Responses to the comments 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 in our replies refer to the revised, new version of the manuscript. The line numbers in the comments by the referees/editor refer to the original version of the manuscript which was peer-reviewed.

Responses to the comments by Associate Editor

__1.__ Thank you very much for submitting your manuscript "Evaluating the carbon and nitrogen cycles of the QUINCY terrestrial biosphere model using remotely-sensed data" to Biogeosciences. The reviewers and I appreciate the effort you have invested in this study and the proposed changes and recognize its potential contribution. Your proposed changes will be helpful in improving the manuscript.

We thank the editor for the comprehensive suggestions and comments on how to improve our manuscript. More detailed responses to the comments by the editor are written below. In addition, the final responses to the referee comments are added to this document.

In summary, the following points seem particularly important:

__2.__ Revise Discussion for a clearer structure (both reviewers)

We have done our best to improve the Discussion section as suggested. Further details are described in Referee #1 comments (See Referee #1 comment 21.) and Referee #2 comments (See Referee # 2 comment 17.). The main changes include adding more subtitles and re-structuring the text so that the presentation of the Discussion section is more coherent.

__3.__ Better describe leaf Chlorophyll and N-related representations and how alternative representations affect results (both reviewers)

To improve the manuscript description regarding chl_{leaf} and leaf N representation, we have made the following modifications:

-We have modified the text in Section 2.4, where details of the QUINCY model are presented (see reply to Referee #2 comment 5.)

-We have clarified the text in Section 3.3 and in Discussion (See reply to Referee #1 comment 2. and 26.)
-Additionally we have added to the Abstract one more sentence reflecting the change in the results, as suggested by the editor comment nr. 5.

4. *Make observational data publicly available (reviewer 1).*

We have now uploaded the RS chl_{leaf} data for the PLUMBER2 and GLOBAL sites to the B2Share repository of FMI, to be publicly available. In addition, *in-situ* fAPAR observations for the Sodankylä site (FI-Sod) are also made publicly available in the same repository, and *in-situ* chl_{leaf} for FI-Sod are shared via the Zenodo platform.

In addition to the reviewer's point and your replies, I recommend that attention is paid to the following points:

5. *Linking conclusions more clearly with your analysis, in particular the statement made on l. 16-17 ("the revised scheme produced a more reasonable sensitivity of gross primary production to increases in chlleaf.") and on l. 759 ("adding chlleaf to the model evaluation provides additional information on photosynthetic processes and leaf N distribution compared to using LAI alone"). The complementarity of information in LAI and Chl for model evaluation should be more clearly demonstrated and discussed.*

Regarding the revised N allocation scheme, we have added further clarification to Section 3.3 (See L522 onwards), and also modified the text in Discussion (L699 onwards).

In addition, we have now added to L17 the following:
"However, the revised scheme did not directly lead to improvement in simulating chl_{leaf} and gross primary production."

Furthermore, we have added to the Conclusions section, at L800, the following:
"While LAI provides information about seasonality, information based on chl_{leaf} complements this by enabling us to address the N status of the leaves and identify the main drivers of the chl_{leaf} content."

6. *Clarify model evaluation on seasonal cycle: It's ok to evaluate upper quantiles of seasonal variations, but the shortcoming of the model to produce the observed seasonality of leaf Chl in evergreen vegetation should be clearly stated and discussed.*

While the seasonal cycle of RS chl_{leaf} has been shown to correlate well with the in-situ observations for various forest types (See Croft *et al.*, 2020), our analysis indicates that the seasonality of remotely-sensed LAI is off for the Sodankylä site. This can be seen from Figure 3 from the new version of the manuscript, which presents a comparison of RS LAI and *in-situ* fAPAR observations. Our findings are also supported by expert knowledge we have from the site (based on site observations), and by the literature (Heiskanen *et al.*, 2012, Wang *et al.*, 2019).

The seasonality of the RS chl_{leaf} is derived from the seasonality of the RS LAI, therefore, potential biases from the LAI product will be reflected in RS chl_{leaf}. In our manuscript, we showed seasonal development of chl_{leaf} at two boreal needleleaf evergreen sites, and they do not support the results from RS chl_{leaf}. We are not able to obtain the observed seasonality of the RS chl_{leaf}, but we also would claim that the product could be potentially biased in this respect and therefore, adding that kind of seasonality to our model would be senseless.

