Responses to the comments by Anonymous Referee #2 on "Evaluating the carbon and nitrogen cycles of the QUINCY terrestrial biosphere model using remotely-sensed data"

Miinalainen, T., Ojasalo, A., Croft, H., Aurela, M., Peltoniemi, M., Caldararu, S., Zaehle, S., and Thum, T.: Evaluating the carbon and nitrogen cycles of the QUINCY terrestrial biosphere model using remotely-sensed data, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2025-2987, 2025.

Our responses are written below each comment separately. The referee comments are marked with blue color and italic, and the author replies are marked with black color. The original manuscript text is marked with orange color, and modified text with red. The line numbers refer to the original submitted version of the manuscript which was peer-reviewed.

This manuscript (MS) addresses an interesting and relevant topic by combining remote sensing and terrestrial modeling to explore the leaf chlorophyll and its role in carbon and nitrogen cycles. The study makes use of a very comprehensive dataset for model evaluation and remote sensing analysis, covering both spatial and temporal perspectives, and presents a number of interesting results and discussion points.

However, the overall presentation requires improvement to enhance clarity and readability. In particular, the M&M section would benefit from a more structured and concise description of the datasets that the model uses as input or for comparison, as the current presentation feels somewhat disorganized. Similarly, some points in the Discussion appear scattered and not well-connected, which makes it difficult to follow the logical flow of the arguments.

We thank Anonymous Referee #2 for the positive and valuable comments, which help us to clarify the presentation of the manuscript message. We will modify and streamline the M&M section to give a more clear description of the harnessed data and methods. We will also re-organize the Discussion section for improving the readability, and add subtitles to increase the focus.

Detailed comments:

Introduction:

L. 33: It appears that the sentence "TBMs use different modeling approaches to represent N limitation of photosynthesis" repeats the previous one.

We have modified the from at L33 from:
"For example, models have varying methods to represent the N-limitation of photosynthesis, which can lead to different results for plant productivity (Medlyn et al., 2015). TBMs use different modeling approaches to represent N limitation of photosynthesis and the effect of N availability on leaf N. "
to
"TBMs use different modeling approaches to represent N limitation of photosynthesis and the effect of N availability on leaf N, which can lead to varying results for plant productivity (Medlyn et al., 2015)."

L. 35: Why does increasing model complexity introduce additional uncertainties? In the previous sentence, you only mention different approaches that may lead to different predictions, but do not explain why greater complexity itself results in more uncertainty.

The modern TBMs have different approaches to model the N limitation, with a varying level of complexity. Increasing the complexity of process description will necessarily mean adding more process equations with associated parameters. This increases uncertainty in two ways: 1) process uncertainty as there could be multiple ways of representing one process and 2) parameter uncertainty given by new parameter values.

We have modified the text at L35 from

"Increasing model complexity can thereby also introduce further uncertainties into the estimates of the carbon sink (Fisher and Koven, 2020; Famiglietti et al.,2021), which is reflected in significant divergence of N pools and fluxes predicted by the current generation of TBMs (Kou-Giesbrecht et al., 2023)."

to

"Increasing model complexity regarding modeling the N limitation can thereby also introduce further uncertainties into the estimates of the carbon sink (Fisher and Koven, 2020; Famiglietti et al.,2021), through both process and parameter uncertainty given the inclusion of new process equations. These uncertainties are also reflected in significant divergence of N pools and fluxes predicted by the current generation of TBMs (Kou-Giesbrecht et al., 2023)."

L. 76: Is this a modeled response? I recommend clarifying this point here, as the previous paragraph mentioned models and remote sensing, which may confuse readers.

