Response to reviewer 1 (Yujie He)

We thank Dr. He for her careful review and positive assessments. We address below the specific points raised by Dr. He, but want to first address the most fundamental criticism identified in the review. Dr. He argues that the "false priming" response that we describe in this manuscript will always arise as a disequilibrium flux in any carbon cycle that is perturbed from equilibrium by increasing productivity, while we argue that it arises only in systems with a broad range of turnover times such that carbon fluxes equilibrate more rapidly than stocks to an increase in inputs. We argue below that this distinction rests on the definition of turnover time and the way in which it is calculated.

The behavior described and shown in the figure by Dr. He requires that turnover time τ be calculated as the ratio of carbon stocks in a system to carbon inputs to the system; in this paper we consistently define τ as the ratio of carbon stocks in a system to outputs from the system. The reason for this is precisely to avoid the issue brought up by Dr. He: that if one used an inputs definition all systems would show a reduction in turnover times when perturbed from equilibrium. Whereas a reduction in turnover times in response to increased inputs is conditional on, and informative of, the distribution of turnover times when using an outputs definition. This one issue comes up several times below.

Point by point responses

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.

We thank Dr. He for the positive assessment.

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;

We agree that there are certainly errors introduced by the linearization approach shown here. The full set of errors (those introduced by both the linearization and the equilibrium assumptions) can be assessed by comparing the linearized approximation against the full ESM behavior (figures 2 and 8), and we will add further text emphasizing these limitations.

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).

As discussed below, we have some issues with the calculation of Dr. He; however we will add further text to clarify our approach in response to the points raised here.

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;

This is an interesting point; we have not assessed the degree of equilibration in the models, assuming here that the models are sufficiently equilibrated as per the CMIP5 protocol. However we can add further qualifications to the text on this point.

4) the organization of this manuscript might be further tightened (e.g. some figures should be reordered)

We appreciate the suggestion on reordering figures and will likely do so to make sure they are presented in the same order for both the live and dead carbon pool sections.

Specific comments:

P5758, L12: "This reponses arises from", change to 'These responses arise'

Text changed to "This response arises from"

P5762 L13-15: why not lump Ra 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.

This is a good point and brought up by the second reviewer as well. The reason of definition for the input term is that a large fraction of the carbon passes through plants on a fast timescale, on the order of less than a year, with an output flux of autotrophic respiration. By using net productivity as the input metric rather than gross productivity, we allow a separation of timescales with fast processes (gross productivity and autotrophic respiration) considered together as the input term, and slow processes (plant growth and allocation, mortality, heterotrophic respiration) considered together as the turnover term. We will add further text to the manuscript discussing this point.

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?

This is an interesting assertion by the reviewer, which we have not explored. We will note that any deviations from the initial steady state specified by the C4MIP protocol will affect our results.

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

No. Equations 9 and 10 are the definitions of τ for the CMIP5 models. τ is diagnosed from the actual C stocks, and is used to project the linearized equilibrium carbon stocks. Thus T is calculated as a function on C, not 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 $\varepsilon \leq \frac{1}{4}(\Delta f + \Delta\tau)^2$, 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.

Agreed, there is potentially a significant error in the linearization. We will add further discussion of this point in the revised manuscript.

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.

We focus on the allocation scheme of HadGEM because it does show changes to the live C turnover times. Since most of the models do not show large changes in the live C turnover times, we do not focus on the other model's allocation schemes. A detailed examination of allocation in the CMIP5 models is explored in a related paper, Negron-Juarez et al. [2015], which we will cite here so that the interested reader can explore further.

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

Corrected in the revised.

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.

Agreed.

P5771, L26: delete one "seen"

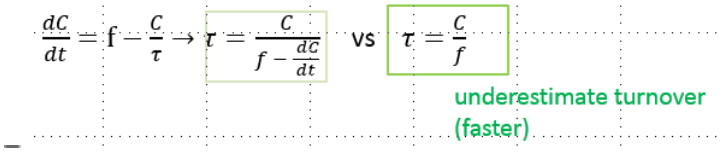
Corrected

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

These are now in the order in which 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.



To be clear here; we do not define τ as c/f, where f is the input flux as in the equation above. This is precisely for the reason shown above: that τ = c/f only when dC/dt = 0. We define τ based on the ouput flux; i.e. equations 9 and 10. Such a definition of τ is valid whether or not dC/dt = 0 for the case where the kinetics are first-order with a single exponential timescale; i.e. a single-pool box model. Thus what we are exploring here is the domain in which such an approximation is valid, and how such a simplification can inform the more complex model behavior.

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.

See the response to the comment above and to the next comment below, which also apply to this comment.

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.

