Interactive comment on "Using satellite data to improve the leaf phenology of a global Terrestrial Biosphere Model" *by* N. MacBean et al.

Anonymous Referee #1

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MacBean et al. "Using satellite data to improve the leaf phenology of a global Terrestrial Biosphere Model"

GENERAL
This study presents a framework to improve phenological parameters of a terrestrial biosphere model and the impacts on the simulated global phenology and carbon fluxes. The most significant change is the bias correction of delayed end of season in the orig- inal model. The results are interesting and the impacts are significant. I recommend some minor changes before the publication.

>> We thank the reviewer for reviewing the manuscript and for their kind comments.

First, the optimization procedure was not explained clearly. The authors estimated global minimum of the cost function by starting at 20 different random "first guest" points. It was not clear which model outputs were used in the calculation, fAPAR, SOS, EOS, or GSL? Did the 20 points mean 20 sets of parameters that change at the same time, or 20 different values for each individual parameter while fixing other parameters? Was the ORCHIDEE model required to re-run for each different set of x (parameters), that is to say 20 times?

>> We agree with all the issues the reviewer has raised here.

> Firstly it was perhaps not clear enough in the abstract that the NDVI are used to optimise the model FAPAR. Thus we have added an extra section in the following sentence in the abstract: "Here we optimized the phenology-related parameters of the ORCHIDEE TBM using satellite-derived Normalized Difference Vegetation Index data (MODIS NDVI v5) *that are linearly related to the model fAPAR*".

> We have also added this extra section into the following sentence in the introduction: "Here we focus purely on improving the timing of both spring onset and autumn senescence of ORCHIDEE at global scale, by using a novel approach to assimilate normalized medium-resolution satellite-derived vegetation "greenness" index data (MODIS NDVI collection 5) *that were linearly related to the simulated daily fAPAR*".

> We have also changed the last half of section 2.2.1 (describing the NDVI data) to make it very clear that the NDVI is used to optimise the model fAPAR: "Instead therefore, we considered a vegetation greenness index, the Normalized Difference Vegetation Index (NDVI), that is directly related to the near infrared (NIR) and red (RED) surface reflectance, ρ ($NDVI = \rho NIR - \rho RED / \rho NIR + \rho RED$). *This index is based on the fact that photosynthesising vegetation reflects a high proportion of the incoming NIR radiation, whilst absorbing most of the red. NDVI* has been shown to

be linearly related to fAPAR, though with uncertainties related to the issues mentioned above (Fensholt et al., 2004; Knyazikhin et al., 1998; Myneni and Williams, 1994). *Therefore although* NDVI is not directly related to a *physical property of the vegetation*, it does capture its seasonal cycle together with inter-annual anomalies*, and therefore can be used to optimise the model phenology (via the simulated fAPAR)*. In order to *optimise* the seasonality (but not the magnitude) of modelled *daily* fAPAR *using* the NDVI data, we normalize both to their maximum and minimum values of the whole time series at each site (following Bacour et al., 2015)."

> For the question about the 20 first guesses: 20 parameter vectors means 20 completely different parameter sets (with all paramter values different) that are used as a starting point in the optimisation (iterative minimisation). So no we do not mean 20 different individual parameter values with the others kept fixed. To explain the 20 random first guess experiments more clearly, we have added the following into the manuscript. First in Section 2.3.1 that describes the data assimilation system more generally, we added the following sentence on page 13319 Line 17: "The prior parameter vector is most commonly used as the starting point in the iterative minimisation, but it can be started from any point (set of paramete values) in the parameter space."

> We have then described what we did in more detail by modifying the last sentence in Section 2.4.1 with the following: "Following (Santaren et al., 2014) we tested the ability of the algorithm to find the global minimum of the cost function by starting *the iterative minimisation algorithm (see Section 2.3.1)* at different points in the parameter space, choosing twenty random "first guess" *sets of* parameters and performing a MS optimisation for each."

