Review on

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“Understanding the uncertainty in global forest carbon turnover” submitted to Biogeosciences, Feb. 2020

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This paper analyzes turnover of forest carbon simulated by six TBMs (terrestrial biophere models) using a common simulation protocol “to assess their current capability to accurately estimate biomass carbon turnover times in forests and how these times are anticipated to change in the future.” (abstract). The concentration on turnover is motivated by referring to studies that according to the authors have shown that turnover time has “comparable or even larger importance than NPP when assessing the response of C_{veg} to environmental change” (lines 69/70). The analysis proceeds by first setting up a list of potential mechanisms that might modify turnover time upon environmntal change (table 5). Then, splitting the turnover fluxes obtained in the different simulations into components arising from different mechanisms (phenology, mortality), the analysis of the simulation data is condensed into a set of hypotheses on the controls for the behaviour of turnover time seen in the different models. The main result of this analysis is that in the different models the dominant mechanisms controlling turnover are different so that more research is needed. To direct such research the authors discuss for their hypotheses on the diverse controls how the scientific community could proceed by making use of existing data and what the challenges are in achieving progress.

Overall, I really don’t know what to make out of this paper. The result from the analysis of the different turnover fluxes that in the different models the overall turnover is caused differently is not much surprising since this diversity is “strongly linked to model assumptions concerning plant functional type distributions and their controls” (lines 47/48), which are different across models. Making at all something out of this mess by coming up with a list of hypotheses on the different controls in the various models (summarized in the paper title somewhat overstated as “understanding the uncertainty”), is a rather heroic act, demonstrating the author’s solid knowledge on ecological processes and their implementation into models. But then selling this mess by coming up with recommendations for the community on how to proceed makes the whole paper resembling an (extended) introduction for an application for funding rather than being a text informing the interested comunity on advances in understanding. I do not deny that this type of reasearch must be done to advance – and I appreciate the author’s sophistication in their analysis – but it seems to me research that needs to be done *before* performing the true research because it is “only” formulating questions and setting up the research program. Accordingly, I am not convinced that such type of research that stops where it starts to get interesting is of sufficient interest for a larger community to be published in a reviewed journal instead of simply staying a paper in the grey literature.

Otherwise the paper is well readable, although the structure of the paper, outlined in the first paragraph above, could be emphasized stronger within the main text of the paper to keep the reader aware of the purpose of the different sections within the overall construction of the paper so that the 'red line' is better visible; e.g. only later I realized that the aim of section 3 (Results) is to identify the hypotheses listed in table 5 that subsequently, in the discussion part (section 4), are further discussed.

One other general issue concerns the specification of author contributions (Lines 605-7): Looking through the list, 3-4 authors remain that only commented on the manuscript without indication of any significant contribution to it. According to good scientific practice, this is not sufficient to justify authorship (otherwise I as reviewer should also be listed as author): *The European Code of Conduct for Research Integrity* [1] specifies "... authorship ... is based on a significant contribution to the design of the research, relevant data collection, or the analysis or interpretation of the results.". Hence for this paper authorship should be reconsidered. (And btw: who is "KZ"?)

In addition to this, I list below a number of remarks concerning the scientific contents of the paper and their presentation.

Major remarks

(1) Underlying this study is the general picture that vegetation carbon is growing due to NPP and diminishing due to "turnover" fluxes called collectively F_{turn}. This is embodied in the first part of eq. 1, the rate equation $dC/dt = NPP - F_{turn}$. During the analysis presented in the paper, F_{turn} is further subdivided into fluxes arising from different processes. Hence underlying this analysis is also a formula like $F_{turn} = F_1 + F_2 + F_3 + \ldots$. Moreover, for each such partial turnover flux a separate characteristic time is defined by a formula like $\tau_i := C_{veg}/F_i$. All this structure is not made explicit and transpires only while reading. I think for a better readability it would be helpful to make this structure explicit right at the beginning, maybe directly in connection with eq. 1. This would also be the right place to explain in detail the processes underlying all the partial turnover fluxes distinguished in this study, because these explanations are scattered across the paper (what is eg. "Background" in Fig. 6?).

