Review of “Paleometeorology: visualizing mid-latitude dynamics at the synoptic level during the Last Glacial Maximum”, by Unnterman et al.

General comments

This paper presents a high temporal resolution “synoptic” simulation of the LGM that has been run in tandem with a lower resolution “climatology” run. The purpose of the paper seems to be threefold: (i) to present a high resolution animation tailored towards visualizing synoptic systems, (ii) to compare the “synoptic” LGM run with the “climatological” LGM run, and (iii) to understand the LGM atmospheric dynamics and (modeled) climatological features. The author’s are very successful at purpose (i); the animation allows the reader to visualize the synoptic dynamics in way that would be unachievable through other mediums and I appreciate the time and effort it took to make the animation run the way it did. Perhaps an accompanying animation for the modern simulation with identical colorbar and vector scales would allow the reader to compare and contrast the LGM and modern synoptics.

Unfortunately, both the presentation and content of purpose (ii) are not compelling. The differences between the “synoptic” and “climatological” runs are not documented; the abstract states that there are differences are in temporal resolution but the details are missing. It is unclear what additional dynamics might become important with the reduced time step. Do results differ simply because the physical parameterizations that are honed and appropriate for the “climatological model” resolution are not re-tuned (and hence are inappropriately) to the “synoptic resolution” model? The authors note the data are archived hourly and the possibility that this may improve our understanding of climate. The paper does not present any examples of such benefits, however. In fact, the only analyses that utilize the hourly archiving are the animation and the feature tracking statistics. No animation or feature tracking statistics are presented for the lower resolution “climatological” run.

In regards to purpose (iii), the findings in this study concerning the differences between LGM and modern day climate are not new, and confirm results from many earlier studies using lower resolution models (T31, T42) using uncoupled (eg. PMIP1) and coupled models. These results include the well-documented and well-understood warm conditions in Alaska during the LGM due to the stationary wave induced by the Laurenttide Ice Sheet (which is well resolved at T42), and the reduced synoptic variance in the Atlantic during the LGM compared with the modern climate.

The computational demands of the high temporal resolution model precludes long integrations to ensure statistically significant results concerning differences in the simulated LGM and modern day climate. Storm tracks vary greatly in position and strength from one winter to the next, and combined with the very short integration, this brings into question the statistical significance of the results presented. Unfortunately, the analysis of statistical significance presented in the paper is not adequate or rigorous.

In summary, the paper lacks a rigorous statistical analysis and makes claims concerning the impact and importance of the simulated climate to the temporal resolution of the model but does not present results to support those claims. All of the results concerning the differences in the LGM and modern climate that are simulated by the synoptic resolution model have already been well documented in the literature using much longer, coupled and uncoupled climate models. I recommend the authors abandon the present manuscript and consider a new manuscript that is focused on the single new and interesting result (that is not explained and poorly documented in the current paper) -- the apparent tight relationship between the storm paths and sea ice edge that is evident from the excellent and very worthwhile animation. A publishable paper on this topic might include an analyses (not included in the present paper) of the structure and energetics of the storms (and how this differs in the low resolution model) and could evaluate the hypothesis for why the storms seem to track so closely the sea ice edge in the LGM climate (but not so in the modern climate).

Specific comments

(1) What is the temporal resolution of “climatological” and the “synoptic” runs? Though this seems like a simple omission, the comparison of the runs is the main focus of the manuscript and this would be useful information. It would also be useful to see a discussion regarding what physical mechanisms would be expected to change with the enhanced resolution. I thought the time step in the GCMs was chosen to give numerical stability (CFL conditions) so I’m not sure why we should expect the model behavior to change as the

temporal resolution is increased beyond the stability criteria. Please state how the paramerizations were re-tunded to be appropriate for use in a higher resolution model.

(2) Page 1887, line 9: at what level is the vorticity tracked?

(3) Figure 3 could be replaced by an F-test.

(4) Page 1888, lines 4-5: Changes between the LGM and modern or between the “climatological” and “synoptic” runs? There is a single animation so it’s hard to appreciate what causes the change between simulations. Furthermore, what physical mechanism is responsible for the eddies changing the mean state? It seems to me that the more obvious relationship is the mean state steering the synoptic features.

(5) Page 1888, lines 7-14. It is unclear whether the authors are suggesting the mean flow or eddy heat transport is responsible for the warm conditions in the Gulf of Alaska. The point is further confused by the statement that there is enhanced poleward heat transport in the region when it is the eddy heat transport divergence (on spherical geometry) and not the heat transport itself that would lead to local heating. An analysis of the mean versus transient eddy (most likely defined from a temporal high pass filtered) heat flux divergence changes, relative to the modern day simulation, in the region might clarify the cause of the warming in the area. Overall, this result has been known for decades (using lower resolution models), however, and the analysis presented in this manuscript offers no further insight.

(6) Are the same storm magnitude distributions found in the SLP based tracking? Given the single season of data, are the storm magnitude distribution differences (between the LGM and modern) significant. A Kolmogorov-Smirnov test on the cumulative distribution functions of storm magnitude would be well suited to assessing the significance of the results.

(7) Are there significant differences between the tracking statistics between the “synoptic” and “climatological” simulations? This could potentially prove to be a reason to archive output at hourly intervals.

(8) Similarly, are there any differences between the Eulerian variances defined from the hourly archived fields as compared to the archiving interval of the climatological run. It seems that the SLP spectra are very red and that including information at the hourly interval would have a negligible influence on the synoptic statistics.

(9) The abstract mentions that the hourly saves were motivated by an attempt to resolve diurnal features, are there any examples of diurnal features in the simulation that would be poorly resolved at six-hourly archiving?

(10) Page 1889, first paragraph. Is the upper level flow dominated by a ZONAL wavenumber 1 or 2 response? It seems to be wavenumber 2 to me, especially since the authors are calling on the mechanism to explain Northward flow in both the North Pacific and North Atlantic sectors. Also, it's unclear what dynamical mechanism is being called upon to explain the "compensatory" Northward flow in the North Atlantic region; this seems to rely on an eddy feedback that is not explained.

(11) Page 1889, lines 20-26. Donohoe and Battisti (2009) attributed the reduced North Atlantic storm activity during the LGM to weaker (magnitude and number) seeding of the baroclinic region. A discussion of those points as well as performing similar analysis on the T170 simulation would be relevant here. The authors are in a better position to test the seeding hypothesis given the much finer spatial and temporal resolution of their output.