> We hope that together these changes make the description of the optimisation procedure clearer, but we would be happy to make further changes if required.

Second, the impacts of phenological change on carbon fluxes might not be quantified properly. The optimized parameters did shorten growing season length (Fig. 4). Moreover, I found that the peak of LAI also decreased significantly (Fig. 3c), which definitely contributed to the lower GPP (~10 Pg yr-1) in the updated simulations. It was unclear whether such reduction in LAI was related to the optimization of phenological parameters or the feedback of lower carbon uptake, which affects carbon allocation and tree growth. In addition, it was unclear how much reduction in GPP was related to the shortened growing season and how much was related to the decreased LAI in peak season.

>> It is true that the impacts on the carbon fluxes were not quantified properly; indeed this was a deliberate decision. In the original version of the manuscript we included a fuller description of the impact on the carbon fluxes, but the manuscript was too long and therefore the main messages related to the optimisation were lost. As a result we decided to cut this section out and just provide a little "teaser" in the discussion, with the view that we will investigate and quantify the impact more thoroughly in a later study. The suggestions you provide are indeed very interesting and would be valuable to include in a longer study. We did not want to add too much to the length of the

manuscript, but we have attempted to address the points you've raised and have therefore added in a few sentences, as described below.

> The question of whether the reduction in LAI was related to the optimisation of phenological parameters or the feedback of lower carbon uptake is indeed an interesting one. We investigated this and concluded that although less C is assimilated on an annual timescale due to the reduction in GSL (and LAI in some cases), the amount of C that is allocated to the carbohydrate reserves does not change as this pool is already large enough to meet the requirements set in the model for building a canopy. In fact, as less C is taken from the carbohydrate reserve during leaf growth due to the decrease of K_{lai_happy}, there is a scalar increase in the carbohydrate reserve as less Cis being used for leaf growth. The other explanation that we therefore provided was simply related to the decrease in the value of $K_{lai\_happy,}$ which is the parameter that determines the point at which the carbohydrate reserves stops being used and growth proceeds via normal photosynthesis and C allocation to leaves. At this point the growth normally slows down (in the ORCHIDEE "world" at least) due to limitations on photosynthesis that are not present when C is being taken from the carbohydrate reserve. The lower values of K_{lai_happy} after the optimisation mean that the growth from the carbohydrate reserves stops earlier, growth is limited, and we think this, together with the fact that the senescence starts earlier, is the reason for the lower LAI. This reasoning/explanation was initially discussed in Section 3.4 (P13331 ~Line 17), but we see that the discussion was more related to how the fit to the data were improved, rather than a discussion of the impact on the processes, as the reviewer has pointed out. We greatly appreciate the reviewer's comments as ultimately we hope to interpret the results in terms of processes rather than just the fit to the data! We have therefore changed one sentence: "In ORCHIDEE the rate of leaf growth slows down after this LAI threshold is reached as the C now comes from photosynthesis, which may be limited by various factors.", and added in an extra sentence to the end of that paragraph (P13331 Line 22): "The lower value of K_{lai_happy} (together with the earlier start to senescence) is also responsible for the reduction in the peak LAI at some sites for some PFTs, particularly BoND trees (see Figure 3 for example). It may be hypothesised that the optimisation resulted in a reduction in the C allocated to the carbohydrate reserve, which therefore may also have contributed to the decrease in the peak LAI. However an examination of the simulated carbohydrate reserve showed this not to be the case (results not shown). In any case this observed change in peak LAI was not a common result of the optimisation when considering all PFTs."

