We thank the Editor for careful reading of our manuscript and the helpful suggestions. We have addressed all the comments in the revised manuscript.

1. The concept of equilibrium climate sensitivity (ECS) is defined very clearly the introduction; however, I wonder if (for the benefit of some readers who may be more exclusively familiar with the palaeoclimate context) it might be useful to disambiguate 'ECS' from the term 'ES', designating 'Earth System Sensitivity', which has been introduced in the literature (e.g. by Schneider et al., Lund et al.) and which aims to include the effects of slower feedbacks in the climate system. This is just a suggestion of course; the definition that is provided for ECS per se is very clear.

Reply: We agree that it is better to compare ECS with ESS, especially in the paleoclimate context. We will add the following sentence in the revised manuscript.

"By tradition and for practical reasons, ECS does not account for slow feedback processes, such as changes in vegetation, cryosphere and ocean circulation, effects of which has been included in the Earth system sensitivity (e.g. Lunt et al., 2010)."

2. Line 46: reference is made to the term 'efficacy' here, and elsewhere in the introductory text; however, it is only clearly defined on line 162 and in equation 4. I would suggest that a brief verbal definition of the term be provided up front (e.g. along the lines of "the ratio of the climate sensitivity parameter (K/wm-2) derived for a given forcing relative to that for a doubling of atmospheric CO2", or "the ratio of the warming effect attributed to a given forcing, relative to that due to a doubling of atmospheric CO2 under pre-industrial conditions".. etc. . .). I realise that it might be hard to be succinct and accurate at the same time, but this is an important concept in the paper and it will be important to make it clear to readers early on.

Reply: Thank you for the suggestion. We will add a verbal definition of efficacy in the revised manuscript.

3. Line 73: I found this sentence hard to decipher, and wondered if the following was an accurate reflection of what was intended: ". . .to provide a complete quantification of the LGM LIS and GHG forcing, and their respective 'efficacies', using a suite of climate simulations."

Reply: Thank you for the suggestion. We will change the revised manuscript accordingly.

4. Line 117: It is not clear what the last sentence means to say; please rephrase to clarify (e.g. do adjustments reflect changes that occur 'as a direct result of a given forcing, without mediation by global average temperature change, i.e. not including the Planck feedback?'). It is hard to see immediately what changes in temperature, clouds etc. . ., would be mediated by 'global average' temperature change specifically, as opposed to local/regional changes, apart from the Planck feedback on global longwave output. For example, I understand that sea-ice and snow cover changes that arise from a cooling caused by a GHG change would be excluded as 'adjustments', but do these really arise from 'global average' temperature changes?

Reply: The concept of effective radiative forcing and adjustments is developed to quantify the forcing such that the forcing excludes radiative effects associated with surface temperature change (forcing being independent of response) and that the top-of-atmosphere energy imbalance is closer linked to global mean surface temperature. The concept fits better with the forcing-feedback framework.

In the revised manuscript, we will add citation to Figure 1 of Sherwood et al. (2015) to clarify the concept.

5. Line 160: Is it possible to clarify this sentence? E.g. ". . .represents the global surface air T change associated with an effective radiative forcing, but that is driven indirectly (by SST change)"? Is my suggestion accurate?

Reply: We will re-write the sentence as: "$\Delta T_{SOM} - \Delta T_{fsst}$ represents the SST-mediated surface air temperature changes that is *driven* by ERF_{fsst}".

6. Line 214: As I will expand upon a little more below, I find the phrasing

‘overestimation/underestimation’ somewhat misleading at times, or at least open to misunderstanding. For example, here, I would suggest that it might be clearer to state something like: “. . .this APRP approach overestimates the shortwave radiative forcing that is attributable exclusively to changes in LIS extent, as it includes the radiative effect of snow increases over ice sheets (or regions with shelf exposure); the albedo of fresh snow is considerably larger ”.

Reply: Thank you for the suggestion. We will make the suggest change.

7. Line 216: Similarly I would suggest a minor clarification such as: “The snow-induced overestimation [of the LIS direct contribution] is larger if the cooling over ice sheets is greater.”

Reply: Thank you for the suggestion. We will make the suggest change.

8. Line 218: I think that the use of plural for simulations might be better, i.e.: “ is greater in coupled simulations. . . atmosphere-only simulations. . .

Reply: Thank you. We will make the suggested change.

9. Line 224: Is the study of M. Crucifix (2006, Does the Last Glacial Maximum constrain climate sensitivity?, Geophys. Res. Lett., 33, L18701) relevant here at all (with respect to the temperature dependence of cloud feedbacks)?

