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Climate of the Past Discussions

Interactive comment on "LGM climate forcing and ocean dynamical feedback and their implications for estimating climate sensitivity" *by* Jiang Zhu and Christopher J. Poulsen

Andreas Schmittner (Referee)

aschmitt@coas.oregonstate.edu

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The authors present climate model simulations and analysis regarding climate sensitivity, radiative forcing and feedbacks in the LGM. Simulations with fixed SSTs and a slab ocean model and a model with a fully dynamic ocean component are used to analyze forcing from greenhouse gases and ice sheets. Differences between the fixed SST and slab ocean model results give "effective radiative forcings" and "efficacy" of forcings. The method is quite complicated (at least for a person not intimately familiar with these concepts). This also makes for difficult reading. I find it a little confusing that some of the so called forcings include what would be traditionally considered feed-

backs, e.g. the longwave response to temperature changes illustrated in Fig. 2 would have traditionally been considered a feedback. But I guess this is the purpose, to separate effects beyond the traditional forcing/feedbacks concept. Despite the difficulty for a general reader I think technical papers like this are important because they advance the science by adding more detailed information. For this reason, I support the publication of this paper, perhaps in a slightly modified form.

An important conclusion is the ocean dynamical feedback, which the authors suggest increases climate sensitivity.

This is a pure model analysis without consideration of observations. This is fine, but I think the paper could benefit from some additional discussion regarding observations. E.g. the shortwave forcing from ice sheets certainly depends on the cloud cover simulated in the preindustrial (PI) model over regions that become ice covered in the LGM. E.g. if cloud cover and thus planetary albedo is high already at PI it won't change as much by adding ice compared with a low cloud cover/albedo case. I think this is a major source of uncertainty in determining the LGM shortwave forcing from ice sheets. It could be addressed by comparing model's cloud cover/albedo with present day observations in those regions. So, I suggest the authors to think about this and perhaps include a discussion about it in a revised version. If possible, a model data comparison would be useful, but it is not necessary. (My philosophy of reviewing papers is that the reviewer should not demand additional evidence, but evaluate the evidence presented.)

Another more major issue is the effect of sea level drop. It is not mentioned in the paper, but I think it should be, because the topographic effect of adding ice to the continents is accompanied by the effect of lower sea levels. Was this considered in the simulations presented here? In other words, was atmospheric mass conserved?

Another issue is the vegetation response, which apparently is fixed at present day. This will presumably affect land temperatures and thus the calculations presented in the paper. A discussion is warranted. E.g. it is well known that during the LGM much

of the present day boreal forest was converted to tundra. This would affect the surface albedo not only directly but also by modifying the effect of snow on the surface albedo.

Minor comments: numbers indicate line numbers 55: consider including Broccoli 2000 J. Clim. 13, 951pp and Schmittner et al., 2011 doi: 10.1126/science.1203513

102-103: The authors compare their model with new results from Tierney et al., which is fine, but I think they should also compare to previous results e.g. the MARGO SST compilation. Also, I think there is some circularity here because I think Tierney et al. used the CESM model data for their temperature estimates, so they are not independent. I'd also suggest comparing their model's whole ocean temperature change with data from Bereiter et al., (2018, Nature 553, 39–44). It may also be useful to show or note the trend in whole ocean T if not in equilibrium.

105: Sherwood et al. should be (2015) I think

111: "active land model" not quite correct if vegetation is fixed

127: is the correction applied locally or globally?

225: "over high-latitude regions" Looking at Fig. 3c,f, I don't see large changes at high latitudes. I see more negative values for lambda over the western tropical Pacific and more positive values over the eastern subtropical Pacific. Would be useful to elaborate on this more.

234: "0.31" shouldn't this be 0.1? Does it refer to the CLD column in SOM_GHG?

252: typo: the "importance"

296: consider adding Schmittner 2003 EPSL doi:10.1016/S0012-821X(03)00291-7

311: add "in the model" to clarify that this statement refers to the model, not the real world

362: "high latitudes" see my comment above

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