

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

General comments:

The manuscript by Koven et al. addresses the relative contribution of productivity vs. turnover change to the changes in carbon stocks of live (vegetation) and dead (litter and soil) carbon pools using linearization in 5 CMIP5 models, via 3 forcing experiments – fully coupled, radiatively coupled and biogeochemically coupled runs. Overall this is a very interesting study that provides insights on the performance of current Earth system models and suggests avenues for future model improvements. The decomposition/ approximate approach used to analyze complicated ESMs is the right direction (in my opinion) to evaluate model performance. I also fully agree with the ‘false priming’ phenomena revealed in current way of calculating turnover time. The paper will be of interest to the wide modeling community of Biogeosciences. A few moderate concerns are: 1) I do have some reservations on the linearization approach given the un-ignorable numerical error associated with this process (see specific comments), yet given the lack of alternatives in such analysis, I would suggest the authors to carefully rephrase their statements and add discussion on the limitations of this approach. The bottom line is, the relative contribution of productivity-driven vs. turnover-driven carbon change may still hold, but their absolute amount is less robust due to the errors from linearization; 2) introducing the concept of ‘false priming’ is a novelty, yet the way of teasing out false priming imposes some assumptions that will yield in biases in the estimated carbon change attributable to ‘false priming’. I suggest at least a mention of this in the discussion, or even better, reanalyze using the suggested alternative approach (see specific comments). 3) I totally agree with using the last year values of pi-control run as the equilibrium values for linearization, however, some models are not at equilibrium even at the end of the pi-control run. For example, the soil carbon of IPSL (cSoilSlow) is not at equilibrium, this will yield in an underestimation of initial turnover time and consequent biases in other calculations. While there is nothing can be done with this sort of cases, it might be worth mentioning in the text; 4) the organization of this manuscript might be further tightened (e.g. some figures should be reordered)

Specific comments:

P5758, L12: “This reponses arises from”, change to ‘These responses arise’

P5762 L13-15: why not lump R_a into turnover term? This will make the turnover bigger, although the relative change wont be influenced. It seems to be a matter of definition of turnover in live pools. Please justify.

P5763 L4. IPSL is not at steady state even at the end of the pi-control run. Maybe briefly mentioning how this will have an effect on the results?

P5764, equ 9, 10. The C should C-hat?

P5764. L18-19: the linearization over a period of 72 years is long enough to yield in big errors. It can be proved that the upper bound of error (deviation, ε) in linearization in this case is $\varepsilon \leq \frac{1}{2}(\Delta f + \Delta \tau)^2$, f and τ both had significant changes over the simulation period, so the deviation is large. This is reflected in the correlation between calculated $\Delta \hat{C}$ and the realized ΔC . Some discussion of the effect of this error on the conclusions of the analysis might be warranted.

P5769, L19-28: what about the allocation schemes in other models other than HadGEM? i.e., what might be attributable for the lack of changes in live turnover time under enhanced productivity in the rest 4 ESMs? Some explanation might be helpful to reveal the mechanistic difference between ESMs.

P5771, L9: Fig 2 should be Fig 8? Please check other figure citations to make sure the correct figure is cited.

P5771, L23: It might increase the readability if the ordering of figure for the dead carbon sections follows that for the live carbon. Here, Fig 7 and 8 may switch order so that the regression comes first to show validity of the linearization approach, and then show the spatial pattern of productivity- vs. turnover- driven carbon gains.

P5771, L26: delete one "seen"

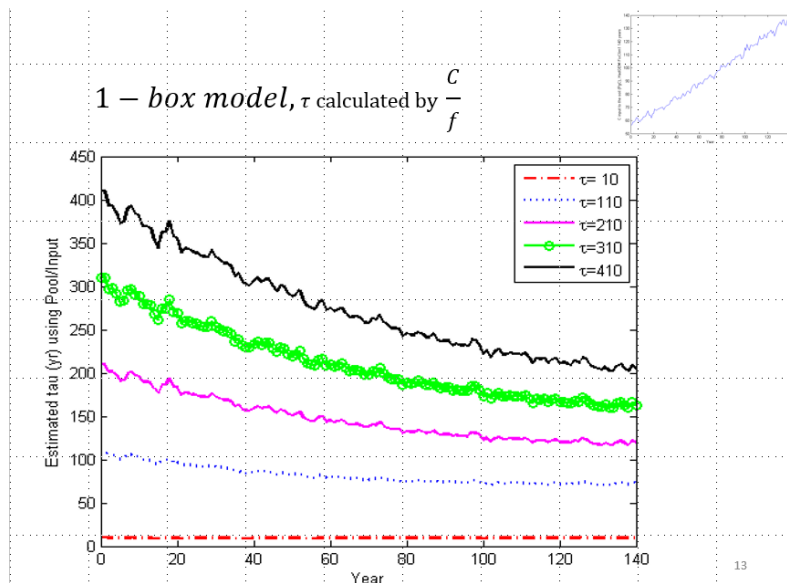
P5772 L3-6: Similarly, Fig 10, 11, and 9 might be re-ordered according the sequence they appear in the text.

P5773. It might be easier for the reader to understand the mechanism of 'false priming' phenomena if it is described as a simple math problem: when dC/dt is positive (C pool is accumulating as under CO2 fertilization), the calculation of using pool/flux will unavoidably yield in underestimation of turnover time. Similarly, if a C pool is depleting, it will yield in overestimation.

$$\frac{dC}{dt} = f - \frac{C}{\tau} \rightarrow \tau = \frac{C}{f - \frac{dC}{dt}} \quad \text{vs} \quad \tau = \frac{C}{f}$$

underestimate turnover (faster)

P5774. The 3-pool box model might be a bit distractive, might just use a simple one-box model to illustrate this. The 'false priming' exists even for a simple one-box model. Below is a simple one-box model, with increasing npp (upper right corner, HadGEM global total soil C input under BGC-coupled 140 years run), and different constant true turnover time (in the legend). The calculated turnover time (using C/f) is underestimated, and the underestimation is greater under bigger true turnover time.



P5775-5776: From the figure above it is clear that the degree of 'false priming' is not linear with the change in productivity, rather it shows a clear pattern with respect to time, as C pool is gradually catching up the increasing input and approaching equilibrium, the degree of false priming tend to be stable (asymptotic) over time. While I really like and vote for the idea of using false-priming coefficient to teasing out such effect from 1pctCO2 and radiatively-coupled runs, it might be important to consider also the time effect. Imposing linear assumption (that the degree of false priming is linear with productivity) will yield in overestimation in the 'false priming' effect, and consequently underestimation of the true turnover-driven C change, as shown in figure 13. The actual turnover change effect should be bigger than what is presented here. At the minimum, a discussion of the limitations and potential bias of this approach will be appreciated.

P5777, L3: delete 'the'

Minor comments:

L5778 L1: Since the global total is reported here, why is a remapping needed? Please clarify.