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Interactive comment on "An improved parameterization of leaf area index (LAI) seasonality in the Canadian Land Surface Scheme (CLASS) and Canadian Terrestrial Ecosystem Model (CTEM) modelling framework" *by* Ali Asaadi et al.

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We appreciate reviewer #2's comments on our manuscript. Our point-by-point responses are included below. The reviewer's comments are indicated in an italic font and our responses are in a regular one.

Reviewer:
General Comments

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The study by Asaadi et al. aims to improve the seasonal timing of LAI simulated by the CTEM model by including a representation of non-structural carbon (NSC) pools and fluxes in the model (in addition to a few other modifications). The new developments in the model are tested at three temperate broadleaved deciduous sites against LAI, carbon and energy flux observations. They show an improvement in the timing of various stages in the phenological cycle, and corresponding improvement in the timing of carbon fluxes (though limited improvement in energy fluxes). This is of use to the land surface modeling community as not all LSMs currently include specific NSC pools and related processes. As the authors have discussed, NSC processes are relevant for other components of biogeochemical cycling or ecosystem functioning, in addition to phenology that they focus on in this study (though this discussion could be expanded). The paper is clearly written and structured (although the goals of their model development work might be better stated as questions or hypotheses). However, I have some concerns about the lack of breadth of the study and depth of the analyses undertaken, which are detailed more in specific comments and outlined briefly here. 1) The addition of non-structural carbon pools and associated fluxes is the primary focus of the study; however, there are no observations of NSCs used to evaluate the authors' modifications to the model. I would have expected that any model modification would be tested against observations that are directly relevant to the new processes added in the model even though I appreciate that NSC data are scarce (see Dietze et al., 2014 for a review of previous such studies in the literature). Why was this not the case? Although the authors state the sites chosen were those with available LAI data, was it not possible to evaluate this model at any site that had observations of non-structural carbon pools (even if those data came from sites that did not also have LAI data)? The authors only chose three sites representing only one plant functional type. I would think there are a greater number of sites with LAI data that this model could be tested against.

Authors:
As mentioned in many other studies as well as in Dietz et al. (2014), plants physiological processes are extraordinary complex and there is still a considerable debate and

lack of understanding regarding the behavior of NSC reserves. In addition, there are huge differences between plant species as to how they use their NSC reserves (e.g., Li et al., 2016; Wiley and Helliker, 2012; and Hoch et al., 2003). We are not aware of any long-term NSC measurements where meteorological data are available to drive the model with and LAI measurements are also available to evaluate the model. The relative size of the NSC pools (~5-10% of the total carbon pool in each component of a tree) is inferred from observations (e.g., Li et al., 2016) and the model aims to achieve this. The NSC-reallocation process during bud burst follows the classic source-sink paradigm. This study is the first attempt to include NSC pools and their associated processes in the CLASS-CTEM model and in our humble opinion model evaluation at three sites is enough to illustrate the proof of the concept. Evaluation of the model at regional and global scales will follow in due course but these are subjects of our future studies.

We have, however, followed on other recommendations of reviewer #2 as explained below in answers to his/her comments.

Reviewer:
2) While the authors detail improvements in their modified model in comparison with observations (though less so for energy fluxes), it is not clear which of the model modifications made (detailed in Sections 2.1.1 to 2.1.4) are responsible for the improvements in the simulated carbon and energy fluxes. The authors could show the impact of each modification individually, before evaluating all together. I think the modeling community would appreciate knowing how each of the different modifications made to the model contributed to the overall improvement in the model. Such an analysis would help them ascertain if there are potential structural deficiencies in their own model, thus placing this work in a wider context.

Authors:
Thank you for this suggestion. We have carried out such an analysis and this is illustrated for the Harvard Forest site in the figure below. A similar behavior was seen at

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the other two sites. Panel (a) of the figure below indicates the resulting LAI after implementing the first step (i.e., reallocation of NSC during leaf out period, presented in section 2.1.2). The reallocation occurs only in early spring and as expected, it improves the ascending side of the simulated LAI during the same period. In panel (b), the effect of the second modification which has been discussed in section 2.1.3 is also included. As it can be seen, reducing the allocation fraction to leaves after summer solstice affects the LAI during the peak growing season and shifts the annual maximum LAI to earlier days of the year. Finally, the effect of the third modification step, section 2.1.4 in the text, is also included (panel (c) on the figure below). Increasing the lower air temperature threshold after summer solstice leads to an earlier leaf litter fall (not shown) and consequently an improvement on the descending side of the LAI curve. Together, the three modification steps lead on an overall improved CLASS-CTEM's simulated phenology.