Here we indeed refer to a modelled response, thanks for pointing out the unclear aspect. We have now modified the sentence in L76 from:

"Initial results suggested that the response of chl_{leaf} to leaf N was not realistic"

to

"Initial results suggested that the modelled response of chl_{leaf} to leaf N was not realistic"

M&M

L. 97-119: The description of the model feels somewhat fragmented. I suggest establishing a clearer logical link between N cycling, its relationship with leaf chlorophyll, photosynthesis, and photosynthesis-related parameters. In contrast, the detailed descriptions of processes such as N uptake functions, maintenance respiration functions, and soil pools/layers seem less relevant to the main focus on leaf chlorophyll, carbon, and nitrogen, from my point of view. This leads to information spreading out, and the description of chlorophyll itself—just one sentence—seems insufficient.

We will modify and streamline Section 2.1. text describing QUINCY model processes to better serve the manuscript needs.

L. 120-130: Here are considerable details on the start and end of the growing season. It would be helpful if the authors could clarify whether and how this influences the LAI *simulation, which is the main output of focus. In lines 120 and 122, leaf biomass development and plant growth are mentioned—are these the same as, or directly related to, LAI development in the model? Overall, I suggest that the model description could be streamlined with a clearer emphasis on the outputs that are central to the study.*

This was a good suggestion. We have modified the text at L120 from:
"The seasonal development of leaf biomass depends on the ability of the plant to grow new tissues and the fractional allocation to plant organs."
to
"The seasonal development of leaf biomass and LAI depend on the ability of the plant to grow new tissues given available C and N as well as the fractional allocation to plant organs, constrained by allometric relationships and nutrient and water availability."

Also, we modified the text at L122-123 from:
"Both the beginning and the end of the growing season depend partly on the PFT."
to
"Both the beginning and the end of the growing season, which determine the LAI seasonal cycle, depend partly on the PFT."

L. 165: What are "other issues"? I suggest avoiding such vague descriptions, and it would be better to specify what these issues are.

By "other issues", the purpose was to indicate different aspects that made specific sites unsuitable for the data-analysis. For instance, the RS chl_{leaf} and RS LAI were missing for a number of sites. In addition, for a few sites, the input data for QUINCY was such that the simulations were not successful. This was due to, for instance, too strong water availability limitation.
We have modified the text from:
"Of the available sites, we excluded sites with anomalous precipitation data (Abramowitz et al., 2024) and other issues, leaving 143 PLUMBER2 sites."
to
"Of the available sites, we included 143 PLUMBER sites that had RS chl_{leaf}, RS LAI and QUINCY data available, and that were not reported by Abramowitz et al. (2024) to have anomalous precipitation input data."

L167: The RS chl_{leaf} data is available here, but you still retrieve the RS chl_{leaf} data later for these sites? Which one do you use for analysis?

Thanks for bringing to our attention that this part was ambiguous. What we meant was that at the end, we had 279 GLOBAL sites in our analysis after we had done RS chl_{leaf} postprocessing for the sites, and for which there were RS chl_{leaf}, RS LAI and QUINCY simulated data available. The requirement for RS chl_{leaf} was that the QUINCY PFT classification should match the land cover class of the site pixels and / or neighboring pixels, as described in Section 2.4.3.
We will reorganize the Materials and methods section, and add after L167 a text:
"(See Section 2.2.3)."

L177: Input is in which period?
We have modified the text at L183 from:
"The meteorological fields were obtained from the PLUMBER2 dataset and the CRU JRA dataset as previously mentioned." to
"For the PLUMBER2 sites, the meteorological fields were obtained from the PLUMBER2 dataset (Ukkola et al., 2022), and depending on the site, there were meteorological data available between years 1992-2018. For the GLOBAL sites, the meteorological data was obtained from CRU JRA dataset, and the data was from years 1989-2018."

L 183: In Section 2.2, you note that meteorological data are available for PLUMBER2, but there is no similar information provided for GLOBAL, making here a bit abrupt. It may help readers if you clearly specify, for each site set, which input datasets are available. Additionally, summarizing all available datasets used for either model input or comparison—including remote sensing and in-situ data—in a table could make the information clearer and improve the overall clarity, as you have various and large amounts of datasets.

We have updated the text (See the response above), and will add a new Supplementary table summarizing the data utilized in the study.

L190&192: How do you deal with the N and P deposition input in spin-up and simulations?