There is a crucial point of clarification that I think is required to understand the argument we are making here and how it relates to the referee's comment. See the below figure, which uses the toy model approach from the paper but given two different model structures (multi-pool and single pool) and two different ways of defining τ (as the ratio of carbon stocks to outputs and as the ratio of carbon stocks to inputs). Note that, as discussed above, we consistently define turnover in the paper relative to outputs; whereas in the figure in referee's comment, and as indicated in the earlier comment, τ is being defined based on inputs in these comments.

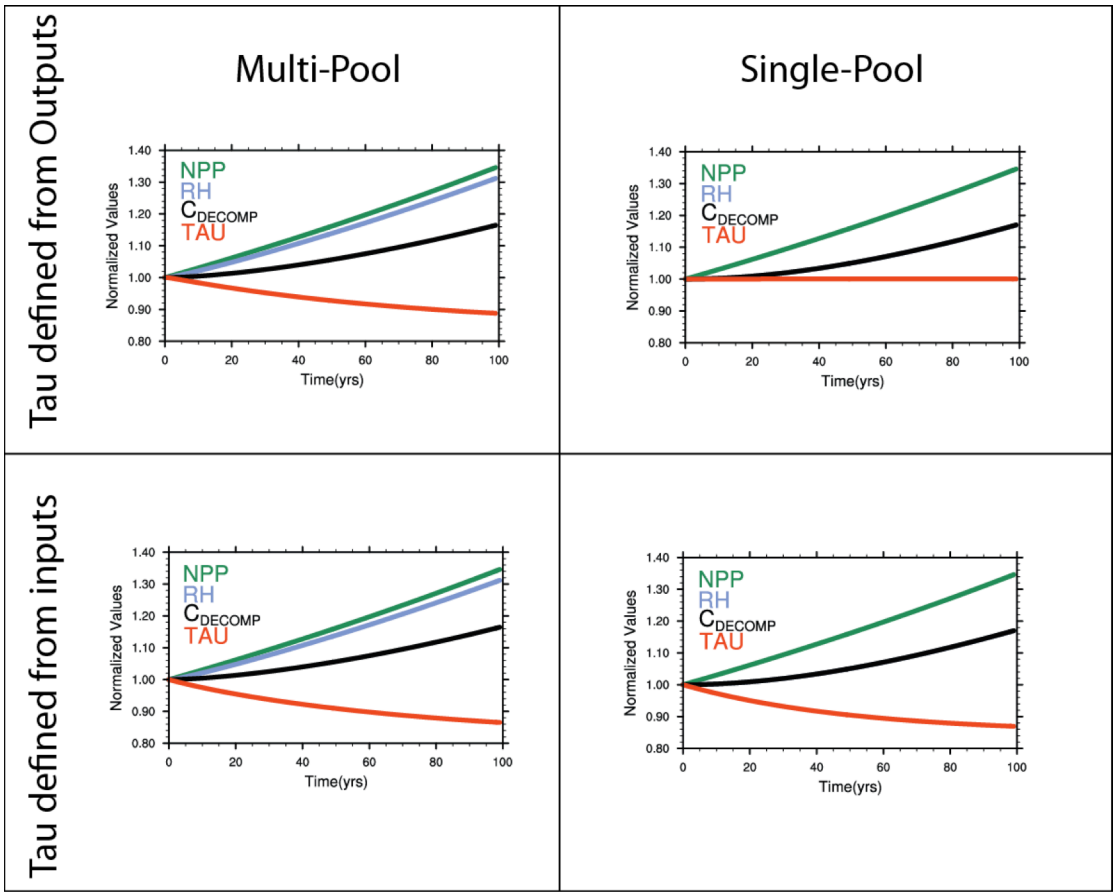


Fig. 1: Simplified model results on turnover time response to changing inputs given an increase in inputs, as a function of both the pool structure (single-pool, right column, versus multi-pool, left column) and the definition of τ (based on outputs, top row, versus inputs, bottom row). Here the multi-pool model is the same used in the original manuscript, while the single-pool model used a single turnover time (here, 60 years). The definition of τ based on outputs, as in the manuscript, allows the identification of qualitatively different behavior from a single-pool versus a multi-pool structure because τ decreases by this definition only in a multi-pool structure. The definition of τ from inputs, as in the referee's comments, shows a reduction in both cases.

As the referee notes, if τ were to be defined based on inputs, then it would decrease under any model structure in response to increased inputs; this would follow simply from the disequilibrium. However, we specifically define turnover based on outputs to avoid this ambiguity. As can be seen here, given this definition, turnover does not change in response to elevated inputs in the single-pool structure, it only changes under the multi-pool structure. It is thus informative of the presence of multiple turnover times, both in models and in observations of a system. This is the essence of the "false priming" behavior that we describe here in the models, and it is precisely this reason that we use the simple toy model approach to articulate the nature of the issue in the absence of any other potential complex behaviors in the ESMs.

P5777, L3: delete ‘the’

Corrected.

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

Will clarify in the text. The answer is that the remapping is needed since we are using ensemble-mean values at a given location for this calculation, and reporting the global integrals.