Reviewer:

3) The analysis lacks depth – namely, there is a lack of a rigorous quantitative evaluation of the modified model. It would be useful to include certain metrics to quantify the improvements simulated by the modified model (simple correlations for example). In addition, given the authors state that their primary goal is to address the issue of delayed leaf phenology, their analyses should be focused only on that question; general discussions of model behavior and magnitude of fluxes are distracting, especially given they have decided not to run a historical or transient simulation of the model after the spinup, with increasing CO_2 and climate.

Authors:

We will add coefficient of correlation (R^2) and root-mean-squared error (RSME) to Figs. 4, 5, 6, and 10 when revising our manuscript. We, however, respectfully disagree that general discussion of carbon fluxes is distracting. While LAI is an important variable, it is the land-atmosphere CO_2, energy and water fluxes which determine the regional and global climate in an Earth system model. Assessment of carbon and energy fluxes is an important part of any model evaluation exercise. We are unable to perform historical transient simulations because we do not have long-term meteorological data to drive the model with. The conclusions derived in the manuscript will not change even if we were able to perform transient historical simulations. Performing transient historical simulations would have provided the current observation-based annual positive values of net biome productivity to evaluate the model against but this is not the focus of our manuscript. Our past experience shows that steady state simulations allow an easier interpretation of model modifications. We will expand on this discussion when revising our manuscript.

Reviewer:
4) The authors state in the discussion around lines 534-535 that the omission of NSC pools in the original model was a structural error. However, they do not definitively provide evidence to support their claim that the omission of NSC pools was a structural error. While their results show that this process can improve model LAI temporal dynamics, they have not conclusively shown that this is the only process that could be responsible for any discrepancies between the model and the observations, and therefore how important it is to add these specific processes. Incorporation of NSC pools and fluxes may not be the only process that can alleviate the problems in the simulated LAI. As they go on to state, biological systems are complex and difficult to represent with physical equations in models. To ensure that we do have the right model behavior, the processes we include must be rigorously tested against data corresponding to that process. Ideally, the authors would test alternative functions available in the literature for the processes they have implemented, in order to estimate the structural uncertainty associated with the new model developments. A Bayesian model selection framework could be used in order to select the most parsimonious model based on a model selection criterion (such as the Akaike Information Criterion – see Melaas et al., 2013 for an example). I would also be interested to see an analysis on the uncertainty related the parameters they have implemented. It might then be useful to discuss other NSC related processes that remain poorly understood that are not captured by their

new model.

Authors:
An aspect of model testing that we omitted in the manuscript is that in the past we tried implementation of time-dependent maximum carboxylation capacity of Rubisco (V_{cmax}) as a function of the day length at each latitude. Since V_{cmax} is typically much larger early on in the growing season than during the latter half of the season, we suspected this may fix the problem of delayed leaf out, but it did not. The fact that plants do indeed use their NSC pools for flushing new leaves and that such parameterizations did not help, indicates that omission of NSC pools in the original model version was indeed a structural error. Testing alternative available parameterizations is a big exercise which is beyond the scope of our study and parameterizations from other models require that they fit within the conceptual framework of our model.

Reviewer:
5) The discussion lacks depth as to how the models they have implemented compare to other studies that have already implemented NSC models, as well as a discussion of any caveats to their modeling work related to the points I mention here. See specific comments.

Authors:
Thank you for pointing this limitation which was also raised by the other reviewer. We will modify our manuscript to include references to and discussion of existing work on inclusion of NSC pools in ecosystem models and compare their conceptual model with ours.

Reviewer:
Specific Comments
Introduction
Lines 109-111: Unless I have misunderstood, this model has been used in a phenology comparison at these sites (Richardson et al., 2012). If I have the right model, it

seems to me that the problems in the behavior of CTEM (for simulating LAI) shown in Richardson et al. are different to that in Anav. This shows that there might be other issues in the phenology models already implemented in CTEM due to differences between versions/parameterizations, without the addition of new processes/modifications to the model?

Authors:
Results presented in Richardson et al. (2012) were produced using a very old version (which dates back to year 2005) of the model utilized by a university collaborator who coupled it with his own nitrogen module. The model has evolved considerably since then and includes several new parameterizations. As an example, the two simulated LAIs for the US-UMB site (Fig. 1 in Richardon et al. and Fig. 6 panel a in our study) are not similar.