The N and P deposition input data are dealt in a similar manner as the atmospheric CO_2. We have updated the text to describe this. In addition, we noticed that there was an inaccuracy in the original manuscript text regarding the CO_2 concentrations and spinup, and we have modified the text now to be correct.
We modified the text in L189-19 from:
"Atmospheric CO2 concentrations were taken from a randomly selected year between 1901–1930, and meteorological data were taken from a random year of observed meteorological data."
to
"Atmospheric CO2 concentrations, N deposition and P deposition were taken by repeating the values from a period between 1901–1930, and meteorological data were taken from a random year of observed meteorological data. "

L 192: I cannot understand the sentence: this was continued ...; what are the respective years?

We have modified the sentence at L192 from:
"This was continued until the year when data from observed meteorology were available for the respective years."
to
"The transient simulation was continued with data from a random year of observed meteorology until the start of the period for which there were observed meteorological data available. The start of the period was site-dependent for the PLUMBER2 sites, and for the GLOBAL sites, the meteorological data started from 1989."

L259: Why do you specifically focus on evergreen needle-leaved forests? Does it mean that model simulations were also performed for these two sites? If so, this step is not described in the Model Simulation section.

Preliminary analysis indicated that QUINCY and RS chl_{leaf} show differing seasonal patterns for BNE sites in cold environments. Therefore, we wanted to do additional analysis for these evergreen needle-leaved sites for which there were in-situ chl_{leaf} data available. The evaluation of the RS chl_{leaf} product conducted by Croft et al. (2020) included needle-leaved sites, but not boreal sites specifically.
The two sites were included in the PLUMBER2 site set, which we had not mentioned in the earlier version.
We have modified the text at L259 from:
"To investigate the chl_{leaf} magnitude and seasonal cycle for the evergreen needle-leaved forests, we performed an additional comparison for RS and QUINCY output with in-situ observations for two sites:"
to
"To further investigate the chl_{leaf} magnitude and seasonal cycle for BNE forests, we performed an additional comparison for RS and QUINCY output with in-situ observations for two PLUMBER2 sites:"

In addition, we have moved the text describing the Sodankylä in-situ chl_{leaf} handling to Supplementary materials. This was done in order to improve the readability of the manuscript.

Overall, to improve clarity, I would suggest first describing all the datasets available at each site—for model input and model comparison—before explaining the model simulations. This would make the overall workflow easier for the reader to follow.

Thanks for the suggestion, we will re-organize Section 2 as suggested.

Sect. 2.7: It seems you did not describe your data analysis for the two sites with in-situ chl_{leaf} data? And the mention of additionally analyzing data for the Hainich site is a bit abrupt. Why do you want to investigate this site?

The referee is right that the comparison for the two sites was not mentioned here. We have now added after L344 the following text:
"We analyzed the seasonal cycle of chl_{leaf} for two evergreen needle-leaved sites, FI-Sod and US-NR1 (See Section 2.3.2) by comparing the QUINCY simulated data, in-situ observations and remote sensing observations. We calculated the averaged seasonal cycles over years for both QUINCY and remote sensing chl_{leaf} and compared them with in-situ observations. Furthermore, for the FI-Sod site, we also analyzed the seasonal cycle of LAI, fAPAR and GPP, and compared QUINCY simulated values to the observations. "

Both Referee #1 and Referee #2 pointed out that analysing the seasonal cycle for only one deciduous site (DE-Hai) seems a bit vague. Therefore, we decided to remove the part focusing on the Hainich site individually, and replaced this with an analysis of the seasonal cycle over all PLUMBER2 temperate broad-leaved deciduous sites. The text from L330-332 is now updated from:

“We analyzed the seasonal cycle of chl_{leaf} , LAI and GPP for one specific site, Hainich in Germany (DE-Hai, 51.08° N, 10.45° E). The Hainich site is located in the middle of a beech forest, and is characterized as a deciduous broad-leaved forest (TeBS). We also studied the seasonal cycle over all PFTs for the Northern Hemisphere (NH) sites by comparing the monthly PFT averages of QUINCY chl_{lea} and RS chl_{lea}. Tropical broad-leaved evergreen (TrBE) sites did not show detectable seasonality in either QUINCY or RS, and therefore these sites have been omitted from the seasonality analysis. In addition, we calculated the average values over April, May, October and November for the PLUMBER2 TeBS NH sites for the QUINCY results and observations, to study the differences in seasonal development..”

