Dear Dr. Skinner,

Thank you for carefully assessing our manuscript and your constructive suggestions. We agree with you that these are important points that need to be clarified. Please find below our response.

When you prepare your revised manuscript, I would like to encourage you to reconsider your dismissal of one particular review comment, regarding the illustration of the 'LGM' CESM ocean state (reviewer 2). You state that:

"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."

However, it seems entirely relevant to me (and I invite you to respond) whether or not the ocean is at thermal equilibrium in your LGM simulation, and whether or not its state is one in which a significant net heat flux to/from the ocean exists, due to e.g. disequilibrium effects. A global ocean cooling of only 0.15°C is quite far from a realistic equilibrium LGM ocean state, based on observations such as the noble gas isotope measurements of Bereiter et al. (2018) for example. I would therefore suggest that when noting the mean ocean temperature change of 0.15°C in your LGM simulation (which i agree is important) you also provide a short discussion of why this result differs from observations such as those of Bereiter et al. (2018) (i.e. is it due to a disequilibrium ocean state?), and also what arguments you can advance for why your analysis and conclusions regarding ocean dynamical feedbacks etc... are unaffected by the ocean's energy/heat budget not being at equilibrium. In response to my own comments on a related theme you have proposed that the surface ocean is at equilibrium, even if the deep ocean is indeed far from equilibrium. I still think it is important to comment on this issue in the manuscript, since it is not obvious that the surface ocean or atmosphere can really be at thermal equilibrium if the deep ocean is in the process of accumulating heat over thousands of years (with a fixed TOA energy balance). The resulting changes might be slow, but they will accumulate into a different 'true LGM' state. Or, to state things differently, if you are indeed analysing a disequilibrium ocean state, then it might be more accurate to see the simulation as a transient cooling experiment and not as a comparison with the 'LGM', as modelled or reconstructed in other studies. Please therefore add a short note on why you believe it is not relevant for your study that the deep ocean heat budget is not at equilibrium in your simulations (if indeed this is the case).

Reply: We point out that our findings on the effective radiative forcings of LGM GHG and LIS and their efficacy are independent on the equilibrium state of the fully coupled LGM simulation. LGM forcing and efficacy are obtained using fixed-SST and slab ocean simulations with a preindustrial SST/sea ice and ocean dynamical effects.

We agree with you that the trend in the whole-ocean temperature could have a small impact on the magnitude of the ocean dynamical effect. We have now acknowledged this by stating: "Due to limited computing resources, our fully coupled LGM simulation has a cooling trend in the deep ocean (see Section 2.1), which will not impact our results on LGM radiative forcing/efficacy but will likely cause an underestimation of LGM ΔGMST and the contribution of the ocean dynamical

feedback in the model." Please see Lines 123–124 and 444–447 in the revised manuscript with tracked changes.