Reviewer:
Model, data and methods
Sections 2.1.1 and 2.1.2 There is a lack of references and/or reasoning for some of the mechanisms they are implementing in the models and the various assumptions they make in doing so (e.g. assumption that respiratory losses occur from non-structural part – line 176, and the references/reasoning behind formulation in equations 5, 6). In addition, the reasoning behind fixing certain parameter values needs to be detailed (e.g. why $\mu_i = 70$ *line 191; beta in line 241). Were the parameter values found from the literature, or perhaps they were calibrated based on sensitivity studies or optimization experiments?*

Authors:
Thank you for pointing this out. We will add references and expand text to justify our parameterizations. For example, maintenance respiration which is temperature dependent occurs only from the nonstructural pools in agreement with observational and modelling studies (e.g., Hoch et al., 2003; Sperling et al., 2015; and Li et al., 2016) which show that plants' NSC stores become depleted due to excess respiration during

dormant season and during disturbances such as cold and drought stresses. Since biological processes have to be parameterized their parameters have to be calibrated to obtain realistic model behaviour. We will expand on this and discuss the basis for choosing the parameter values that we have used.

Reviewer:
Line 186: maybe refer to Section 2.1.2 for how is Tj calculated?

Authors:
Agreed.

Reviewer:
Section 2.1.3 Are you referring to the fact that CLASS-CTEM has a flat peak of around 2 months in Anav et al., 2013 Fig 11, as opposed to the sharper (~1 month) peak seen in the observations and other models? In that case, it might be good to just state this in parentheses, as I was distracted by the fact that LAI simulated by CLASS-CTEM does not start to decline until long after the summer solstice (Anav et al., 2013 Fig 11) and much later than the observations. In any case, is there evidence in the literature that allocation fractions is modulated by day length – as represented in Section 2.1.3? No references are given to support the addition of this process. Could this slower rate of decline be due to incorrect parameters/processes related to senescence? Initially I was more distracted by the fact the seasonal cycle is delayed (out of phase) by a month or so. I am not therefore convinced if this correction factor based on day length presented in Section 2.1.3 is needed on top of other structural changes in the model.

Authors:
We will make it explicitly clear the problems with simulated LAI in Anav et al. 2013. Yes, indeed there are two problems with simulated global mean LAI – it peaks later on with a much flatter peak, and it starts to increase later than observation-based LAI. We will also cite additional references in support of using summer solstice as a date to adjust our allocation fractions. Adjustments to allocation fraction for leaves after

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summer solstice have also been made in other studies (e.g., see Eq. (6) of Gim et al., 2017). Summer solstice is a trigger for many other physiological processes. For instance, Luo et al. (2018) showed that summer solstice marks a seasonal shift in plant growth, leaf physiology, and foliage turnover in temperate and boreal forests. In earlier versions of the CLASS-CTEM model, continuous allocation of net primary productivity to leaves led to increase in LAI throughout the growing season rather than a constant or slowly decreasing LAI over the growing season. The effect of the adjustment which we have made to the allocation fraction to leaves after summer solstice is more obvious in the figure above which indicates the effect of each model parameterization separately.

Reviewer:
Section 2.1.4 Similarly to Sections 2.1.1 (above), why is a value of 12 °C now used for T_{cold}^{leaf} *? Is this based on the literature, or experiments, or a calibration exercise? Please give details and/or references as to how this value was chosen.*

Authors:
The value for T_{cold}^{leaf} has been obtained based on a calibration exercise during our third modification step (section 2.1.4) and its individual effect will be shown in a supplementary figure based on the figures shown above. Model parameters depend on their parameterizations. In the CLASS-CTEM model, leaf loss due to cold stress begins when air temperature falls below thethreshold but its rate accelerates as temperature continues to drop. Leaf loss rate due to cold stress is maximum when temperature falls 5 °C below thethreshold.

Reviewer:
Section 2.2.2 Please could you detail where you got the site meteorological data from, and which method and/or software you used to gap-fill the met data? Also, please could you detail why you chose to use a CO_2 *concentration of 350 ppm (this is detailed around line 418 in the results, but needs to be put here). Finally, please could you detail how the LAI measurements were made at each site? Are there differences between sites? This information would be helpful for readers.*

Authors:
As mentioned on lines 113-115 of the discussion manuscript, gap-filled meteorological data are obtained from the Fluxnet network. We will move the part detailing the CO_2 concentration to section 2.2.2. LAI measurements are taken using an LAI-2000 plant canopy analyzer instrument (details are provided in Urbanski et al., 2007, Schmid et al., 2000, and Gough et al., 2008 for the Harvard Forest, Morgan Monroe, and U. of Michigan sites, respectively). We will make this clear when revising our manuscript.

Reviewer:
Results
Figures 4-6: It would be good to state that both simulated and observed values represent averaged daily values across all years where data are present in the figure captions.

Authors:
Agreed. We will mention this when revising figure captions.