(2) Furthermore, for the analysis framework it is important to note that all partial timescales are defined with reference to the same carbon mass C_{veg}. By this common reference the flux decomposition can also be written as $F_{turn} = C_{veg}(\tau_1^{-1} + \tau_2^{-1} + \tau_2^{-1} + \ldots)$ so that the rates $1/\tau_i$ indicate the size of the relative contributions of the different turnover processes to the total turnover flux. Since it is hardly believable that for all the different turnover processes the fluxes are proportional to the total biomass C_{veg}, the meaning of those time scales τ_i is only diagnostic and may have nothing to do with the actual time scales underlying the various processes causing turnover. An example is fires that depend on the amount of fuel (litter or crown biomass) but not on stem biomass that contains the majority of vegetation biomass. This poses a serious question mark on the overall approach taken in this study by putting time scales into the center of the analysis. – Nevertheless, ratios of the diagnostic time scales stay well interpretable as ratios between turnover fluxes because in this framework $\tau_i^{-1}/\tau_{turn}^{-1} = F_i/F_{turn}$.

(3) For the analysis done in this paper the authors hop between comparing fluxes F_i (e.g. Figs. 6 and 7d) characteristic times τ_i (Table 4, Fig. 7), rates τ_i^{-1} (Figs. S11-S15), and the additional time scale $\tau_{NPP} := C_{veg}/NPP$ (Fig. 1). It would be good to have a discussion on the advantages of using the one or other quantity. Anyway it should be made transparent why F_i, τ_i, τ_i^{-1}, or τ_{NPP} are chosen in the different parts of the analysis and why not the respective others.

(4) The descriptions in the main text heavily draw on plots in the supplement and there seems to be no systematics in putting plots here or there. E.g. for all the many claims on p. 11 the reader is referred to plots in the supplement which is in my opinion a good reason to move those plots – maybe in aggregated form – into the main text. And if the authors stick to their decision to have Fig. S9 in the supplement, then also the description on how to arrive at that figure (lines 221-229) should be shifted to the supplement. In conclusion, it seems to me that the readability of the paper could be improved by reconsidering the division of plots between main text and supplement.

(5) As already mentioned above, the study is motivated by the claim that τ has "comparable or even larger importance than NPP when assessing the response of C_{veg} to environmental change" (Lines 69-73). Because this is the major justification of this study, the authors should be more precise here. E.g. Ahlström et al. (2015a) demonstrate that for stationary conditions turnover has only about 2/3 the importance of NPP (l.c. Fig. 3c), and should (to my understanding) be even less important for the non-stationary conditions of environmental change one is actually interested in. Even better would be to add further plots to this study demonstrating explicitly the author's claim by separating contributions to the biomass changes from NPP and turnover; an analysis in the spirit done by Friend et al. (2014) applied to the transient simulations in the paper would do it. To add: Simple reference to that Friend et al. paper will not be sufficient because their main message is so cryptically formulated that it is inconceivable (they write: "The variance in final C$_{veg}$ caused by differences in fitted residence time relationships between models was found to be 30% higher than that caused by differences in the fitted NPP responses when all models were considered." (p. 3283). What 'variance' is meant here, and why only "when all models are considered"?)

