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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.

Anonymous Referee #1

Received and published: 29 June 2011

Dear Editor, Dear Authors,

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 'upper 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

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Interactive Discussion

Discussion Paper

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.

Contrary to previous studies (e.g., Myers et al., 1998 – Paleoceanography, 13, 586-606; Myers & 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.

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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.

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.

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.

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).

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).

Page 1469, Lines 11-12: '...In summer, the intensified North African monsoon isresponsible 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.

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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 symbiont-bearing 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.

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.

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