Reviewer:
Section 3.1 It would be helpful to have some metrics that show improvement (or lack thereof) between model versions for the full timeseries at each site. Even just RMSE or R would be helpful to quantify this and help put the results in context. This could be added to Table 3 for example.

Authors:
We will add R^2 and RMSE to each figure when revising our manuscript.

Reviewer:
Lines 382-384. It would be helpful if the authors showed a comparison of the observations and the model for each of the different modifications to the model that the authors have made in this study (as described in Sections 2.1.1-2.1.4), in addition to the overall improvement brought about by all modifications together. That way, other modeling groups can assess which modifications might be necessary for their own model – thus

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making the study useful in a wider context. These may only be put supplementary figures or tables, but it would still be useful to discuss in the text.

Authors:
As mentioned above and shown in the figure we have performed such an analysis and we will include this in the supplementary information.

Reviewer:
Lines 390-391: This is true, and a good result. However, I also noted there seems to be an offset in the start of LAI and GPP in the observations at both US-MMS and US-UMB. At US-MMS the onset of LAI now better matches the observations, but there is now a bias towards a too early rise in GPP. Similarly, although the LAI at US-UMB now better matches the observations, and the GPP matches the observations very well, there is a still this offset. Why do you think that is? Is there a discrepancy between the two types of observations?

Authors:
Indeed! The observations of GPP and LAI are not perfectly consistent with each other. The two sets of observations are made by different groups of people. This highlights the issue that when comparing model results with observations care must be taken to ensure that observations make sense. We will expand more on this aspect when revising our manuscript.

Reviewer:
Lines 396-414: Why haven't the authors run a historical simulation after their spinup using increasing CO_2 values, so that they can compare to the observed NEE and ecosystem respiration more directly, rather than comparing the (naturally offset/biased) equilibrium state of the model? I appreciate that the lack of a site and disturbance history would result in biases in the model simulations, but this spinup + historical simulation protocol is very common, and I presume is normally used to run CTEM for model inter-comparisons as well as climate change simulations? The authors state

that their primary objective is to evaluate the temporal dynamics. But I do not see any issue therefore with running a historical run – as is the often used protocol – and then stating more clearly that their only goal is to look at the temporal dynamics. In any case, the decision to only compare the model at its equilibrium state (as detailed in lines 421-425 for example) should be put in the methods, not in the results, so the reader is fully aware before they get to the results.

Authors:
Doing a transient historical simulation for each site requires CO_2 concentration data (which we have) as well as meteorological data (which we do not have). We mentioned this in section 2.2.2 but we will expand on this even more. Note that the increased ecosystem respiration at present day is not only due to increased GPP (associated with increasing CO_2) and therefore somewhat increased size of carbon pools (compared to what would be obtained from spinning a model at 350 ppm CO_2), but also the increased temperatures that ecosystems are subjected to over the historical period. Without meteorological data that shows a warming trend over the historical period, we would not be able to properly simulate increased respiration at present day. We perform transient historical simulations on a routine basis but chose not to do so for this study since our objective was to evaluate seasonal dynamics of LAI in a clean manner.

Reviewer:
Section 3.2 Line 440: I am a bit confused as the stem's NSC pool does not get depleted in Figs 7-9c? It decreases a little, but not by a large amount as a fraction of its size? I also would expect that given the addition of NSC pools is the main focus of this study, the model should be evaluated at sites which do have NSC data.

Authors:
Very little carbon is actually needed to construct leaves. In fact, on average, the total NSC pool for trees are estimated to be enough to completely rebuild the whole leaf canopy 1-4 times (Dietze et al., 2014). Even higher values have been mentioned in other studies (e.g., Hoch et al., 2003, and Mei et al., 2015).

Reviewer:
Section 3.3 I find this section somewhat distracting given, aside from the last sentence, the differences between the original and modified model are not discussed much. In fact, the differences are very small. The authors note this, but do not provide any discussion as to why the change in seasonality of the simulated LAI does not alter energy fluxes more, as one might expect.

Authors:
Actually, we discussed this briefly on lines 504-508 in the Discussion section but will expand on this and include it in section 3.3 as well. The reason for much lower effect of changes in LAI on latent heat fluxes (or equivalently evapotranspiration), than for GPP, is that while GPP is solely determined by LAI, evaporation also occurs from soil and from intercepted water on canopy leaves. If evaporative demand for a given available energy cannot be met by transpiration then the demand is met by evaporation from soil. Although, of course, soil evaporation also depends on soil moisture in the top soil layer. But generally speaking, the different components of evapotranspirative flux are able to compensate for each other. As a result, small changes in LAI do not affect latent heat flux as much as they do GPP.

