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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.

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This manuscript presents an analysis of CESM Last-Millenium simulations, compares them with other models (esp. MPI) and looks in more depth particularly at aspects of forced variability and carbon cycle response/sensitivity.

I found the manuscript interesting and well written with some good points made. With one exception I found very little to comment on beyond minor presentational aspects, and therefore recommend publication after some minor revisions.

The aspect I would like to dwell on though is the presentation of the carbon cycle "sensitivity". There has been much made on the diagnosis and constrain of this quantity from both short term and long term observations and the area is extremely important,

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but often controversial or not treated consistently. Hence some caution is required to make sure that the work shown here is not misinterpreted.

I don't disagree with your analysis, nor your figure 13, but there are two main things I think which need to be brought out much more clearly.

1. The quantity "carbon cycle sensitivity" (both sensitivity to temperature and sensitivity to CO2), is not a fundamental quantity that can be measured in one context and applied in another. You acknowledge this briefly in p.375, line 20, but I think it needs more discussion. The processes behind any response are many and varied and have different magnitudes and timescales - hence the global sum of these varies a lot across timescales. For this very reason we assembled a table of processes and timescales in AR5 carbon cycle chapter (Ch. 6, table 6.10). So the key thing to bring out in the discussion is that this is an interesting metric to measure (in obs and models), but the value cannot be compared across timescales or used to infer future behaviour. As a model diagnostic, experiments should be designed so that model behaviour cn be compared with observations sampled in the same way. I'd recommend Friedlingstein and Prentice paper on this: Current Opinion in Env. Sustainability, vol. 2, issue 4, 2010.

Reply: We agree that this might cause confusion and have now extended the explanation to hopefully clarify the distinction of our metric with respect to other studies as well as potential implications.

2. perhaps more importantly, the quantity itself you present here is subtly different from anything I've seen elsewhere, and is really quite different from "gamma" as used to quantify carbon cycle feedback metrics in Friedlingstein 2006 or Arora 2013.

- firstly, their definition of gamma is indeed a true sensitivity - i.e. atmospheric CO2 is held fixed, climate is allowed to vary, and then you can diagnose the impact this has (as an isolated forcing) on land/ocean carbon stores. This is what C4MIP regard as "gamma".

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- in observations, and fully coupled models (i.e. with interactive atmospheric CO2), changes in carbon divided by changes in temperature ARE NOT gamma. They fold in the feedback responses - the initial sensitivity to climate is modified because the atmospheric CO2 has changed, and hence the climate changes further, and the carbon stores respond to this. So, for example, the quantity presented in Frank et al is not the C4MIP gamma. That's not to say it's not a good quantity to calculate - but please don't call it gamma. For obvious reasons this gets confusing and wrong comparisons are made between it and other studies.

- in your study here, you present something SIMILAR to Frank et al, but the experiment design is such that it's not exactly the same. By having an interactive CO2 which the carbon cycle sees, but a prescribed CO2 which the climate sees, then you have a brand new experimental design. It sits somewhere between the "COUPLED" and "UNCOUPLED" designs of C4MIP. Your carbon cycle can therefore respond to changes in atmospheric CO2 caused by the climate effect on carbon stores. But the climate itself will not further respond. Hence i would expect exactly what you see - a relatively low value of gamma, because any increase in CO2 in the atmosphere will be offset by increased uptake (i.e. the C4MIP "beta" term kicks in).

So in summary for the carbon cycle sensitivity section: i. DONT call it gamma. It's not.

Reply: Ok, done.

ii. DO explain how/why it differs from Friedlingstein gamma, and the Frank quantity

Reply: We included a paragraph contrasting the different sensitivity quantities that exist and how ours fits in.

iii. DO stress more clearly that it can't be extrapolated across timescales due to many different processes

Reply: We do that now in section 6.

iv. also, consider splitting into land/ocean values - these should be readily available

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from model results and would be interesting to see how the magnitude and lags vary

Reply: We calculated and included those values now.

v. can you also explain why you use NH temperature to define it? Again, this creates a difference from Friedlingstein definition. In observational reconstructions maybe NH is better constrained? but in model results at least global T is available. given you don't compare this result to observations, should you therefore use a global T? or at least justify why not.

Reply: Frank et al. and Jungclaus et al. used NH temperature; to be as comparable as possible we did the same. In response to other referee comments we repeated the analysis with global temperature and included the results in the paper. The conclusions remain unchanged.

some more minor comments follow

- methods section - as this model has a nitrogen cycle, can you mention how you treat N-deposition as a forcing? I imagine there is no standard PMIP protocol for this. e.g. Is anthropogenic N-deposition assumed zero until 20th century?

Reply: For pre-1850 anthropogenic deposition is included in the fixed 1850 prescribed nitrogen deposition values. For post-1850 it follows the references given under Experimental Setup (Lamarque et al.). We now clarify this in that section.

- sec 4.2. You say carbon cycle variability hinders the analysis of phasing between models. Could you remove some of this using a simple regression to ENSO (as you do later for your Pinatubo CO2 figure). This may be an easy way to remove some internal variability in CO2 in the model to let the forced responses show through a bit more. (e.g. if you look at fig 2 of Jones and Cox 2001, the volcanic signal is very clear once a Nino3-regression is removed)

Reply: We tried that without success. The reason this works in Jones and Cox or our Fig. 12 is because they both look at global CO2. We, on the other hand, attempted here

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to find spatially coherent changes between models (we essentially produced something like Fig. 5 for land and ocean fluxes and found no significant correlations). Regional fluxes are much more noisy and regressing out ENSO did not help that. One reason could be that ENSO has a different influence on the carbon cycle in the two models. In the section on volcanoes, however, we discuss the differences between the models in terms of globally intergrated or averaged quantities. These results are much more robust (even though they still reveal fundamental differences between the two models considered).

- can you check units on figure 9[c]? it says both PgC and ppm? (I assume PgC by looking at panels d and e)

Reply: This is intended and additionally stressed in the caption, but you are right, we should have indicated that it is “Pg C” and “ppm CO2” in the panel title. Done.

- I did like the analysis of the volcanic response of CO2. I wondered if something similar for the climate sensitivity of Carbon would be possible (not here, but as a later study). Looking at the mechanisms which control the changes in your (soon-to-be-renamed!) gamma, would this throw up a clue on how we can evaluate models better? Why do the different timeperiods have different sensitivities? can the model be used to figure out why? There has been a lot of interest in using short term interannual variability to try to constrain carbon cycle sensitivity (Cox et al 2013). There must be some more constrain from palaeo runs/data too, and the first step would be to find the model processes which lead to these time-changes in gamma. A process-understanding of a palaeo carbon cycle constraint would be very powerful!

Reply: Thanks for this valuable comment! There might indeed be merit in proceeding this line of thought in a dedicated study. In particular making use of a larger number of models as well as new paleo proxies.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 351, 2015.