

This paper uses an ESM to study the different interactions of the physical and biogeochemical aspects of climate at the LGM and PI. It uses these two cases to make some more general comments on this interaction and how it is treated. I note that I was asked to review only the revised version of the manuscript. I did not read the initial version although I did read enough of the responses to see that the authors have largely rewritten it and that the initial critiques were robust. My comments are based only on the revised version.

My first thought when reading the paper and responses is that the terminology and even mathematics around this field is a hideous mess. The same terms seem to be used to describe sensitivity of fluxes to perturbed forcings or the sensitivity of equilibrium stocks. The same term is also used to describe the total role of the carbon cycle in mediating the relationship between anthropogenic emissions and climate change or the modulation of this role by climate impacts on the carbon cycle. Here we also see sensitivities of transient changes in stocks. The authors can't avoid getting entangled in this mess and, as reviewers, we can't blame them for it. The authors can, though, avoid confusing the issue further and getting caught up in interpretation of quantities that probably depend on details of their simulation.

I also sympathise with the authors' dilemma. They have a relatively clear (perhaps obvious) model result. They properly try to analyse and generalise this result using diagnostics developed for other reasons. Much of the controversy relates to this analysis rather than the result itself. Previous reviewers were obviously concerned about aspects of this analysis and I will add to those concerns. The nature of the critiques does, though, suggest a way out. I recommend the authors focus their analysis on the model results themselves. Where are the differences between LGM and PI sensitivities greatest, what processes contribute to these differences. There is much of this material already in the paper and probably more that could be taken from the simulations. I am not sure that what the authors have to say about β and γ rewards the difficulties it has caused them. I also think there are difficulties with this analysis. For example, quoting the time-dependent changes in sensitivity seems quite risky, since it may well arise from an interaction of the time-scales of the transient forcing convolved with climate and carbon-cycle time-scales. We can't easily tell and it would seem to be a lot of work to disentangle the effects.

I believe there is a useful paper within the material the authors present. I believe this will be a shorter, more focused paper. I hope the authors will persevere and revise the manuscript.