Anonymous Referee #1

This paper describes experiments performed with the CESM model and their analysis with respect to a deeper understanding of LGM climate and related calculations of equilibrium climate sensitivity (ECS). To my knowledge no such in-depth analysis has been performed so far, so I believe this is a great step forward.

I fully support publication and have mainly minor comments, dealing with details and representations. The discussion of the Southern Ocean stratification and of the resulting climate sensitivity need to get enhanced. However, my comments should be all addressable by the authors with little efforts.

Reply: We thank Reviewer 1 for the positive assessment of our manuscript. In the revised manuscript, we will also address all the insightful comments.

Comments, in order of the written text:

1. lines 29–30: Please discuss GCM responses in the context of ECS analysis for longer than 100 years, e.g. based on LongRun-MIP (Rugenstein et al., 2019, 2020). Update the 1.5–4.5K range from the IPCC with results from the most recent review showing 2.6–3.9 K (66% confidence interval) (Sherwood et al., 2020).

Reply: Thanks for the suggestion. We note that we do not discuss models and model-based ECS estimates in the first paragraph of Introduction. Therefore, we will not mention LongRun-MIP results in Introduction.

We will revise the sentence as follows: "ECS range is estimated to be 2.6–3.9°C (66% confidence interval) in a recent assessment (Sherwood et al., 2020), which represents a narrower range than the traditional one of 1.5–4.5°C (IPCC, 2013)."

2. line 38 and later: The reference "Rohling et al (2012)" should be cited as "PALAEOSENS-Project Members (2012)".

Reply: We will make this change in the revised manuscript.

3. throughout the draft: The way papers with more authors are cited differs. Eg. line 43 says "Stap, Köhler & Lohmann, 2019", but the same paper is addressed as "Stap et al., 2019" later in line 59. Please homogenize the reference style to what is requested by the publisher.

Reply: The citation is automatically generated following the APA style. We believe this will be homogenized during typeset by the publisher.

4. line 52: To my knowledge "surface topography" should be included in Friedrich et al. (2016), please check.

Reply: We have checked and found that Friedrich et al. (2016) did not consider temperature change that is directly attributed to changes in surface elevation, i.e., "surface" is defined at different elevation between the preindustrial and LGM.

5. line 53: What about sea level-based elevation change? Figure 1b does not show that a sea level drop was considered (e.g. nothing seen in Sunda Shelf or Bering Strait). Maybe change color bar resolution for negative values.

Reply: Sea level-based elevation change is on the order of ~100 m and the effect has been included as changes in land-sea mask in our simulations. This can be seen in the surface albedo change (including Sunda Shelf and Bering Strait) in Figure 1a.

6. line 74: The effect of sea level is not mentioned here. Clarify if it is included or not.

Reply: Thanks. Our simulations have accounted for the effect from sea-level change between the preindustrial and LGM. We will clarify this in "Method, model, and experiments" by adding the following sentence: "LIS forcing is derived from the ICE-6G reconstruction (Peltier, Argus, & Drummond, 2015) and includes changes in land elevation and surface properties due to the presence of LGM ice sheets, as well as changes in the land-sea mask to account for the lower sea level at the LGM".

7. lines 82–84: The list of past climate studies using this model is rather long, please reduce to some essentials or examples.

Reply: The list will be shortened.

8. line 99: "a different atmosphere model". Which model?

Reply: We meant the change of the atmosphere model from CAM4 to CAM5. We will clarify this in the revised manuscript by saying "an updated atmosphere model".

9. line 102: It is stated that –6.8 K obtained in FCM_LGM is in agreement with Tierney et al. (2019). This is not true, since Tierney finds an LGM cooling of – 5.9K (–6.3 to –5.6K, 95% CI), so what is found here is 0.5K below the 95% CI of Tierney.

Reply: The is a good point. Tierney 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, as the data assimilation results contains information from CESM.

We will change the sentence as follows: "GMST in FCM_LGM is 6.8°C lower than that in FCM_PI and falls within the range directly estimated from proxy data in a recent study (–6.8 to –4.4°C; Tierney et al., 2020)."

