

## Review of 'Controls on terrestrial carbon feedbacks by productivity vs. turnover in the CMIP5 Earth System Models' by Koven et al. in Biogeosciences Discussions

### **General comments:**

This study analyses Earth System Model outputs in a creative way and pinpoints specific issues with the current generation of Earth System Models concerning the level of aggregation of model outputs and more importantly a pervasive structural issue. Koven et al. provide estimates of uncertainties arising from initial discrepancies and changes in productivity versus turnover in Earth System Model projections of the 'living' and 'dead' components of the carbon cycle using a linearisation approach. In my opinion, the study convincingly illustrates that a process, which they call 'false priming', currently occurs in all analysed models and masks intrinsic changes in turnover times for the 'dead' carbon pool. While I agree with their conclusions generally, I have a few general concerns:

(i) I feel that potentially underlying issues with models need to be highlighted and discussed in more detail. The result that changes in productivity are more important than changes in turnover is in my opinion an artefact of a lack of variability and realism in the representation of allocation and mortality processes in the current generation of models. The authors acknowledge this but I feel it needs further discussion.

(ii) The assumption that woody carbon turnover is a reasonable approximation of 'life' carbon turnover for all ecosystems is questionable, especially given that they state a significant difference between forested and non-forested ecosystems.

(iii) The information about specific details of scenarios and runs seems contradictory at times and could profit from some more detail. Especially, more detail is necessary concerning the CO<sub>2</sub> feedbacks in the terrestrial and ocean biogeochemistry components, because the biogeochemically-coupled scenario is used for the calculation of the 'false priming' coefficient but does surely also include other feedbacks. I also wonder, if scenarios would have been available to determine the impact of land-use change, which I would expect to be crucial given that the terrestrial surface has been largely appropriated by humans.

(iv) I think it is worth mentioning how these models performed in benchmarking tests, i.e. evaluation against historical climate datasets.

(v) The study assumes equilibrium of the carbon pools at the end of the pre-industrial period, but does not treat explicitly the cases which are actually not at equilibrium at this stage. Further discussion is needed in my opinion.

(vi) The manuscript could be made a bit more concise by cutting out some equations (see specific comments about the equations). Finally, the figures and figure captions need tidying

and some typos and grammatical errors need to be corrected. The figure axis are generally very small and it is almost impossible to read the  $r^2$  and regression equations.

**Specific comments:**

P5757 : I think the title should strictly-speaking read 'Controls on terrestrial carbon feedbacks by productivity vs. turnover in five CMIP5 Earth System Models', because the authors analysed only five of the models that participated in CMIP5.

P5759 L26 : This is the most studied effect on plants and the primary effect in models, but I would not rule out that other effects such as changing water-use-efficiency, which are not captured by these models according to Keenan et al. (2013), or changes on mortality regimes could actually be more important. I feel that these issues could be at least mentioned briefly.

P5758 L20 : I agree that this study gives an indication that *false priming* masks the intrinsic changes in turnover times. However, I think it needs further highlighting that the linearisation approach is only an approximation based on the available model outputs. I think there is further need to follow this up due to potential issue with the approach.

P5759 L27 : Although the increase in productivity is widely observed, it is questionable if it will be sustained (i.e. Norby et al., 2010 or Hungate et al., 2003). Furthermore, the transient increase in vegetation carbon could also result from changes in water-use-efficiency, allocation or turnover. Or at least these effects could be partially responsible for the observed increase.

P5760 L6 : It is assumed here that an increase in productivity, will result in an increase in the input to soil carbon. However, they do not need to be proportional. Can the authors clarify, if these fluxes are proportional for all models?

P5760 L11 : I would replace 'warming' with 'temperature'. Although the global, long-term trend is a warming, local and short term changes can be cooling and they also impact productivity (especially chilling and frost damage).

P5761 L21 : This sentence was not clear to me initially. Although it is correct, I suggest the authors rephrase and provide a bit more detail here.

P5762 E 2, 3 and 4 : The exact choice of the turnover calculation needs better justification and in my opinion there is no need for all three equations. I suggest the authors only justify why they use NPP rather than GPP as a flux and why they chose to lump all mortality terms

together and represent them as a turnover time. To make the manuscript more concise only one equation - presumably equation (4) - would be necessary.

P5763 L7 : How long would it take for the carbon stock to reach equilibrium, if the productivity and turnover terms would be held constant?

P5763 E7 : I suggest that the authors also replace the  $f_{l \rightarrow d}$  by  $C_l / \tau_l$  to make the connection between the two pools more obvious.

P5764 L5 : In the methodology it is stated that land-use change was not considered as a forcing in the experiments, but here it is implied that it was not static either in some scenarios/experiments. I wonder how models implement internal variation in land-use change, when this is not prescribed as a forcing and how this internal variation affects the results. Please explain.

P5764 L7 : Is the assumption that all fire related fluxes are generated from living pools reasonable given the models' fire modules? In reality CWD and make up a significant amount of the combustible biomass of an ecosystem.

P5764 L18 : This linearisation over such a long period (72 years) is possibly resulting in significant errors. I suggest the authors attempt to quantify the error as suggested by the other reviewer.

P5766 L9 : Why is the smoothing done over 15 years? Would it change the results at all if the smoothing is done over 8, 9, 10 or 20 years?

P5766 L20 : Again it is mentioned that land-use change and harvest were not considered at all. See my comment on P5764 L5.

P5767 L3 : Indeed, I would expect lower decomposition rates at higher latitudes, but I would also expect lower productivity in higher latitudes. So is the change in turnover times only a result of the temperature-dependency of decomposition or also related to climatic effects on productivity. Please explain why the changes in productivity are not mentioned here.

P5767 L19 : The values in parenthesis are the range of  $r^2$  values, right? It is not perfectly clear to me.

P5768 L26 : Do the authors have an idea why HadGEM2-ES shows the opposing trends in

turnover times?

P5769 L12 : How much is the difference, when including leaves and roots? Using only the woody pool to estimate the carbon turnover, is likely to over-estimate the vegetation carbon turnover time, as foliage and fine roots have both generally shorter turnover times. While I think that the assumption is defensible for forests, I find it highly questionable if this assumption is reasonable for non-forested ecosystems such as grasslands and savannahs, which are also investigated here. In fact, the authors even show that there is a significant difference in the turnover time of forested and non-forested ecosystems.

P5769 L15 : I assume that mortality of individual trees is either not at all incorporated due to the lack of individual representation or done very simplistically. This major caveat ought to be mentioned.

P5771 L18 : These statements are concerning global averages over the 72 year period, right?

P5777 L17 : It is as important to evaluate models against data and especially for turnover-related processes our understanding is limited and suitable dataset are rare. I feel this ought to be reflected upon here.

**Minor comments:**

P5767 L7 : I do not think that 'likely biased low' is what the authors actually mean here. I suggest they rephrase to 'likely under-estimations'.

P5771 L26 : There is one 'seen' too much.

P5777 L10 : Missing a 'to', as in '...,it would be useful to be able...'

P5777 L10 : It should read 'described' instead of 'describes'.

P5787 : It should read '...consider dynamic vegetation distributions...' and not '...consider dynamics vegetation distribution...'

P5788 : The word 'agreement' is missing an 'e'.