> The final question in the above paragraph about how much reduction in GPP was related to the shortened growing season and how much was related to the decreased LAI in peak season is also an interesting one. Practically speaking though, quantifying the relative contribution is quite a hard question at large scales because of non-linear and interacting effects. For example, for a certain region (e.g. the NH) where we can see that the predominant change is related to change in GSL, the relationship between the change in GSL and change in GPP is still nontheless non-linear, due to differences in physiology between PFTs. Even within a PFT we do not find a simple relationship, further pointing to regional conditions (e.g. soil texture) and different sensitivities to climate regime. Thus performing a simple and quick analysis such as multivariate linear regression between the change in GPP and the change in amplitude of the fAPAR plus the change in growing season length at global

scale, would not work. We would likely need to carry out an ensemble of simulations in a multifactorial experiment to covary the the change in GSL and amplitude. As we mentioned above, the impact on the C cycle was not the focus of this paper (and we do not want to add to the length) so such an experiment is beyond the scope of this study. Instead, we have added in a short qualitative description of the results we see, to try to answer the question posed by the reviewer. Therefore we added in one paragrpah around P13340 Line 13 after 'prior: 172.2 PgC yr^{-1}, posterior: 162.5 PgC yr^{-1}': "Reductions in GPP (results not shown) follow the same global spatial pattern as the difference in annual mean fAPAR (Figure 8c). The decline in GPP is predominantly caused by the shorter GSL in the high latitudes and grasslands across the NH (median of -30 and -10 days for boreal and temperate regions respectively), and in equatorial Africa and western South America (Figure 8a). The exception to this is eastern Siberia where the decrease in fAPAR amplitude, due to the lower peak LAI for BoND trees discussed in Section 3.4, contributes to an even stronger reduction in GPP. In east Africa and South America, the lower fAPAR amplitude is predominantly responsible for the decrease in GPP. The mean annual GPP only increased in the Sahel and northern Australia, which is due to the fact that the optimisation resulted in an increase in both the GSL and fAPAR amplitude in these regions (Figure 8)."

SPECIFIC

Abstract: Abbreviations (GPP, GSL, and fAPAR) are not defined.
>> This has been changed with the acronyms defined in the text. We have added the following for clarity: "…trends in the *vegetation productivity as represented by the GSL and mean annual fraction of Absorbed Photosynthetically Active Radiation (fAPAR)*"

Page 13315, Line 9: fAPAR and GSL are not defined.
>> Also changed, thank you.

Page 13318, Line 6: BRDF is not defined.
>> Thank you for pointing this out. As we don't feel the reader actually will be any the wiser if we explain that BRDF means Bi-directional Reflectance Distribution Function – we have removed "BRDF" and instead written "…atmospheric and directional effects (related to the change of reflectance with observation geometry) following…"

Page 13322, Line 11: "Fifteen of . . ., and the other fifteen . . ." How to determine which sites are used for optimization or validation?
>> This is indeed not detailed. We added the following sentence after the sentence you mention: "To separate the thirty sites into two sets of fifteen for optimisation and validation, we ordered all sites by their grid cell row number and took alternate points for each list."

Page 13324, Line 25: "multiple growing seasons" How to determine SOS and EOS for grids with multiple growing seasons?
>> We did not calculate the SOS and EOS for grids with multiple growing seasons for now. This is what we mean in the sentence "**Therefore unlike the SOS and EOS, the GSL** was also calculated for grid cells that contain multiple growing seasons

within a calendar year." If this is not clear to the reader we would be happy to make this more explict.

Page 13329, Line 10: "a negatively biased" Might be "positively" instead?
>> Thank you for spotting this mistake, indeed it should be "positively". We have changed this in the text.

Page 13333, Lines 13-14: "changes in the amplitude are the results of ..." Is that possibly contributed by the changes in the simulated carbon flux and tree growth as well?
>> We have discussed this point above, but agree with the issue raised by the reviewer, so we have added in a qualifier at the end of the sentence that they mention: "…timing of senescence *that ultimately result in a lower maximum LAI, as discussed in Section 3.4*"

Appendix A2: How does the model consider the impact of photoperiod on autumn phenology?
>> The model doesn't consider photoperiod, although as mentioned in the discussion (Section 4.2) this is something that may need to be included. We have verified this at the Appendix with the following sentence: "Note that no senescence models in ORCHIDEE currently include a photoperiod term for either onset or senescence."

