

Interactive comment on “Climate and carbon cycle dynamics in a CESM simulation from 850–2100 CE” by F. Lehner et al.

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General comments Referee: This paper presents a new CESM past to future model simulation and is generally within the remit of ESD. I found this paper difficult to follow because it lacked a clear direction and purpose. The abstract suggests that the originality of this work is the continuity of the simulation from 1000 years before present to 100 years into the future, and using a different solar irradiance reconstruction. However, it is not obvious how the aims of the paper: to detect large-scale forced variability; forcing vs. structural uncertainty; and provide context to future projections (p.355 l.3-7), are novel or can be addressed with this simulation. Providing context, in particular, is a rather vague aim.

Reply: (1) The main novelty is the interactive carbon cycle over the last millennium.

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We rephrased the abstract to stress this. (2) The referee comment on “structural uncertainty” is addressed in our answer to the next comment. (3) Some of the aims in the introduction have been rephrased to hopefully appear less vague.

Referee: The paper goes on to compare this new simulation to a mix of previously published data and model simulations with different components, different forcings applied, and different resolutions. Because of these differences, I found the comments about structural vs. forcing uncertainty rather less credible. The paper ‘fails’ to find any large-scale variability, and it was unclear what the contribution on past context was. So, the claimed originality doesn’t have much in common with the aims, the aims only loosely tie with the results presented, and the conclusion is that it is a null result.

Reply: We agree that there are multiple components that contribute to what we summarize as “structural model uncertainty”. Some of them may rather be referred to as “differences in implementing a given forcing”, some as “differences in resolution”, some as “differences in climate sensitivity”, some as “differences in magnitude of internal variability” or others. The point here is to highlight the implications these differences can have in presence of supposedly identical forcing across models. To provide an in-depth discussion and dissection of the underlying causes of all the model differences is beyond the scope of this paper. We revised parts of the introduction and conclusions in light of this comment.

Referee: I think perhaps that the basic issue with this paper is that it tries to cover much. There are references to millennial timescale, pre-industrial, future, comparison between CCSM4 and CESM, comparison between CESM and MPI-ESM, comparison with other PMIP model simulations, comparison with other CMIP model simulations, comparison with data, orbital forcing, climate sensitivity, carbon cycle feedbacks, and carbon cycle response to volcanic forcing. Consequently each section of results is quite superficial. The paper is quite long and not clear in its overarching aim or aspect of novelty. This would be a more useful paper if the scope were reduced and there was a definite focus on what the scientific contribution of this work is.

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Reply: We agree that there are a lot of different topics addressed in this paper, which reflects the overview character of the paper as well as the contributions from many different co-authors. We think, however, that each section provides a new result, even if one can certainly argue that each of those results could be investigated in more depth in a subsequent study. We revised the abstract and discussion to hopefully reflect this better.

Specific comments: Referee: My specific comments do not address sections 3.2 or 5. Given the large range of scope of the sections in the paper, I do feel well qualified to assess these.

Referee: Is the control simulation properly spun up? A supplementary figure would help in this case. And the soil carbon storage units need checking on P.357 L.21

Reply: As stated in the Experimental Setup section, the model is not in equilibrium and in response to another referee comment we now give more information on model drift in that section. The units have been revised as well.

Referee: The methods and experimental set-up desperately needs a table with a clear table of the features, and forcings of the models that are mainly used in the paper. A simple explanation of why these model simulations were chosen, why the authors consider them comparable despite their noted differences would help readers.

Reply: We implemented a table and a more detailed reasoning for the model choices.

Referee: The first part of the results seems to be a competent description of this model simulation over this time period. There are a few null results, it's more or less in line with the results from other models, it doesn't always agree with the data (but then neither do the other models). I don't know what the objective or hypothesis for this model simulation was, and I'm not totally convinced that the authors know either.

Reply: As stated in the introduction, we are interested in the carbon cycle sensitivity and climate variability in presence of altered forcings as compared to traditional PMIP3

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protocols. The fact that some of the altered forcing do not result in a discernible effect on the simulated climate might be the confirmation of a null hypothesis, yet it is still a valuable result. Further, having the orbital forcing fixed allowed to study its influence when comparing with other simulations.

Referee: Section 6. I'm a bit dubious about the methods used here. "Mimicking to some extent" other methods is rather vague, more clarity would be helpful here. The methods section doesn't say whether dynamic vegetation is turned on in the simulation, but presuming that it is, I'd be surprised if the low pass filter didn't obscure the reaction of the C3 grasses and other quick growing vegetation types to temperature increases.