10. line 123: Why are simulations run only for 30 years, if fast feedbacks should consider the changes of the first century? Can you state the final radiative forcing imbalance at TOA as a convergence criteria?

Reply: We run the "fixed-SST" atmosphere-only simulations for 30 years, following the Radiative Forcing Model Intercomparison Project protocol (e.g., see Pincus, Forster, & Stevens, Geosci. Model Dev., 9, 3447–3460, 2016). 30 years are long enough to remove the internal climate variability and produce robust estimates of effective radiative forcing, especially when SSTs and sea ice are prescribed. For the reasons that SSTs are fixed in the simulation, the simulations do not have a convergence problem and therefore the TOA imbalance is not shown.

11. line 128ff: The climate sensitivity parameter in units K W−1 m2 is given throughout the text as λ. I find this rather disturbing, because in most literature I am aware of λ is used for the climate feedback parameter in units W m−2 K−1, which is the inverse of the climate feedback parameter, e.g. to name a few PALAEOSENS- Project Members (2012); Köhler et al. (2010); Sherwood et al. (2020). I am aware that there is no agreed-upon way to define variables here, but I believe the majority of the readers would be pleased if not λ is taken as climate sensitivity parameter.

Reply: We thank Reviewer 1 for the suggestion. We will change the notation and use α as the climate sensitivity parameter in the revised manuscript.

12. line 152: "60 years simulation time" again needs some more motivation, why not 100 years? If you make averages over the last 20 years of these 60 years, does this imply the TOA energy imbalance is already below 0.1 W/m2 after 40 years, thus having only well equilibrated results to be averaged?

Reply: Slab ocean simulations use prescribed effects from ocean dynamics and reaches equilibrium in less than 50 years; this timescale is determined by the thermal inertia of the ocean mixed layer (Bitz et al., 2011; Danabasoglu & Gent, J. Climate, 22(9), 2494–2499, 2009). In our analysis, we used last 20 years, which has an average TOA energy imbalance less than 0.1 Wm^{-2}. Results do not depend on whether last 20 or 10 years are used for analysis with a difference less than 5%.

13. line 185: ΔT_{fSST} is 0.2deg C here, but I understand that SSTs are fixed and this averages over land only. If so, it should be negative, not positive, please check.

Reply: We will make the correction in the revised manuscript.

14. line 189: Labelling a scenario LGM_2CO2 is ok, but if you used 2CO2 in the text, you need to

revise the writing to “2×CO2” or so.

Reply: Thank you for the suggestion. We will make the correction in the revised manuscript.

15. line 191: should be “–24”deg C.

Reply: This will be corrected.

16. line 194: Expand if sea level fall is also part of the considered processes.

Reply: The “increased coverage of land” in the manuscript refers to the shelf exposures due to the lowered sea level. We will add clarification in the revised manuscript.

17. line 205: Does this (ERFλ from LGM LIS) include snow outside LIS regions and cloud adjustments (mainly due to sea level)?

Reply: Yes, ERF_{λ} (Equations 1 and 3) here contains effects of atmosphere adjustments (including cloud changes). Accounting for these adjustments makes ERF_{λ} more consistent with the framework of forcing and response (Sherwood et al., 2015). Effect of snow outside LIS on ERF_{fsst} is relatively small outside LIS regions in the simulation with prescribed preindustrial SST; this can be partly seen from Figure 2d, which does not show large negative shortwave ERF_{fsst} outside LIS regions. Importantly, this snow effect is largely removed when correcting the land surface temperature changes in “fixed-SST” simulations (Equation 1). This interpretation is supported by the fact that ERF_{λ} nearly equals ERF_{kernel} for both LGM GHG and 2×CO_2 (Table 1). It suggests that ERF_{λ} effectively removes effects from changes in snow albedo, air temperature, and water vapor over land, as the kernel method does.

18. line 215: “–3.3” misses a unit of “W/m2”

Reply: Units will be added.

