Clim. Past Discuss., 9, C2472–C2475, 2013
www.clim-past-discuss.net/9/C2472/2013/

Climate of the Past Discussions

Interactive comment on "Assessing the impact of Laurentide Ice-Sheet topography on glacial climate" *by* D. J. Ullman et al.

D. J. Ullman et al.

dullman@coas.oregonstate.edu

Received and published: 30 October 2013

We thank Anonymous Referee 2 for her/his insightful review on our manuscript. These comments support and agree with those of Dr. Siddall, further motivating our revision of the structure of the paper. We agree that much of section 3 could be moved to a supplementary section, with the most salient points summarized in the main text. This "option D" as expressed by referee 2 is a helpful suggestion to improve the structure of the manuscript.

Other major comments

Please show statistical significance for anomaly plots. Could be done through stippling, or just not coloring the non-significant regions.

We will improve the figures so as to provide a better assessment of significance in the color bars, particularly in those figures that highlight the differences between reconstructions. We will use natural model variability to assess 95

Regarding interactive comment on relation to PMIP3 simulations. I, too, am a little confused about how these simulations relate to PMIP3. It sounds like the 21ka-5G and the 21ka-L simulations were the GISS contribution to PMIP3 LGM simulations. One was r1i1p150 and the other was r1i1p151? I had originally assumed that there was a third simulation using the PMIP3 ice sheet that was then contributed to PMIP3. I think it would be useful to give the CMIP5 ensemble names (r1i1p150 and p151) in the paper.

As of right now, there is no GISS simulation using the PMIP3 ice sheets. However, we will note the CMIP5 ensemble names in the text (r1i1p150=Ice-5G; r1i1p151=Licciardi et al.)

Minor comments Pg. 3245, line 14: Change "extend" to "extent"

Will change to "extent"

Pg. 3245, Line 25: Why the maximum reconstruction? Isn't the purpose to use a minimum reconstruction?

Both reviewers have expressed confusion on our use of the "Max" Licciardi et al. reconstruction as the lower bound. We will elaborate on our reasoning for using this reconstruction (see earlier response to Dr. Siddall).

Pg. 3247, line 20: "Additionally, cold surface temperatures drove sea ice growth requiring an expansion of the land mask." Don't understand why this required a change to the land mask – aren't land masks fixed based on the desired sea level?

The growth of sea ice required a change to the land mask because cold sea surface temperatures created perennial ice that froze to the bottom of ocean grid – effectively making an ice shelf, which is elsewhere treated in the model as land ice. In these regions, we converted these ocean grid cells to land cells. We will clarify this phe-

nomenon in the revised manuscript.

Pg. 3248, line 17: Missing "ka" after "21"

Will add missing "ka."

Pg. 3250 Lines 15-20: There is also the synthesis by Bartlein et al. 2011 Climate Dynamics 37:775. Some of their sites suggest warming in Beringia at LGM, though of course there is the no-analogue issue.

We agree that a comparison with Bartlein et al. (2011) is a necessary component of this paper as the main synthesis of proxy data from Beringia. We also agree that there may be a no-analogue issue with a number of the records in this synthesis, and we will note this in the call for more proxy data from the region. Even so, the larger differences between simulations appear to occur more over eastern Siberia, where data are more limited.

Pg. 3252, Line 28: Word missing "with some of the in the tropical. . ."

Should have the word "anomalies" to read ". . .with some of the anomalies in the tropical. . ."

Pg. 3255, Line 25: Delete "-L" from "21ka-L"

Will fix this so that it reads "21 ka"

Table 2: Dykoski misspelled.

Will correct spelling

Figure 2a, 3a, 4a, 6a: The white color is not aligned with zero, so that some regions of cooling are unintuitively colored yellow.

We will modify the color bar of these figures with something more intuitive (so that white is not at a non-zero value).

Figure 2e, 3e: How is atmospheric jet speed defined? I assume it is a wind speed at

200 mb height or something like this?

The atmospheric jet wind speed is defined as the wind speed at 250 hPa. We will edit figure caption to note this definition in the model.

Figure 5 caption: The caption mentions uncertainties for the 21ka simulations? I don't see these. Or, maybe, the sentence should say: "Longitudinal transects of SST anomalies averaged across the Atlantic and Pacific basins for the 21 ka simulations, with comparison to published MARGO data and uncertainties."

We will modify the caption of Figure 5 in accord with the reviewer's suggestion.

Interactive comment on Clim. Past Discuss., 9, 3239, 2013.