Reply: We rephrased the whole section to reflect this and comments by other referees. Given the experimental setup, we cannot apply the identical methods as in Frank et al. or Jungclaus et al. We follow them as much as possible and describe in detail what we did. Following from that, it is worth stressing that the intention of the low-pass filter is exactly to filter out interannual variability due to volcanoes, ENSO, and other short-lived temperature variations, since these variations would not allow for a robust estimate of the climate-carbon cycle sensitivity (which we state in that paragraph). In fact, we show that it is difficult enough with the low-pass filtered data to find robust results. We would apply a very different analysis if we were interested in the interannual variability of the carbon cycle and the land vegetation. Due to the prescribed land use changes, there is no dynamic vegetation active in the model. We clarify this in the Methods section now. We encourage the referee to consider section 5, despite not being an expert on volcanic forcing. This section is looking in some detail on the interannual variability (i.e., without filtering) of the carbon cycle in response to volcanoes.

Referee: Similarly, selecting only the northern hemisphere biases the results because of the smaller amount of ocean. The oceans are obviously a huge part of the carbon cycle, particularly over longer time periods and it seems quite possible to me that the global sensitivity could be different to that of the northern hemisphere. If considering the global CO₂, surely you need to consider global temperature, else you could be

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attributing a CO₂ change originating in a S hemisphere ocean circulation change to a N hemisphere temperature change.

Reply: This a good point. With this approach, we were again following Frank et al. and Jungclaus et al., who use global CO₂ and NH temperature. We double-checked the results using global CO₂ and global temperature, which results in median values of 1.7 ppm/K for the transient simulation and 2.3 ppm/K for the control simulation. The values for using global CO₂ and NH temperature were 1.3 and 2.3. This result is not surprising, as including the SH smoothes the temperature time series, especially after volcanoes or solar minima (due to the ocean damping). Therefore, temperature variations are reduced while the CO₂ time series remains the same, resulting in a higher sensitivity estimate. In contrast, for the control simulation there are no volcanoes or solar variations, so that the inclusion of the SH has little effect on the climate-carbon cycle sensitivity estimate from the control simulation. We include the following paragraph in the revised manuscript: "Note, that we use NH SAT in order to be comparable with existing studies. Using global instead of NH SAT can influence the estimate of gamma, especially for the forced simulation: including the vast ocean area of the SH tends to dampen temperature variability induced by volcanoes and TSI variations. With temperature variability dampened, gamma increases to 1.7 ppm/K (1.4-2.1). For the CTRL, on the other hand, which does not see volcanoes or TSI variations, using global SAT has no discernible effect (2.3 ppm/K)."

Referee: p.375 l.1-20 To say that the c cycle sensitivity is "comparably low" is just not the case. The median value is outside of the reconstructed range, so "very low" would be a better way of describing the sensitivity. I find the sentence about Arora et al rather misleading, since the model is "in agreement" with other CMIP5 models, but the positioning of the sentence makes it seem as though it is in agreement with data.

Reply: We changed this to "very low" as suggested by the referee. Further, we changed the sentence discussing other model studies: "This low sensitivity of CESM was found in other model studies as well, e.g., Arora et al. (2013)." Similarly, in the conclusions

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we now state: "Generally, the sensitivity of the carbon cycle to temperature variations in CESM is very low compared to observations. . ."

Referee: The discussion in general doesn't add to the paper as it reiterates the findings. It also gives general advice about how paleoclimate modelling can be better conducted. This advice is not (so far as I can see) novel, and the last paragraph of the paper is particularly galling, since it calls for ensembles with properly separated forcings, which is what the rest of the paleoclimate community usually already do, and what probably should have been done to address the aims of this paper.

Reply: While we agree that an ensemble of simulations and/or a number of single forcing simulations would have been helpful, this was far beyond the resources of our institution. Upon the start of this study there were simulations from 9 different models available within the PMIP3 framework, but only one of them had single forcing simulations and an actual ensemble (i.e., more than one simulation). So, we disagree that this approach is already the "standard" in paleoclimate modeling. For those reasons it is in our view worth stressing the need for such ensembles. We expanded that part of the discussion to highlight the problem of optimizing the usage of computing resources.

Referee: The figures are nicely presented.

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