Minor Remarks

- Line 64: Here you introduce as key term the word 'residence time', nowhere else used in the paper, and the term 'turnover time' used throughout the paper, is only introduced in brackets. It's better to drop 'residence time' because its meaning is anyway ambiguous (see Sierra et al. 2017).
- Lines 75/76: For stationary states I agree that there are only these two causes, but for nonstationary conditions another one appears: Once NPP is changing, the internal memory causes a mismatch between NPP and F_{turn} so that the turnover time τ_{turn}, which is only a diagnostic obtained from C_{veg}/F_{turn}, changes in addition to the causes mentioned because the input NPP changes. I think the authors are aware of this (see lines 141/42 where the difference between τ_{NPP} and τ_{turn} is addressed), but since one is ultimately interested in the nonstationary case of environmental change this should not be overlooked.
- Throughout the paper the term "vitality" is used 28 times to refer to a certain group of processes. In lines 95/96 two of them are named, but there is no explanation on the general notion of this term. But such a general notion is needed to understand e.g. the claims in lines 97, 119, 295-298 etc. To me a formulation like "vitality-related mortality" (lines 320/21) sounds a bit weird: Increased mortality improves vitality? Without further help I have problems to make sense out of this vitality concept.
- I find it hard to memorize the meaning of your notation 'MI', 'MP', 'MS'. Maybe you indicate why you choose these combimations of letters or switch to a more mnemonic notation.
- Table 1: It took me really a hard time to understand how columns 2-4 should be read, the caption is in this respect rather unclear. Now I got that these are a pictorial representation of the assumptions on the topics in the headline. To prevent that other readers also struggle with this, I suggest to have another headline covering columns 2-4 eg. with title "assumptions on" and columns 5 and 6 with title "consequences for". Nevertheless, I still do not understand how to read column 2 "Resource capture (NPP) and allocation to woody or soft tissue": Has the combined length of the bars for "Soft" and "Wood" any meaning in relation to some maximum length, e.g. the largest combined bar or the width of the column? Or do you want to emphasize the relative lenghts of the bars "Soft" and "Wood"? Does a "Wood" bar shorter than the associated "Soft" bar mean that the influence on the former is smaller?
- Table 1: In some cases the consequences for biomass and τ shown in columns 5 and 6 seem to me rather speculative and do not follow directly from the information given in columns 1-4. Therfore this table should be revised or at least discussed in detail somewhere. My concerns on the proposed mechanisms are:
  $\mathbf{MI_{NPP,F}}$: Upon rising NPP it is claimed that τ remains unchanged. This is only true for very slow changes in NPP so that the system essentially stays in equilibrium. For a faster increase of NPP the turnover flux stays behind NPP because of the internal memory so that $\tau_{turn} := C_{veg}/F_{turn}$ changes even though only the input to the system changes. (See also comment on lines 75/76 above.)

$\mathbf{MI_{RA}}$: A typical experience when experimenting with water stress in TBMs is that by increasing water stress, in dry regions annual production may increase. The reason is that the resulting reduced NPP flux leads to water savings so that the growth period is lengthened, to the consequence that the annual production (accumulated NPP) rises. For your table entry this would mean that you can as well reverse the direction of your biomass arrow. What the consequences for τ are would be hard to say.
$\mathbf{MI_{ST}}$: The same remark on the counter-intuitive reaction on increased water stress applies also here so that biomass may increase. On the other hand: By increasing exudates it would also be plausible that biomass would decrease.
$\mathbf{MP_{MR}}$: For the example driver "Reduced defensive investment" the consequences could be indeed as you describe if the investment was optimal or sub-optimal before. But in case of super-optimal investment in defense, biomass could as well rise so that also the converse behaviour of biomass and turnover rate is conceivable. And, just to note, it doesn't matter whether the reduced investment in defense arises from a shift in functional composition or not.
$\mathbf{MP_{NPP}}$: I do not understand what is meant by "intrisic NPP" and in what respect the example driver is assumed to be "conservative".
$\mathbf{MP_{RA}}$: I do not see why a shift to species with reduced wood density may necessarily decrease the characteristic turnover time: This is – as far as I see – only true when those species with reduced wood density have themselves lower than average turnover time. If not, turnover time may as well increase.
$\mathbf{MP_{ST}}$: In the table you claim that upon an increased phenological turnover rate due to a shift in functional composition biomass stays unchanged. This seems to me rather unintuitive: This assumes that all species have the same productivity per leaf or root area ("effectivity"), which is typically not the case. Therefore, if the composition is shifted to more effective species the overall LAI or root area may decrease but the total productivity and therefore biomass may increase. And also the other direction is conceivable.

- Lines 186-189: It is unclear why this logic for identifying the forest mask was chosen. What goes wrong when one of the conditions (e.g. the condition on boreal PFT) would be omitted?
- Line 235: Wilkox**a**n → Wilkox**o**n.
- All figures: Put units at color scale.
- Figure 2: According to the caption the figure shows "density kernels". In the caption you explain "density", but the term "density kernel" is nowhere explained. I guess that you show the global relative abundance of τ_{NPP} values. Please consider a renaming or explain the term "density kernel" somewhere.
- Line 260: Here you claim that your findings are summarized in the alternative hypotheses H1a and H1b listed in table 5. But in the table entry you link the allocation fraction of NPP to soft tissues or wood to τ_{mort}, but in the text leading to your claim you argue with the turnover fluxes and neither NPP nor τ_{mort} are mentioned, so that the link between text and hypotheses is missing. Moreover, I do not understand why there should be such a close link between the allocation fraction and τ_{mort} as you

claim in either of the two variants of the hypotheses H1 in the table text, it is easily imaginable that allocation fraction and τ_{mort} are completely unlinked.

