in oceanographic papers.

Specific comments

Page 1459, Lines 1-6: It should be noted that large part (if not all) of the domains of the boreal summer monsoon witnessed an intensification during the early to middle Holocene period and not simply the North African Monsoon. This statement should be revised in my opinion.

In the new version, this statement has been revised following your advice.

Page 1459, Lines 11-15: I think there is broad consensus on the fact that the sea surface freshening (e.g., Rohling et al., 2004 – Marine Micropaleontology, 50, 89-123) rather than sea surface warming reduced/suppressed the deepwater formation processes during sapropel deposition. I found this statement somewhat misleading (see my general comment).

We are aware of that, nevertheless some studies (e.g. Emeis 2003) also considered the warming as one of the agents invoking surface density decrease (together with the freshening), so we found it appropriate to mention it as one of the hypothesis in the introduction. However, according to your comment, we modified the new version and only mention the freshening.

Emeis, K.-C., et al., Eastern Mediterranean surface water temperatures and d18O composition during deposition of sapropels in the late Quaternary, Paleoceanography, 18(1), 1005, doi:10.1029/2000PA000617, 2003

Page 1461, Lines 16-25: I would suggest that the Authors update their list of references concerning the Mediterranean-Black Seas reconnection by looking at the study authored by Soulet et al. (2011, Quaternary Science Reviews,1019-1026).

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

Page 1469, Lines 11-12: In summer, the intensified North African monsoon is responsible for enhanced P over the Levantine Sea:

I think there is clear evidence from a wealth of paleoclimate archives from the Middle East and Red Sea (e.g., Arz et al., 2003 – Science, 300, 118-121) that "monsoon moisture" never reached the Mediterranean borderlands during the Holocene Insolation Maximum. I think that the Authors should discuss this point more in detail also taking into account the relevant literature.

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.

So, consistently to your point, we modified our statement in the new version.

Page 1478, Lines 22-29: I think the Authors should provide some more information on the planktonic foraminiferal species that have been used to generate the transfer function reconstruction of the annual and seasonal SSTs that they compare to their model data. Differently from what happens during the winter season, most of the planktonic foraminifera inhabiting the eastern Mediterranean in summer are symbiontbearing

spinose species such as, e.g., Globigerinoides ruber and Globigerinoides sacculifer. These species due to their symbiont-bearing character dwell at the very top 20-50 m of water column, as they need light for their photosynthetic symbionts. As the Authors state, the major disagreement between model and proxy data is restricted to summer SST: and I am not so convinced that can be entirely explained in terms of habitat of the planktonic foraminifera used to derive summer SSTs. In my opinion this point should be clarified and/or discussed in more detail.

The foraminiferal assemblages in the eastern Mediterranean during the HIM were indeed dominated by shallow-dwelling spinose species, including G. ruber, G. bulloides, G. rubescens, G. tenella, G. siphonifera and O. universa. The habitat of these species is known to be limited to the mixed layer, which typically corresponds to the photic zone. It is thus correct to argue that the transfer function results cannot be explained by processes occurring at subthermocline depths. But this is not what we are proposing. The transfer function used to convert the assemblage counts to SST has been calibrated to SST at 10-m depth. The habitat of the species represented in the assemblages is clearly deeper than that and the interpretation we use (0-30 m average) is entirely realistic. The scenario we favor is one where the mixed-layer community of dominantly symbiont-bearing planktonic foraminifera has been exposed in the area S off Crete to a "no-analog" water column structure. The surface temperature by itself did not become higher, but the thickness of the warm layer increased. The foraminiferal community changed in a manner that the transfer functions interpreted as an increase in temperature at the level to which they were calibrated (10 m). We argue here that this strict depth-level attribution of the warming signal by the transfer functions is not necessarily correct, because of the "no-analog" upper water column structure. If such upper water column structure is not sufficiently represented in the calibration dataset (because it does not occur today), then the transfer function for obvious reasons cannot reliably extract the actual SST signal from such assemblages.

Page 1479, Lines 1-29: I think that comparisons between instrumental measurements and proxy-based reconstructions of ocean properties (in this case SSTs) are often extremely challenging. Generally speaking, most proxy-based reconstructions seem to provide fairly reliable assessments of the magnitude of change between two time intervals (e.g., between Last Glacial Maximum and Holocene) but fail to accurately reproduce the instrumental record across the last decades and/or centuries. Mostly, this is due the nature of the sedimentary record itself, the internal natural variability within each sediment sample analyzed, and the calibration uncertainties. I wonder if the Authors, instead of comparing the reconstructed SSTs for the 9.5-8.5 ka BP interval, could make a comparison with the temperatures that are obtained from foraminiferal transfer functions obtained from Mediterranean surface sediment samples (i.e., modern). In my view this would be by far more comparable to the 9K1/9K2 vs. CTRL simulation comparisons that the Author discuss. This applies also to section 4.3.5.

The transfer function calibration, as presented by Hayes et al. (2005) shows no significant spatial bias. This means that planktonic foraminiferal assemblages in modern coretops throughout the Mediterranean Sea when converted to SST using the ANN method yields values randomly deviating from the actual SST in the region where they were deposited. Unfortunately, for many of the cores used to derive the transfer-function SST, there are no coretop data or the coretops have been disturbed and are not modern. However, Fig. **??** shows that the HIM SST pattern derived from the transfer functions is robust with respect to the process raised by the referee, because in the region of interest, all coretop assemblages yielded deviations from the actual SST smaller than the RMSEP of the calibration, whereas the HIM SST anomaly exceeded the RMSEP of the proxy calibration.

The caption of the figure is: "Spatial structure of summer (JAS) SST residuals (ANN average of ten best networks – WOA98). Yellow circles indicate coretops with residuals

within +/- 1 RMSEP (1.14°CC), red circles indicate samples where the ANN method overestimated the instrumental WOA98 value by more than 1 RMSEP and blue circles indicate samples where the ANN method underestimated the instrumental WOA98 value by more than 1 RMSEP. Note the ANN method did not produce any systematic bias in the region centred on Crete, where the HIM warm anomaly has been observed."

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

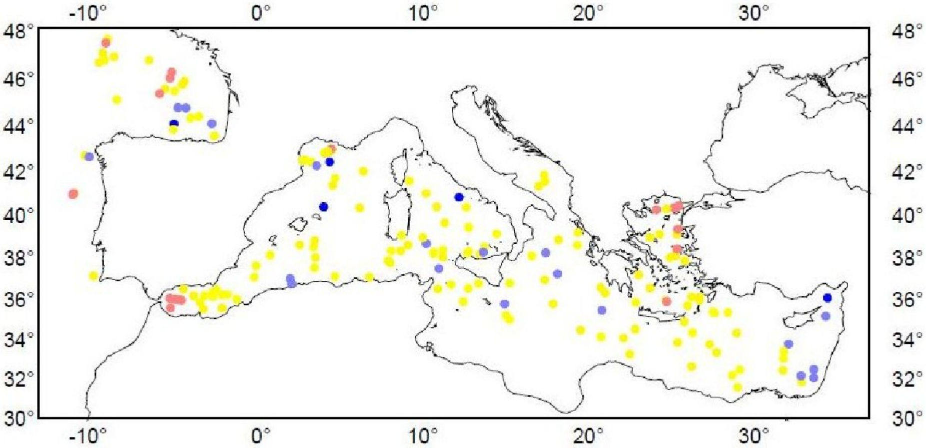

Fig. 1.