Table 2: Is it possible to quantify the sensitivity of SOS/EOS to the changes of model parameters?
>> We think what the reviewer is suggesting here is how much of the change in SOS/EOS as a result of the optimisation is due to the change parameter X versus parameter Y etc. However this question could be interpreted more in a sensitivity analysis (SA) framework where we assess the global sensitivity of the model SOS/EOS with respect to each parameter, which is slightly different as that would include all possible variability in the parameter values, as opposed to just the changes that have resulted from this optimisation.
Whilst it is an interesting question, it is beyond the scope of this paper to include a thorough analysis in this regard, and we have already qualitatively discussed which parameters are likely responsible for the changes in section 3.4 as there aren't that many options for which parameter is likely responsible in this particular study. If the reviewer was suggesting "case 1", i.e. the first option, given that the interactions between parameters may be non-linear we would need to perform several multi-factorial experiments to covary all parameter combinations within the changes observed in the optimisation, in order to get an idea of which combination explained the change in SOS/EOS. Regarding the second option, we would need to do a global SA and examine the impact of the parameters on the SOS and EOS. A global SA has been performed for fAPAR but not explicitly for SOS and EOS. This is possible but a SA takes a reasonably long time as the whole parameter space has to be explored with a sufficient selection of all parameter combinations. Furthermore further processing of the model outputs would have to be implemented in the SA to extract the SOS and EOS dates. From the original fAPAR SA we anticipate which parameters are responsible, and indeed there are really only 1 or 2 that can be responsible for the SOS and EOS respectively. As mentioned above these have been discussed in the text. In any case the relative contribution of each will change depending on the

site/PFT/prevailing conditions/climate etc, as in some areas/species one may be more dependent on the other. So a definitive answer may not be possible.

Table 3: The 1st, 2nd, and 3rd columns should be 2nd, 3rd, and 4th columns instead.
>> Thank you for spotting this mistake. We have changed the table caption accordingly.

Interactive comment on "Using satellite data to improve the leaf phenology of a global Terrestrial Biosphere Model" *by* N. MacBean et al.

Anonymous Referee #2

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MacBean et al. used MODIS NDVI to optimize phenology-related parameters in a famous terrestrial biosphere model, ORCHIDEE and found that the model-predicted vegetation phenology had been overall improved via the optimization and the improvements varied with PFTs. The improved vegetation phenology led to shorten growing season lengthes and resulted in a substantially dereased prediction of global annual GPP by ~10 Pg C yr-1. These information indicates the important role of accurate representation of vegetation phenology in terrestrial biosphere models/earth system models, therefore is useful and helpful for a better simulation of the climate system. The manuscript is very well organized and written, I only have a few minor suggestions.

We thank the reviewer for taking the time to read and review our manuscript, and for their kind comments.

1) Page 13323, Line 8-9: why select "the greatest % reduction" first guess MS optimization rather than the one with lowest cost function?

In practice these were the same as the percent reduction was normalized to the initial value of the cost function with the prior parameter vector. To clarify this we have added this sentence to the end of that section (after the sentence mentioned above): "The first guess with the greatest % reduction in the cost function was equivalent to the first guess that resulted in the lowest value of the cost function, as the % reduction was calculated using the value of the cost function using the default (prior) ORCHIDEE parameters"

2) Section 4.5: were the numbers calculated with area-weighted grid-level values? Please clarify.

They were, but thank you for pointing out the lack of clarification. To clarify this we have added the following (in italics) on P13340 Line 13: "posterior: 162.5 PgC yr-1 *calculated using PFT fraction- and area-weighted fluxes for each grid cell* – results not shown".

3) Figure 2: NC3 and NC4 are the same figure.

Thank you for pointing out this mistake. The NC4 map was indeed wrong. We have changed the figure to include the right map.