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

Anonymous Referee #3

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The manuscript 'Climate and carbon cycle dynamics in a CESM simulation from 850–2100 CE' by Lehner et al. describes the evolution of climate and the carbon cycle from the last millennium to the end of the current century as simulated by CESM model. The authors investigate the response of the climate and the global carbon cycle to the role of orbital forcing and volcanic eruption. They take advantage of this modelling framework to determine climate-carbon cycle sensitivity over several periods. The authors employ a quantitative methodology comparing the response of CESM model to previous simulations of CCSM and MPI-ESM and to available reconstruction and observational data. This manuscript is well written and the analyses are sounds. As such, this manuscript is a good documentation of the climate and carbon cycle evolution during the last millennium as simulated by CESM. Therefore, I recommend its publication after the following minor issues are addressed.

General comments:

(1) The paper is too long and might be shorten if results & discussion are re-arrange.

(2) Several mechanisms rely on the role of the ocean. However, few analyses are provided in terms of ocean physics and ocean marine biogeochemistry.

(3) It is unclear if the ocean component of the CESM model has benefited from a proper spin-up

Specific comments:

P352 L14 what do you mean by "potentially" ?

P352 L16 please cite the adequate references here.

P352 L17-18 in regards of the results/discussion section, few words are needed to indicate that the climate-carbon sensitivity in CESM is lower than that estimated by Frank et al., 2010.

P353 L24 usually the envelope refers to 1xsd (66% confidence interval) while that used in the manuscript is 2xsd (95% CI).

P354 L21 please add (Tjiputra and Otterå, 2011) to the reference list

P355 L9 please remove 'fully'. Your experimental design implies that the carbon cycle is coupled only with biogeochemical components not the climate. Or, maybe add few lines on how biogeochemical responses of the interactive carbon cycle may impact the climate (e.g., evapotranspiration in response to rising xCO2 in CLM4). I seems this set-up might bias the determination of climate-carbon sensitivity. Maybe add few words on this in the discussion.

P357 2.2 experimental set-up I think that description of the ocean biogeochemical initial condition is omitted here. Please provide a description. What are the drift in ocean transport metrics like the AMOC, ACC, AABW flow in CESM ?

P358 L18 you mean that there is no background volcanoes over the future scenario period ? How does this impact the simulated natural variability compared to previsous period (in terms of detrended signal) ?

P360 L3 If I'm right, the experimental design in IPSL model is not similar to yours since impacts of volcanoes is computed offline and added to the variation of the solar constant (see Dufresne et al., (2013; Swingedouw et al., (2013)).

P364 L15 please provide quantitative information here. A table might help.

P365 L28 please cite (Schwinger et al., 2014)

P366 L5 please cite (Wunsch and Heimbach, 2007; 2008). Quantitative information on the Southern Ocean ventilation might help (AABW flows, winter mixed volume etc. . .)

P368 L17 Weaker correlations in the high latitudes domains were expected since you apply a 5-year smoothing filter. You could eventually assess the correlation in high-latitude domains with filter bow larger than 5 years.

P369 L5 please cite Geoffroy et al. (2015) which show how land-sea ratio warming differs between CCSM4 and MPI-ESM.

P369 L17 To my point of view the penetration depth of the signal must refers to heat fluxes not solely to changes in ocean temperature. Please check whether the results are consistent using the ratio between OH [W m-3] and Hflx [W m-2].

P369 L 26 you may also refer to Swingedouw et al., (2015)

P370 section 4.2 Further details are needed here. First the use of DIC anomaly with respect to 850-1849 might be clearly state in the text. Then, It is unclear to me whether the evolution of the distribution of the DIC anomalies in function of time is an artifact of the anomaly calculation or an effective difference of behavior between the two models. If control simulation is available over such period, please assess if the patterns shown on Figure 8 also emerge after correcting the drift in DIC. Since most of the difference

are due to various behavior in Southern Ocean mixed-layer depth, It might be interesting to illustrate these latter with an additional Figure. If models are identical, you could eventually refer to (Resplandy et al., 2015) which provide an quantitative comparison of several CMIP5 model including CESM-BGC and MPI-ESM over the preindustrial control simulation.

P378 L4 please mention that the Time of Emergence framework address solely direct changes not climate-carbon cycle feedbacks.

Figure 4 caption: change 'observational' by 'observation-derived' since GCP data are a combination of several observational source of data plus process-based model reconstruction.

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