Dear Editor,

We wish to thank you and the referees for your precious time in reviewing our paper and providing valuable comments. It was your valuable and insightful comments that led to possible improvements in the current version. Following the editorial office suggestions, we combined the Supplementary figure‘s captions and the figures themselves into a single pdf supplement file, we also checked and revised the figures to ensure that figures are accessible to colorblind readers.
We have carefully considered the comments and tried our best to address every one of them. Below we provide the point-by-point responses to referees' comments. Texts in italic are the referees' comments (C), those in black bold style are our responses (AR), and texts marked in red are relevant changes in the manuscript. A marked-up version, showing the changes in the revised manuscripts, has also been prepared to be submitted accordingly. The page and line numbers in this letter refer to the marked-up version. We hope that you will find the changes satisfactory.

Sincerely,

Bahar Bahrami on behalf of the co-authors
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Referee # 2

Dear Referee,

Thank you very much for your time and attentions on this work. The comments and suggestions are very useful to improve our manuscript. We paid detailed attention to all comments and have addressed all of them below accordingly. We also would like to thank you for the introducing new papers, they are indeed very interesting and helpful. Please find our response in the supplement.

General comments

Bahrami and colleagues presented a manuscript describing the Parsimonious Canopy Model (PCM v1.0), that estimates gross primary productivity and leaf area index. The manuscript

is well written (with some technical notes below) and, in my opinion, useful since the authors provides the code for the PCM in R. I have two main concerns: Please consider including in the title: "Developing a Parsimonious Canopy Model (PCM v1.0) to Predict Forest Gross Primary Productivity and Leaf Area Index on deciduous broad-leaved forest" or something that limits to the actual coverage of the study. Right now, the model only has been tested in this type of ecosystems (with good performance), and the actual title kind of oversells the coverage.

We appreciate the reviewer's overall positive assessment to our work. We understand the reviewer remark regarding title and therefore have revised the title that now more clearly state the "deciduous broad-leaved forest" for which we have developed and tested our model. The revised title is:

"Developing a Parsimonious Canopy Model (PCM v1.0) to predict forest gross primary productivity and leaf area index on deciduous broad-leaved forest"

The phenology module. It is not clear if the phenology module estimates the start and end of the growing seasons using the warm-up period and then these values are used in the subsequent years. If so, this is a limitation of the model, since the SOS and EOS can be different over the years, influencing the carbon uptake period. At the end the annual sum might be correct/similar, however for incorrect reasons. This should be clearly stated in the limitation of the model, if it is the case.

Thanks for these remarks! In fact, the phenology module is run for each year. It uses the number of chill days (it counts days with daily mean temperature less than 5 degree centigrade) from winter solstice of the previous year as a variable which influences the budburst occurrence of each next year. We used the warm up period term referring to the last 10 to 11 days of each previous year that are eventually required for estimating variables in the phenology module for its uninterrupted run in the subsequent year. Indeed, what we observed using phenology module was different SOS's and EOS's during the study period at each site. And the start and end of carbon uptake is exactly in accordance with the SOS and EOS in each individual year.

Specific comments and technical corrections

C 1) Please check the use of the expression "e.g.", in the text it is used as "e.g.," with the comma, while in the abstract is not.

AR 1) Thanks. We changed "e.g." to "e.g.," over the text.

C 2) Please check over the text the use of "R^2" in uppercase, it should be in lowercase since it is a 1:1 comparison.

AR 2) We modified "R^2" to "r^2" in the text.

C 3) Over the text, please use italics when referring to a parameter (i.e., coefficients/parameters from Table 3).
AR 3) Thanks for the comment. We used italics when referring to parameters in the text accordingly.

C 4) Epsilon is in Eq. 3, not Eq. 4, please correct.
AR 4) Thanks for noticing. We modified the equation number to Eq. 3 [Pg. 7, l. 195].

C 5) L217-218. Please check the references in this sentence.
AR 5) Thanks. We Modified the sentence [Pg. 9, l. 268].
"According to Granier et al. (1999) and Fischer et al. (2016) the scw ..."

C 6) L260. If $f_{SP} = f_{ST}$ *(Eq 21), why not make it simple since Eq 18?*
AR 6) Thanks for your kind suggestion. By keeping this equation, we wanted to be consistent with the autumn phenology (Eq. 25), which is the next part where photoperiod factor f_{dl} also plays a role.

C 7) L288. It should be BIOME-BGC (check this all over the text), please check if this version also includes the MUSSO
AR 7) Thanks for the comment. We added the latest version as Biome-BGCMuSo v6.2. [Pg. 12, l. 340].