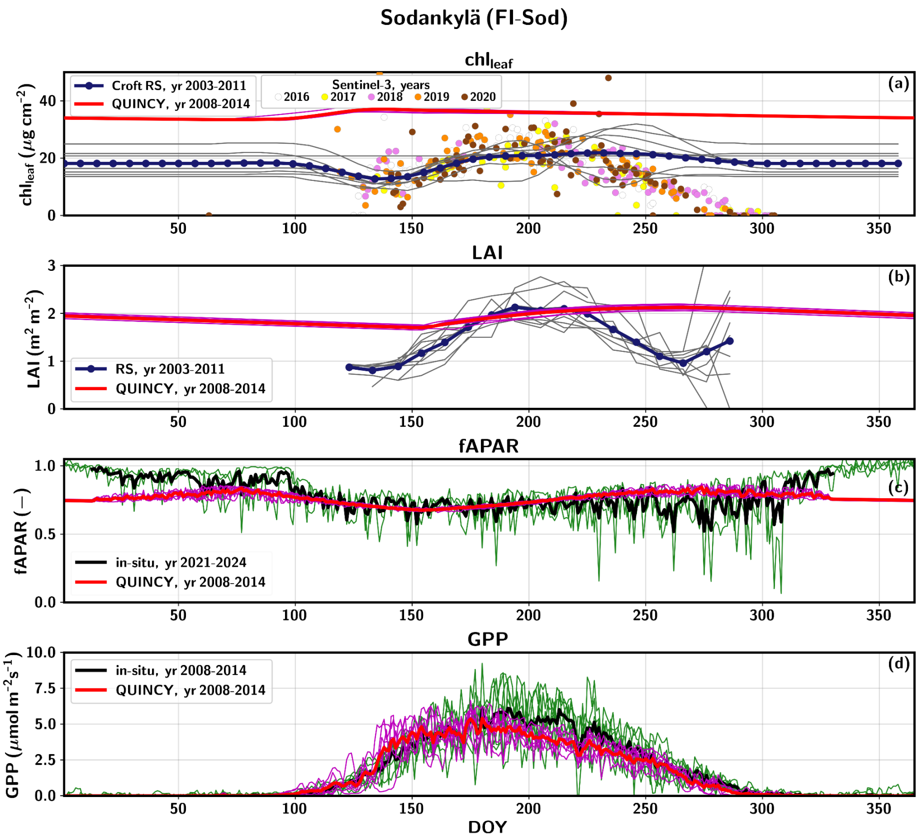
to

“We analyzed the seasonal cycle of chl_{leaf} and LAI for the PLUMBER2 and GLOBAL Northern hemisphere sites. In addition, analysis for the PLUMBER2 sites included also GPP. We first calculated the averaged seasonal cycle over years for each site and variable. Then, using the site-level averaged seasonal cycles, we calculated the mean seasonal cycle over the sites per PFT, and the standard deviation between sites for each DOY. This was done for QUINCY simulated values and for the RS and eddy covariance observations. For the PLUMBER2 Northern hemisphere TeBS sites, we estimated the start of season (SOS), the end of season (EOS) and the length of season (LOS) based on the PFT-averaged chl_{leaf}, LAI and GPP. We calculated the seasonal metrics using the method as described in Thum et al., (2025). The SOS and EOS values from the PFT-averaged GPP were calculated using the first and last pass of the threshold value. The threshold was set as the 30 % of the 90th percentile value of the PFT-averaged mean seasonal cycle of GPP. For LAI and chl_{leaf}, the threshold was determined using the difference between the summer and winter values. The winter values were calculated from January and February mean values, and summer values from June and July values. The threshold was then set to 20 % of the difference, added to the winter mean (i.e. $y_{thres}=x_{winter}+0.2*(x_{summer}-x_{winter})$). The earliest DOY for SOS was set to 50. LOS was calculated as a difference between EOS and SOS.

