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Earth System
Dynamics
Discussions

Interactive comment on "Iterative land proxy based reconstruction of SST for the simulation of terrestrial Holocene climate" *by* K. Haberkorn et al.

Anonymous Referee #2

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The authors describe a method on how to adjust SST changes in the North Atlantic to reach local temperature anomalies over Europe. They use a simplied AGCM with prescribed SSTs and orbital forcing for the Holocene. Target is a high-resolution time-series (O-18) from the Ammersee, Germany. One aim of the presented manuscript is a feasibility study to correct SSTs for past climates.

Rating: The understanding of Holocene climate variability is of high importance. The relative changes of SST and land temperatures are interesting. The methodology of adjusting the SSTs seems to be not appropiate. The annual mean insolation forcing for the Holocene shows a warming at mid- and high-latitudes and a cooling at low latitudes. The correction applied 0-60° N seems therefore not well chosen. The model shall be also run with a mixed layer ocean at least in oder to estimate the effects of the

(artificial) fixed SSTs onto land temperatures.

I have several remarks, some of them are just related to the wordings and formulations, some of them are related to unprecise science or lack of knowledge.

1) Abstract, line 5: "reconstructing SSTs" is promissed too much

2) Abstract at the end: Influence of ice sheets onto the circulation is not the topic.

3) Introduction, page 151, line 12: "Driven by reconstructions..." is not a proper wording

4) Introduction, page 151, line 24 ff: internal variability is not the topic of the paper, some sentences are unrelated to the work. Same with DA and NWP!

5) page 153, line 5ff: Ocean circulation changes are not related to the topic

6) page 154, line 1ff, larger framework is interesting, but not at all related to the topic. My feeling is that the manuscript is embedded in an interesting research project, but of little relevance for this.

7) Model set up: As mentioned above, the approach seems not very useful. Instead many citations of work in the group are listed dealing with the same AGCM, but unrelated work (e.g. Snowball Earth...).

8) 2.2 Paleo data Most of the section is not important for the paper. It is also not excact. Does the O-18 variation reflect temperature or precipitation ? The effects are not properly explained, local precipitation, temperature, seasonalities etc. One could work on that in terms of meteorology: What are the driving mechanisms for the local proxy?

9) page 158, line 5ff: a relationship between annual mean temperature and annual mean precipitation is not necessary for the suitability of the proxy.

10) page 158, line 22ff: The 8.2 event seems to be not in the focus of the manuscript.

11) in the following pages: Much text (partly nice formulations), but not focussed on the

particular problem.

12) several minor things like page 161, line 22ff: In the varability analyses, no phase information is included.

13) Proxy SSTs: they can reflect different seasons, more in-depth knowledge of the recorder systems are required. also on other places in the manuscript...

14) page 163, line 10: this "switch" is not correct und unprecise

15) page 164: as mentioned above, the sensitivity is most likely not suitable for the approach. Some of the cited papers are not well understood. I miss the preliminary set of sensitivity studies (line 14)

16) page 166: not clear.

17) results are not convincing. What is the response in a GCM coupled to a mixed layer (or interactive) ocean?

18) Many misleading sentences, e.g. page 171, line 19 about millenial variability. Or the citations of assumptions in line 12ff page 175.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 149, 2012.