C 8) L346-347. How is the PAR-PPFD conversion done?
AR 8) Thanks for the question. We use the Fluxnet2015 PPFD variable in $micromol\ m^{-2}\ s^{-1}$ **and convert it to PAR in** $MJ\ m^{-2}\ day^{-1}$ **as following:**
4.5 $micromol\ m^{-2}\ s^{-1}$ **= 0.000001** $MJ\ m^{-2}\ day^{-1}$**, then multiplied to 86400 to get the corresponding daily values.**
PAR =PPFD-IN * 0.000001 / 4.5 * 86400

C 9) L348-349. Does this mean that the phenology submodel parameters are fixed according to the warm-up year to the subsequent years? Are there implications for using this? Could the authors report the values of the start and end of the growing season for each year of simulation?
AR 9) We actually meant that since the phenology module for each individual year needs the number of chilling days from the previous year, the very first year of the observation period cannot be included in the simulations. Instead it is used to determine the budburst day of the first modelling year. For instance if the study period is from 2006 to 2013 then the simulation starts from 2007 to 2013. We acknowledge this sentence was not clear enough. We added to the sentence to make this clearer. In the

following we also report the range of simulated Phenological transition dates including the start and end of the growing season (Julian date) for each year of simulation at the DE-HoH site.

Year	Start of growing Season	Maturity state	Start of leaf fall	End of growing Season
2015	109-123	162-186	257-270	296-298
2016	111-122	163-183	270	297-298
2017	101-129	168-183	251-270	290-298
2018	103-112	146-168	270-272	296-298
2019	105-114	154-179	271-273	297-298

"In other words, since the phenology module for each individual year needs the number of chilling days from the previous year, the very first year of observations is not included in the simulations. It is only used for to calculate budburst day of the first simulation year."

C 10) L476. Please check the references in this sentence.
AR 10) Modified [Pg. 18, l. 553].
"... in similar studies (e.g., Xiao et al. (2004) and Xin et al. (2019)); ..."

C 11) L486. Please check this reference (Hirmas et al., 2018, Nature) for increasing the discussion on how soil parameters should not be fixed. I liked this! Hirmas, D.R., Giménez, D., Nemes, A. et al. Climate-induced changes in continental-scale soil macroporosity may intensify water cycle. Nature 561, 100–103 (2018). https://doi.org/10.1038/s41586-018-0463-x
AR 11) Thanks for providing this reference. We added it to the discussion part of the revised manuscript [Pg. 19, l. 568].
"Hirmas et al. (2018) also showed that soil retention properties can change in time. For example, climate change may induce rapid changes in the soil macroporosity and the associated soil hydraulic properties. Those may alter the feedback between climate and land surface."

C 12) L503-504. "Therefore, corresponding parameters do not significantly influence the modelled GPP". This sentence is ambiguous, since I cannot interpret to which parameters the authors are referring to (i.e., temperature stress or phenology).
AR 12) Thanks for the comment. What we tried to explain is that in general upon arrival of favourable condition for plant growth, the period between SOS and EOS, temperature stress and the corresponding parameter roles on the GPP is less pronounced. To make this clearer, we have revised the texts [Pg. 19, l. 586].
"Therefore, temperature stress parameters do not significantly influence the modelled GPP."

C 13) L508. Please check how the references are used.
AR 13) Thanks. We modified the sentence [Pg. 20, l. 595].
"This is in agreement with the previous studies of Jung et al. (2007) and Lee et al. (2019), which showed that GPP output saturates and becomes insensitive at LAI values above 4 m^2 m^{-2}."

C 14) L571-583. This might be a good reference (Vargas et al) for the discussion of drought and Mediterranean ecosystems. Vargas, R., Sonnentag, O., Abramowitz, G. et al. Drought Influences the Accuracy of Simulated Ecosystem Fluxes: A Model-Data Meta-analysis for Mediterranean Oak Woodlands. Ecosystems 16, 749–764 (2013). https://doi.org/10.1007/s10021-013-9648-1
AR 14) Thanks for this reference. We have added it to the discussion part [Pg. 22, l. 574].
"Vargas et al. (2013), also discussed inter-annual dynamics of soil moisture effect on GPP flux in Mediterranean ecosystems using five process-oriented ecosystem models including water balance. They observed a systematically underestimation of GPP in the models that were accounting for soil water balance. Those underestimations may have been related to the complex nature of Mediterranean ecosystems, e.g., due to deep roots and an important role of the lower canopy. In contrast, here we overestimate the GPP and believe that this is due to lack of local information on soil moisture stress. More information of soil moisture stress is therefore expected to improve the model. Overall, they emphasize the importance of drought conditions and the complex nature of Mediterranean ecosystems in representing forest dynamics, including GPP flux."

C 15) In Tables 4-5, I recommend to the authors to report the linear regression coefficients (slope and intercept), not only RMSE and r^2, so the reader can know the biases.
AR 15) The linear regression coefficients are now added to the tables.

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