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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

Anonymous Referee #1

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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.

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).
2. line 38 and later: The reference "Rohling et al (2012)" should be cited as "PALAEOSENS-Project Members (2012)".
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.
4. line 52: To my knowledge "surface topography" should be included in Friedrich et al. (2016), please check.
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.
6. line 74: The effect of sea level is not menioned here. Clarify if it is included or not.
7. lines 82–84: The list of past climate studies using this model is rather long, please reduce to some essentials or examples.
8. line 99: "a different atmosphere model". Which 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 a 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.

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?

11. line 128ff: The climate sensitivity parameter in units K W^{-1} m^{2} 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 how to define variables here, but I believe the majority of the readers would be pleased if not λ is taken as climate sensitivity parameter.

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/m^{2} after 40 years, thus having only well equilibrated results to be averaged?

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.

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 "$2\times CO_2$" or so.

15. line 191: should be "–24"deg C.

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

17. line 205: Does this (ERF$_{\lambda}$ from LGM LIS) include snow outside LIS regions and cloud adjustments (mainly due to sea level)?

18. line 215: "–3.3" misses a unit of "W/m^{2}"

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.

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.

21. Discussion:

    (a) Please calculate out of your Monte-Carlo results and figure 6 and the range typcially given by others, e.g of 68% for $\pm 1\sigma$, or 95% $\pm 2\sigma$.

    (b) You find an ECS for LGM based on GHG, and land ice including efficacy of 3.6 K, so comparable to $S_{[\mathrm{GHG,LI}]}$ in the nomenclature of PALAEOSENS-Project Members (2012), although this is the value BEFORE multiplying with the radiative forcing caused by "$2\times CO_2$". (Might also be called $S^{\varepsilon}_{[\mathrm{GHG,LI}]}$ 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 $S_{[\mathrm{GHG,LI}]}$ in PALAEOSENS-Project Members (2012) for LGM was 0.85 ± 0.19 K W^{-1} m^{2} 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 uncertainities pretty much in agreement.

(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.

(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.

(e) Furthermore, for 2× CO_2 your model finds an ECS of 3.9 ± 0.3 K. The difference between this value and $S_{[\mathrm{GHG,LI}]}$ @ 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?

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.

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