We have now added to the Conclusions section, at L788, the following:
"In addition, for evergreen needle-leaved forests, there was a clear seasonal pattern in RS chl_{leaf} and LAI, while QUINCY LAI and chl_{leaf} did not vary much throughout the annual cycle. However, the comparison to *in-situ* chl_{leaf} demonstrated that the RS chl_{leaf} overestimates seasonality of chl_{leaf} for needle-leaved evergreen forests in cold environments, which is likely caused by the RS LAI biases (Heiskanen et al., 2012, Wang et al., 2019) known to happen in these regions. "

7. *Title should be more specific about the type of "remotely sensed data".*

Thank you for the suggestion. We have now changed title from
"Evaluating the carbon and nitrogen cycles of the QUINCY terrestrial biosphere model using remotely-sensed data"
to
"Evaluating the carbon and nitrogen cycles of the QUINCY terrestrial biosphere model using space-born optical remotely-sensed data"

Responses to the comments by Anonymous Referee #1

__1.__ This study is centered on evaluating the spatio-temporal behaviour of the terrestrial biosphere model (TBM) QUINCY, using mainly a remote sensing (RS) product of leaf chlorophyll content (RS chl_{leaf}). The interest in the use of such a product is motivated by the strong link between leaf chlorophyll and Nitrogen contents. Other datasets are exploited such as RS LAI, and GPP at FLUXNET sites. The study looks at the agreement between simulated and observed chl_{leaf}, evaluating the magnitude and the seasonal cycles across and within plant functional types (PFTs). Alternative algorithms of nitrogen use are also evaluated, and a statistical model is used to look at the main drivers of observed and simulated chl_{leaf}.
The topic is quite innovative. The paper is well structured and clearly written. The methodology is sound, and the results are accurately described. The analysis of the seasonal cycles would gain in clarity from actual figures of the seasonal cycles. To enrich the temporal analysis, the authors could present boxplots of RMSD distribution for each PFT. The Discussion section would benefit from a more clearly defined structure. Even though the model results are not yet fully satisfactory, this paper nonetheless offers a valuable contribution to the field, and paves the way for a larger use of RS chl_{leaf} products among TBMs, both for evaluation and data assimilation purposes.

We thank Anonymous Referee #1 for the encouraging and constructive feedback which certainly helps us to improve the quality of our analysis and representation of the results. We have done our best to address the comments.

We have now added additional figures to show the plant function type (PFT)-averaged seasonal cycles. We have also revised the Discussion section to be more concise and readable.

In addition, we tested two different options for the boxplots that the referee suggested. Figure R1 presents a box plot showing the root mean square errors (RMSE) for leaf chlorophyll (chl_{lea})$_f$ for each PFT. The RMSE values are calculated between QUINCY and remote sensing (RS) chl_{leaf}, using the averaged monthly values over years for each site. The boxplot boxes and whiskers indicate the distribution between sites' RMSE.

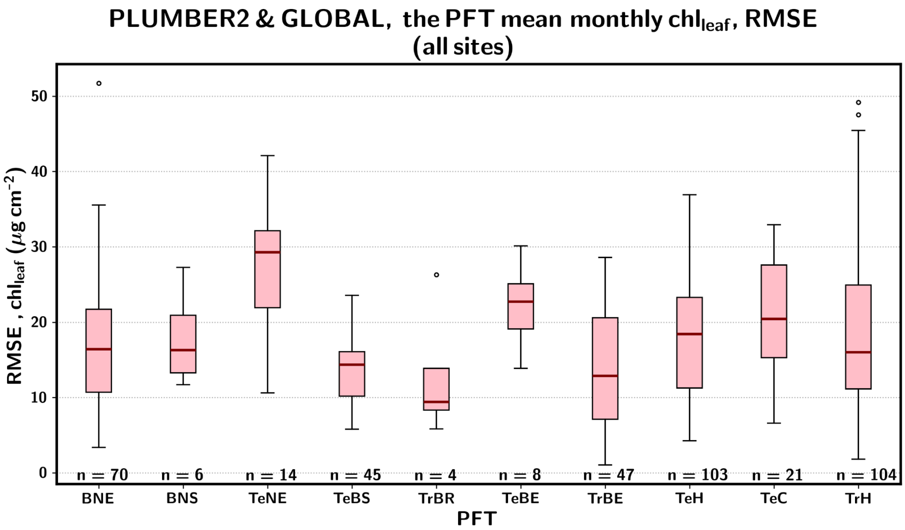

Figure R1: The RMSE values between QUINCY and RS monthly mean chl$_{leaf}$ for the PLUMBER2 and GLOBAL sites.

The lowest RMSE median of monthly chl$_{leaf}$ averages is seen for the TrBR sites, but the median represents only four sites. The highest RMSE median values are seen for TeNE TeBE and TeC sites. For the evergreen needle-leaved sites (TeNE and BNE), the QUINCY chl$_{leaf}$ during winter time is much higher than RS$_{chl}$, as QUINCY is showing a stronger seasonal cycle than RS$_{chl}$, which affects the RMSE values. For the TeC sites, the magnitude of RS chl$_{leaf}$ is much larger than QUINCY, and the seasonal cycle of QUINCY is delayed, which leads to high RMSE values. The TrH sites show a high range of RMSE values, which is partly due to the high number of sites, located in many different regions. However, the median RMSE of TrH is at a similar level as for other PFTs.