19. lines 287ff: SO stratification: To my knowledge paleo data suggest a higher stratification in the Southern Ocean at LGM, while here a smaller one is found. See for example the vertical distribution of 13C in Figure 11 of Hodell et al. (2003). Maybe this has to do with difference in shallow and deep water stratification. There are also other papers discussing the role of Southern Ocean diapycnals or mixing and the variability of diapycnal mixing, e.g. see Jones and Abernathey (2019) or Hines et al. (2019) or Holzer et al. (2017) or Abernathey and Ferreira (2015). Please discuss how those processes are implemented in your model here with respect to what these papers suggest.

Reply: Our LGM simulation exhibits decreased *shallow water* stratification over the deep-water formation region underneath sea ice in the Southern Ocean (*southward of ~60°S and above ~500m*; Figure 5a, b). The upper-ocean stratification is the most relevant for the mixed layer heat convergence and interactions with sea ice.

Major features of deep-ocean circulation and seawater characteristics in our CESM1 simulations agree well with findings from proxy reconstructions (e.g Hodell et al., 2003; Curry and Oppo, 2005; Adkins et al., 2002). These features include an expansion of the Antarctic Bottom Water, a shallower North Atlantic Deepwater, an increase in abyssal stratification, and a saltier and colder southern-source deep water. The role of a dynamical coupling between sea ice and the ocean stratification is also consistent with modeling and theoretical studies (e.g., Shin et al., 2003; Ferrari et al., 2014; Abernathey and Ferreira, 2015).

We will add a short discussion on the above aspects, as the focus of this study is not on examining details of the LGM ocean circulation. We will also clarify in the revised manuscript that we are referring to the upper-ocean stratification, which is more relevant to the heat budget within the ocean mixed layer and the expansion of sea ice.

20. line 340: Using mean values and Eq 6 I obtain 3.4 K, not 3.6 K as stated in line 344. Maybe this is based on the effect of Monte-Carlo sampling which considers the uncertainties, maybe it is a typo, please check.

Reply: There is some confusion here. Line 344 reads "In our "perfect model" assumption, all the above values are unbiased, and the "true" ECS is 3.6°C." The value of 3.6 K is the "truth" in the "perfect model" scenario in CESM1.2, obtained through performing 2×CO_2 simulations. We think this is clearly written in the manuscript and, therefore, have left the value as it is.

21. Discussion:

(a) Please calculate out of your Monte-Carlo results and figure 6 and the range typically given by others, e.g of 68% for ±1σ, or 95% ±2σ.

Reply: We will adopt your suggestion and redo the Monte-Carlo calculation and Figure 6 in the revised manuscript.

(b) You find an ECS for LGM based on GHG, and land ice including efficacy of 3.6 K, so comparable to S[GHG,LI] in the nomenclature of PALAEOSENS-Project Members (2012), although this is the value BEFORE multiplying with the radiative forcing caused by "2×CO_2". (Might also be called Sε following Stap et al. (2019) to account for the efficacy.) Please say so and also emphasis here (and in the conclusions) again, that vegetation and aerosols are kept fixed at PI level. Also note (and maybe compare), that S[GHG,LI] in PALAEOSENS-Project Members (2012) for LGM was 0.85 0.19 K W^{-1} m2 which translates into an approximation of ECS of 3.3±0.4K using ERF_2CO2 of 3.9±0.3 W m^{-2} found here, so both studies are within uncertainties pretty much in agreement.

Reply: We stress that the goal of this modeling study is not to estimate ECS but to examine the methodology that have been used to estimate ECS using paleoclimate reconstructions. In the model, we can directly obtain ECS through performing model simulations with the 2×CO_2 forcing.

The apparent agreement of our ECS in calculation (Equation 6) and in CESM1.2 with results (~3.3°C) from PALAEOSENS-Project Members (2012) is caused by cancellation errors. PALAEOSENS-Project Members (2012) used a LIS forcing of –4.5 W m^{-2} and an LGM cooling of –6.1 to –5.1°C, assumed unity efficacies of GHG and LIS forcings, and did not account for the ocean dynamical feedback. In contrast, our CESM1.2 simulation suggests a larger LGM cooling of –6.8°C, a smaller LIS forcing of –3.2 W m^{-2}, non-unity efficacy of both GHG and LIS, and a large contribution of ocean dynamics to the magnitude of LGM temperature change.