Reply: Thank you. Crucifix (2006) is relevant and will be added.

10. Line 258: “. . .the importance of using. . .”. I would also suggest adding for clarity: “ using efficacy to evaluate the overall effectiveness of their radiative forcing as compared to a doubling of atmospheric CO_2.”

Reply: Thank you for the suggestion. We will make the suggested change.

11. Section 3.3: The point here seems to be that the system is broadly linear (at least by virtue of any regional non-linearities cancelling out globally perhaps?); however, I wondered if it would it be justified in your view to add a caveat that this point applies primarily to an evaluation of short-term impacts (i.e. from fast feedbacks)?

Reply: Yes, you are right. The last sentence of Section 3.3 is a caveat that the linearity only holds for the global mean and is not true for regional forcing and response. We will extend the caveat to suggest that the linearity dose not necessarily hold for long-term feedback processes.

12. Line 273: Surely ocean interior temperatures will not be in equilibrium after 60 years, or if they are in the SOM some caution is warranted in extrapolating to the real global ocean? I simply invite your consideration of whether any clarification is needed here.

Reply: SOM simulations usually reach equilibrium in less than 50 years (Danabasoglu & Gent, J. Climate, 22(9), 2494–2499, 2009). SOM simulations are not meant to approximate the real global ocean but used to explore response of the climate system without the consideration of ocean dynamics. Comparing SOM against the fully coupled simulations enables us to quantify the role of ocean dynamics on surface temperature.

13. Line 289: Would it be more complete to state that the remote impact on the SO reflects the impact on SO stratification of a displacement in tropical atmospheric circulations, etc..?

Reply: We meant the other way around, i.e., SO cooling impacts the subtropics through shifting the ITCZ and trade winds.

We will re-write the sentence as: “This reflects a remote impact of the SO processes on the lower latitudes through changing tropical atmospheric circulations.”

14. Line 333: Again, can I suggest to add the clarification: “We note that, due to the inclusion of snow effects in the forcing quantification, the APRP-based approach overestimates the direct shortwave albedo effects that are attributable only to changing LIS extent”? My point is that it is only an ‘overestimation’ if one wants to strip out the knock-on effects of a changing LIS, to consider only

direct impacts. Otherwise, one could argue, conversely that the ‘real’ impact of changing LIS extent is actually underestimated by an approach that does not consider the knock-on effects.

Reply: We agree and will make changes as you suggested.

15. Line 352: Here again I would suggest to alter slightly the language used, for clarity. E.g. “If we do not remove the ocean dynamical feedbacks. . .”.

Reply: We agree and will make changes as you suggested.

16. Line 357: Similarly, can I suggest for your consideration: “In sum, this exercise highlights the importance of the ocean dynamical feedback, which, if included, may cause an overestimation of the (‘fast feedback’) ECS value using reconstructions of LGM forcings/responses.” To my mind, ‘neglecting’ the ocean dynamical feedback would be the same as not stripping it out of the radiative/temperature effects, which is somewhat confusing.

Reply: Thanks. We will change the sentence to read: “In sum, this exercise highlights the importance of the ocean dynamical feedback, which, if not accounted for, may cause an overestimation of the (‘fast feedback’) ECS value using reconstructions of LGM forcings/responses.”

17. Line 375: In the same vein as the above comments, can I propose for you to consider: “LGM-based ECS calculations that neglect to remove ocean dynamical effects produce an overestimation [of fast feedbacks/sub-centennial impacts] by approximately 25%.” My point is simply that I it may be important to make sure no one misunderstand this statement as suggesting that the ocean dynamical feedback dampens warming, when in fact it amplifies it.

Reply: Thank you. We will change the sentence to read: “LGM-based ECS calculations that fail to account for this ocean dynamical effects produce an overestimation of fast feedbacks by approximately 25%.”

18. Finally, I can’t help but add to Referee 1’s comment number 19, that radiocarbon evidence from the LGM is likely more useful as a constraint on large-scale mixing/air-sea exchange of heat/carbon than is d13C, which notably has a non-conservative component due to biological export production. In any event, both lines of evidence would indeed suggest greater stratification/sea ice coverage, not less.

Reply: Thank you. Please see also our response to Review 1’s comments. In the manuscript, we meant to describe the upper-ocean stratification, which is most relevant to the mixed layer heat budget and the interaction with sea ice.

Major features of deep-ocean circulation and seawater characteristics in our CESM1 simulations agree well with findings from proxy reconstructions, including 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. We will add a short discussion on model-data comparison of the LGM ocean circulation.