Reviewer:
Discussion and conclusions
Aside from the conclusions part to this section, I find the rest of this section lacks a more in-depth discussion in places. There is some discussion of future perspectives to further improve the modeling of LAI (lines 513-525), and the possibility to include the other processes such as drought mortality and the N cycle due to the requirement to model N in leaf NSC pools (lines 552-554). However, there could also be more discussion of the results that might place them in a wider context. E.g. what are the implications for the wider modeling community? How do your results compare to ways NSC-related processes have been implemented in other NSC modeling studies (see review and references in Dietze et al., 2014). A discussion of any caveats to their

work would also be useful. These might include some of the points I raised in my general comments, or a discussion about the uncertainty in NSC processes implemented and/or those that remain poorly understood (as the authors stated in the introduction).

Authors:
We will add more discussion along the lines suggested by the reviewer including comparison to other models who have implemented NSC pools. We will also include discussion of implications for the wider modeling community when revising our manuscript including in particular the mention of our failed approaches taken to fix the problem of delayed phenology.

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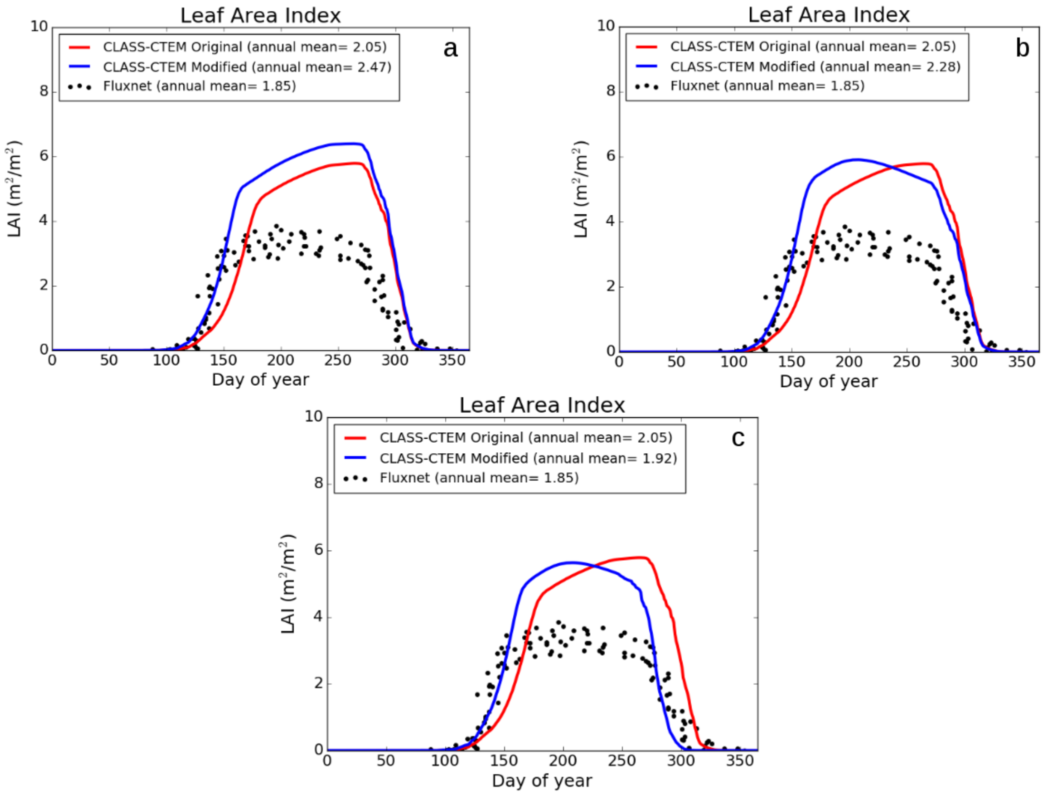
Leaf Area Index
a
CLASS-CTEM Original (annual mean= 2.05)
CLASS-CTEM Modified (annual mean= 2.47)
Fluxnet (annual mean= 1.85)
LAI (m^2/m^2)
Day of year
Leaf Area Index
b
CLASS-CTEM Original (annual mean= 2.05)
CLASS-CTEM Modified (annual mean= 2.28)
Fluxnet (annual mean= 1.85)
LAI (m^2/m^2)
Day of year
Leaf Area Index
c
CLASS-CTEM Original (annual mean= 2.05)
CLASS-CTEM Modified (annual mean= 1.92)
Fluxnet (annual mean= 1.85)
LAI (m^2/m^2)
Day of year
0
2
4
6
8
10
0
50
100
150
200
250
300
350

Fig. 1.