Response to Anonymous Referee #2

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GENERAL

This study evaluates two phenological schemes implemented into a land surface model (LSM). The comparisons show that simulations with GSI scheme perform better than those with GDD scheme. The authors further estimate that simulated GPP with the former scheme shows smaller biases than the latter. Evaluation of phenological schemes for LSM is important. However, this specific study does not commit such purpose due to the lack of scientific contributions and flaws in the analyses.

First, missing of the scientific merits. The main purpose of the study is to compare two phenological schemes. Such inter-comparison has been widely performed (Chuine et al., 1999; Morin et al., 2009; Migliavacca et al., 2012). This study does not add anything new to the scientific community.

Response:

We must clarify that the main purpose of the study is not to compare the two phonological schemes. The purpose of comparison of the model efficiency of DLM-GSI with DLM-GDD using the in situ data in this study is to assess that how model accuracy will increase if more representation complexity is involved, instead of just simply comparing the performance of GSI with GDD.

Morin et al. (2009) developed models predicting the date of leaf unfolding (date of first fully formed leaf) for 18 North American temperate tree species representing 11 genera. In the paper of Chuine et al. (1999), The Spring–Warming model (Cannell & Smith, 1983), SeqSar and Par1Sar models (Chuine et al., 1999) were used. All of these three models are functions of temperature or based on the sum of degree-days. The models used in the

paper of Migliavacca et al. (2012), are two main categories of models. While the model categories differ in their assumptions of how warm and cold temperatures control developmental processes, none of them belongs to the kind of the GSI model. That is to say, none of the three reference papers performed the phenology schemes comparison. Lack of inter-comparison of phenology schemes can be found in the literature. We cannot agree with the referee that “such inter-comparison has been widely performed”.

Comment:

Second, flaws in the analyses. There are at least three flaws.

(1) Biased selections of phenological parameters. The authors explained that parameters were adopted from the literature. However, those parameters are appropriate for specific models and/or tree species, but may not be fit for the current study. Without reasonable calibrations, we do not know whether improper parameters contribute to the biases in the schemes. For example, the parameters of GSI scheme were further optimized based on GPP data (Lines 383-384) but those for GDD scheme were adopted directly from CLM model (Lines 388-389). This may explain why the former has better performance.

(2) Improper usage of meteorological forcings. As described in the text (Line 210), the GDD scheme relies on soil temperature while the GSI scheme does not. However, soil variables are adopted from a model instead of observations (Lines 367-369). This also contributes to the biases in GDD approach.

(3) Unnecessary repetition in the comparisons. The authors investigated the impacts of phenological biases on carbon uptake. They found that, relative to GDD scheme, GSI scheme has smaller biases in both phenology and GPP. However, the observations of phenology are derived based on GPP (Line 302). As a result, it is not a surprise that one scheme with better performance for GPP-derived phenology (Figure 6) also has better performance for GPP simulations.

Response:

I will respond to the above three points, respectively, as below:

(1) The parameters of the GSI scheme were further optimized using GPP data but those for GDD scheme were simply adopted from the CLM model. Why we did so is to assess the applicability of these two phenology schemes with certain accuracy. The GDD model was simply parameterized in order to test how well it will perform when it is applied to large scales with less proper input information available. The objective of coupling DLM with GSI is to develop a more specific and local scale model, so the parameters of the DSI model were initialized and optimized based on *in situ* measurements.

(2) The measured soil temperature is not always available. And also for testing the capability of GDD, we used the modelled soil temperature as model inputs. Actually, the phenology model is coupled with land surface model. Few studies used measured soil temperature data as phenology model inputs.

（3）We partly agree that the phenology model initialization using GPP data would improve GPP simulation accuracy. It is not necessary that the GPP simulation with DLM-GSI must better than DLM-GDD though the former used measured GPP to initialize its parameters. In this study we aim at quantifying the improvement in GPP modeling using calibrated GSI comparing with those using the default parameters of GDD. We need this model comparison research to develop/improve a coupled dynamic land surface model suitable for a scope of local to intermedia scales and for different research purposes. \

Comment:

Third, the writing of the paper needs further improvements. I found many redundancies in the text. For example, the last paragraph of section 3 (Lines 557-563) can be replaced with the last sentence, because the whole paragraph is repeating the same conclusion. In addition, most of discussion section lists only problems and uncertain ties for current schemes, with limited explorations of the causes, consequences, and/or implications.

SPECIFIC Line 44-45: Sequence of references should be chronological. Similar problems exist for other citations.

Line 62: How to define “explicit”, “implicit”, and “both”?

Line 75: Format of the citation should be “(Arora and Boer, 2005)”. Similar problems should be corrected. Line 103: “researched” should be “researches”.

Lines 183-188: This paragraph is almost identical to that for spring phenology (Lines 165-170).

Lines 334-346: Most of this paragraph is more appropriate for the discussion section. Reference Chuine, I., Cour, P., and Rousseau, D. D.: Selecting models to predict the timing of flowering of temperate trees: implications for tree phenology modelling, Plant Cell Environ, 22, 1-13, doi:10.1046/J.1365-3040.1999.00395.X, 1999.

Migliavacca, M., Sonnentag, O., Keenan, T. F., Cescatti, A., O'Keefe, J., and Richardson, A. D.: On the uncertainty of phenological responses to climate change, and implications for a terrestrial biosphere model, Biogeosciences, 9, 2063-2083, doi:10.5194/Bg-9-2063-2012, 2012. Morin, X., Lechowicz, M. J., Augspurger, C., O'

Keefe, J., Viner, D., and Chuine, I.: Leaf phenology in 22 North American tree species during the 21st century, Global Change Biol, 15, 961-975, doi:10.1111/J.1365-2486.2008.01735.X, 2009.

Response:

Many thanks. We will carefully improve the writing of the paper when we revise the paper.

SPECIFIC Line 44-45: Sequence of references should be chronological. Similar problems exist for other citations.

This suggestion will be followed in the revised version.

Line 62: How to define "explicit", "implicit", and "both"?

Thanks. It will corrected as "…which is embedded in LSMs either explicitly or implicitly.

Line 75: Format of the citation should be "(Arora and Boer, 2005)". Similar problems should be corrected. Line 103: "researched" should be "researches".

Thanks. The suggestion will follow.

Lines 183-188: This paragraph is almost identical to that for spring phenology (Lines 165-170).

These two paragraphs discuss "leaf green-up" and "leaf defoliation", respectively. I will rewrite this paragraph in the revised version.

Lines 334-346: Most of this paragraph is more appropriate for the discussion section.

Thanks for this comment. We will follow your suggestion to move this paragraph to discussion section.