(c) line 346: "If we neglect the ocean dynamical feedback and assume that both GHG and LIS forcings have a unit efficacy, as has been done in most previous studies (e.g., Rohling et al., 2012)". This comparison to Rohling et al., 2012 is only correct for efficacy (no efficacy considered in Rohling). It is however incorrect for ocean dynamical feedback, since in Rohling temperature change is taken from data, which always include all processes, also vegetation and aerosols that has been kept fixed here.

Reply: Our simulations suggest that 28% of the LGM cooling is caused by ocean dynamics. This additional LGM cooling is achieved through a dynamical ocean-sea ice coupling, which produces a stronger sea-ice albedo feedback (see Figure 5c for the sea-ice cover in SOM and FCM simulations). In contrast, in response to 2×CO_2, the sea-ice albedo feedback is much weaker than that in the LGM simulation (see Kutzbach et al., 2013). The results, therefore, suggest that the LGM cooling obtained from data is not fully caused by fast feedbacks that are operating in the 2×CO_2 scenario. A correct way would be to subtract the ocean dynamics-induced temperature change before estimating ECS, or to account for the non-constant fast feedbacks that depend on ocean dynamics.

In the revised manuscript, we will clarify that the ocean dynamical effects on temperature is achieved through changing fast feedbacks.

(d) Stap et al. (2019) finds an efficacy of land ice of 0.45 with a large uncertainty. This is opposite and a lot more than the 1.1 found here. This need further discussions and suggestions for the differences.

Reply: We will add a discussion on the difference in LIS efficacy between our CESM and Stap et al. (2019) results, but we point out that a precise explanation is impossible considering the differences in the definition of forcing/efficacy, models, and experimental design. Specifically, Stap et al. (2019) used the compilation of model results of ω (the relative impact of land ice changes on the LGM temperature anomaly) from Shakun (2017); most of these results are from intermediate complexity models. Our CESM results suggest an important role of the cloud feedback in determining the LGM temperature response (Figure 3i and Tables 1 and 2). It is unclear how these cloud processes are resolved in models with intermediate complexity.

We will add the following sentence in the revised manuscript: "Our simulations suggest an LGM LIS efficacy of 1.1, which differs from the 0.45 in Stap et al. (2019). A precise explanation about this difference is challenging, given the large differences in the definition of forcing/efficacy, model complexity, and experimental design."

(e) Furthermore, for 2CO2 your model finds an ECS of 3.9±0.3 K. The difference between this value and S[GHG,LI] @ LGM of 3.6 K indicate only a small state-dependency of ECS. If you include the uncertainties around 3.6 K obtained from the Monte-Carlo approach, they are more or less in agreement. This finding needs to get emphasised somewhere, as it seems to disagree with other findings, e.g. Crucifix (2006), for GCMs, but there are certainly newer papers, see also von der Heydt et al. (2014, 2016); Köhler et al. (2017). State-dependency is also discussed in the most recent review of Sherwood et al. (2020). Discuss you small state-dependency of ECS widely. What might be the reasons? The fixed vegetation and aerosols? The too cold state in the full climate model compared to data?

Reply: Our results suggest a strong state dependence of ECS, which manifests in the ocean dynamical effects and the non-unity efficacy (0.9) of LGM GHG forcing. The ocean dynamical effect depends on background climate and its effect on surface temperature is achieved through changing fast feedbacks (see also our response above). In the revised manuscript, we will clarify this point.

22. Figure 2a,b: I do not understand how temperature over ocean can be different from zero here, since I thought SST and sea ice have been fixed. There are blue shadings (negative values) in Arctic and Southern Ocean. Either explain or correct.

Reply: Thanks for pointing this out. The land-sea mask shown in the figures are from the present-day land-sea distribution, so some of the negative values are caused by the fast that they are ocean grid points at present day but land grid points at the LGM. Also, surface temperature above sea ice is free to change, as we only fix the SST and sea-ice cover in the simulations. We will clarify this in the figure caption in the revised manuscript.

References:

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