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Earth System Dynamics Discussions

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

Anonymous Referee #2

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General comments

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. 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.

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.

Specific comments:

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.

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.359 L.21

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

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.

Section 6. I'm a bit dubious about the methods used here. "Mimicking to some extent" other methods is rather vague, a 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. 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 CO_2, surely you need to consider global temperature, else you could be attributing a CO_2 change originating in a S hemisphere ocean circulation change to a N hemisphere temperature change.

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 Arola 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.

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.

The figures are nicely presented.