- Lines 260/61, 314/15 and Fig. 4: At those lines you make clonclusions from Fig. 4 on the relative contributions of F_{phen} and F_{mort} to the variability of F_{turn}. I doubt that such conclusions can indeed be drawn from that figue. Such conclusions could be drawn if the fluxes F_{phen} and F_{mort} would be uncorrelated, but because $F_{phen} + F_{mort} = F_{turn}$ a large phenological flux leaves not much room for variation in the mortality flux and conversely. Therefore those two fluxes should be strongly (inversely) correlated and the condition of vanishing correlation to draw your conclusions is not met. Accordingly, I think this analysis should be discarded.
- Line 265: Where did you already follow "the same logic"?
- Line 295: What is the reason to start the sentence with "However"?
- Fig. 6: What is "Background"? In the caption: "vertical axis" $\rightarrow$ "vertical side bar" (e.g.).
- Fig. 7: I am not sure what you really show in subfigure d: In the caption you write that you show the "Fraction of total turnover due to mortality" but all curves start at zero so that some change is shown. Maybe you show the relative change in $F_{mort}/F_{turn} = \tau_{turn}/\tau_{mort}$? If so it would be more clear to write down this formula. I also don't know how to understand the in-figure text "Δ Fraction as mort." – why "as"?
- When distinguishing between PFTs it remains unclear whether characteristic times (Figs. 2, 5) and rates (Figs. S11-S15) are calculated with reference to total C_{veg} obtained for the whole mix of PFTs in a grid cell, or with reference to the C_{veg} of the individual PFTs in a grid cell. This difference has e.g. a large impact on whether changes displayed in Fig. 8 can be interpreted on the basis of Figs. S11-S15, as done on p. 11.
- Lines 339-41 and hypothesis H5/H6 in table 5: To me the description in lines 339-41 is inconsistent with the resulting formulation of hypotheses H5 and H6. In line 340 it is said that LPJmL's "increased mortality of established trees" is the reason for the shift in PFT composition – hence LPJmL cannot fall under H5, where "changes in turnover rate of individual PFTs" is explicitly excluded as cause, and the latter exclusion covers the former. Since LPJ-GUESS is listed to fall under H5 *and* H6 (which I do not understand) I guess that there is some insufficient distinctness in the formulations of the text and the hypotheses that make it impossible to follow what is meant here.
- Lines 347/48: Here it is claimed that JULES "implicitly" falls under hypothesis H5. But why is it then not listed in table 5 under H5?
- Lines 383-386: I guess "(1)" refers to MI and MS in table 1, while "(2)" refers to MP in that table. Its a bit irritating that you claim to have "identified" (line 383) these *two* groups "(1)" and "(2)" – in table 1 it were *three* groups and they were not "identified" but postulated. How did you "identify" these two goups?
- Lines 389-391: I have a problem to understand this sentence: What is meant by "These differences"? Differences because MI_{ST} and MP_{NPP} don't show up? "These" cannot refer to differences in the process diversity implemented into the models, because such

differences were not addressed before. Hence you mean differences between implemented processes and “emergent response”? This makes no sense. I am lost. And what means “have been under-constrained”? So they are constrained now?

- Lines 403-405: Wrong grammar: “… however … which is usually absent …”?
- Line 458: “that is key” → “is key”?
- Lines 563/64: The construction of the sentence is a bit weird. Maybe better: “… conditions (H1, Section 4.1), it is perhaps not surprising that the TBMs show different responses of allocation to increased productivity following mechanisms $\mathrm{MI}_{NPP,F}$ or $\mathrm{MI}_{NPP,FS}$.”.

Literature

[1] *The European Code of Conduct for Research Integrity*, ALLEA - All European Academies, Berlin 2017, Revised Edition, `https://allea.org/code-of-conduct/`.