Results

L420: Since the years of remote sensing data and model simulations do not fully match, whether this might affect the comparison in terms of magnitude and pattern? Additionally, averaging over several years may reduce the apparent seasonal variation. Have you looked at the seasonal patterns for each year individually?

The referee is correct that the differences in the time intervals might affect the comparison. Below is attached a figure where the individual years are plotted for both QUINCY and observations. The QUINCY individual years are marked with purple lines and RS observations are marked with dark grey and in-situ observations with green lines.



As the figure shows, the interannual variability in QUINCY simulated data is lower than in the RS chl_{leaf} and RS LAI timeseries. Especially the RS LAI end of growing season values show a high variability between years. In GPP, we see more variation between years in QUINCY simulated data compared to LAI and chl_{leaf}. Similarly, the in-situ GPP observations show variation between years. To further analyze the GPP interannual variability in QUINCY and observations, we calculated start of season (SOS) and end of season (EOS) values for the yearly Sodankylä data. The SOS and EOS values were calculated with the same method as was used for the PLUMBER2 TeBS sites. The estimated SOS values for yearly Sodankylä GPP show less spread than EOS for both QUINCY and observations. For most of the years, QUINCY captures SOS quite well when compared to SOS from observations, while for EOS, the QUINCY model tends to predict too late end, approximately by one week.

To minimize the effect of interannual variability, we decided to use the averaged seasonal cycles of simulated and observed data in the comparison.

Discussion

The study analyzes a large amount of data from many perspectives and therefore raises a number of valuable discussion points. However, the discussion as a whole feels somewhat scattered (except 4.2).
For example, in Sect. 4.1, you started with chl_{leaf}-GPP-LAI, but in line 554 you mix the discussion of GPP and LAI, and then in the following paragraph, you return to GPP simulations, which were already discussed in the second paragraph. While I understand that these variables are closely related, this back-and-forth may confuse readers and make it

difficult to grasp the main points.
Secondly, the discussion of the relation between chl_{leaf} and GPP residuals appears in L568 and L654.
And in 4.4, you mentioned the limitation from RS to the model, and again back to RS and the flux tower in L714.
I suggest considering summarizing and reorganizing the discussion points to make the overall discussion clearer and more structured.

Many thanks for bringing this to our attention. We will re-organize the Discussion section and add subtitles to improve the readability.

L560: In addition, the sudden focus on a specific site (Hainich forest) feels abrupt. I always have the question about the rationale for highlighting this site? For example, is it because the simulations or RS data for this PFT show a distinct pattern compared to others?

As mentioned earlier, we have now replaced the analysis of the Hainich site with an analysis over PLUMBER2 NH TeBS sites. In addition, we added the calculation of the start of the season (SOS) and end of the season (EOS) estimates for chl_{leaf}, LAI and GPP.

L627: Which assumptions?

We have added the following sentence after L627:
"For instance, the assumptions made for the LAI seasonality and the effect of snow cover can affect the RS chl_{leaf} retrieval."

L655: This argument appears somewhat abrupt and does not have a clear logical connection with the preceding or following sentences.

We have moved the sentence at L655 to make the connection to be more clear with the surrounding text, and updated the sentence from:
"A comparison of QUINCY CN- and C-only simulations for the BNE sites indicated that QUINCY simulates an N deficit at low chl_{leaf} values."
to
"A comparison of QUINCY CN- and C-only simulations for the BNE sites indicated that QUINCY simulates an N deficit at low chl_{leaf} values, as GPP was lower with the CN-simulation."

Technical corrections:

L496: PFTs

Thank you for pointing this out, we have fixed the typing error.

L505: due to

This is now corrected.