Anonymous Referee #2

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 feedbacks, 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.

Reply: We thank Reviewer 2 for the comments. We agree that part of the manuscript is technical, which, we think, should not be shortened, to ensure the reproducibility of our work. Importantly, to our knowledge, our manuscript is the first to adopt the new forcing-feedback framework of Sherwood et al. (2015) in a paleoclimate study, which is another reason we need to document the methodology in detail.

We emphasize that the methodology described in our study has advantages over the traditional methods in the quantification of LGM forcing and feedbacks. First, the use of the new framework (with the concept of effective radiative forcing and adjustments) fits better with the concept of climate forcing and response, i.e., the global radiative response being a linear function of surface temperature change and the forcing being the radiative forcing without any change in global mean surface temperature. Second, our method provides an effective way to quantify the magnitude and effectiveness of the ice-sheet forcing, which are important for a complete understanding of the LGM climate and the constraint on ECS.

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.)

Reply: We agree with the reviewer and will add a short discussion regarding model-data comparison on the ocean circulations in the revised manuscript.

As for the cloud cover in PI simulations, we note that the existence of LGM ice sheets will make the clouds aloft thinner by occupying the space that would otherwise be taken by air masses and clouds. In other words, the cloud masking effect will be smaller at the LGM than expected from PI conditions, because clouds above ice sheets are thinner due to the existence of ice sheets.

Satellite observation shows a planetary albedo of 0.3–0.4 over the North America (see the SERES images here: https://ceres.larc.nasa.gov/resources/images/). CESM1 well-reproduces the observation with a bias less than 0.05. Changes in planetary albedo over the North America caused by the LGM ice sheets are larger than 0.3 in the model simulation. This result indicates that LGM ice sheets still provide a dominant impact on planetary albedo.

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?

Reply: The effect of sea level drop has been included in our LGM simulation and the quantification of ice-sheet forcing/efficacy. The atmospheric mass was conserved. We will clarify these points in the revised manuscript.

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.

Reply: The definition of Charney Sensitivity does not include vegetation changes, effect from which is considered as climate forcing by PALAEOSENS-Project Members (2012). The benefit of fixed vegetation in our simulations is that we can focus on the classical forcing-response processes (including clouds, water vapor, lapse rate, and snow/sea ice) within the framework of Charney Sensitivity. In the present manuscript, we point out that, even without the consideration of vegetation changes, the forcing-response processes within the atmosphere-ocean-sea ice coupled system is complicated enough that caution should be exercised when directly estimating ECS using paleoclimate data.

We will add a short discussion in the revised manuscript pointing out that forcing-feedback processes associated with the LGM vegetation and aerosol is less understood and worth further study.

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

Reply: Thanks. These will be added.

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.

Reply: Tierney et al. (2020) provided two estimates: a data assimilation result of –6.1°C (–6.5 to –5.7) and a data-only result of –5.6°C (–6.8 to –4.4). We used the data-only result for the model-data comparison to avoid any circularity.

The global volume-mean ocean temperature in the LGM simulation decreased by 0.15°C during the last 900 years. We will add this information in the revised manuscript. We do not think the comparison of the modeled whole ocean temperature with Bereiter et al. (2018) offer new insights regarding our findings, as our study uses CESM1 in a "perfect model" scenario to explore the assumptions associated with estimating ECS from knowledge of paleoclimate forcing and response.

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

Reply: Yes, this will be corrected.

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

Reply: The "active land model" means that the land surface temperature is free to evolve, unlike the fixed SST. We will clarify this in the revised manuscript.

127: is the correction applied locally or globally?

Reply: The correction is applied locally. The global mean of ERF is independent on whether the correction is 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.

Reply: Thanks for pointing this out. To better show the state dependence of the cloud feedback, we will add a plot of the zonal mean value.

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

Reply: The value should be 0.21 and it refer to the CLD column in SOM_2CO2. We will correct this.

252: typo: the "importance"

Reply: It will be corrected.

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

Reply: It will be added.

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

Reply: Thanks. It will be added.

362: "high latitudes" see my comment above

Reply: We will add a plot of the zonal mean to better show the differences over high latitudes.