Furthermore, we drew a boxplot showing monthly averaged chl$_{leaf}$ values for each PFT separately. The boxplots are presented in Figure R2. The boxplot whiskers and boxes indicate the distribution of site-level mean monthly chl$_{leaf}$ values for QUINCY and RS.

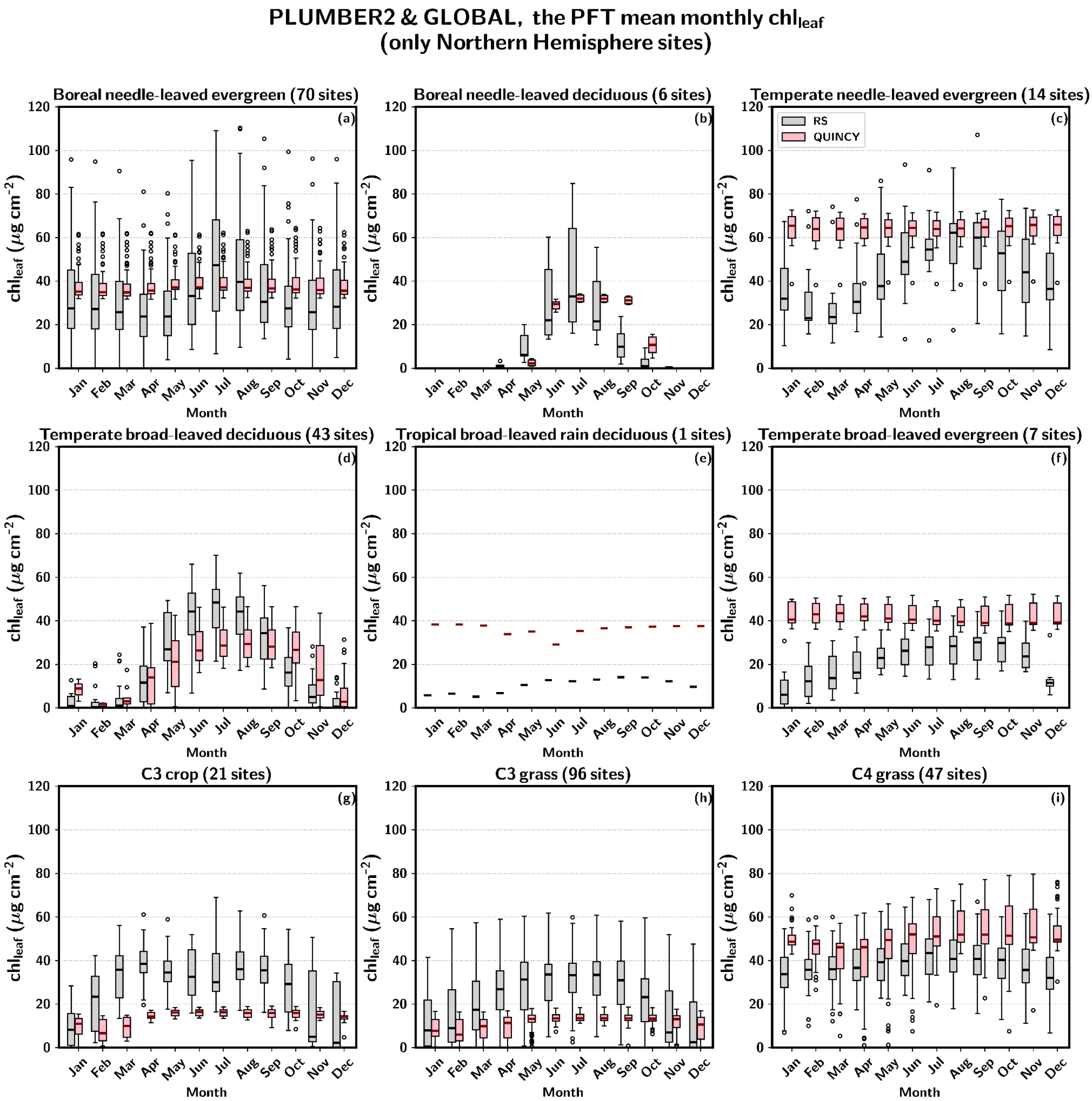

Figure R2: The monthly mean chl_{leaf} for QUINCY and RS for the Northern Hemisphere PLUMBER2 and GLOBAL sites.

The boxplots of the monthly mean chl_{leaf} values show that there is a larger deviation between sites in RS chl_{leaf} compared to QUINCY chl_{leaf}. Especially needle-leaved sites show a larger spread in monthly chl_{leaf} values in RS than in QUINCY.

After consideration, we decided that we will present only the PFT-averaged seasonal cycles with the standard deviation shown in the manuscript, including the RMSE values, as they represent very similar information content compared to these plots.

Main comments

Abstract.

__2.__ L14-16: 'Our results also show that compared to the original leaf nitrogen allocation scheme of QUINCY, the revised scheme produced a more reasonable sensitivity of gross primary production to increases in chl_{leaf} '-> Where is this demonstrated in your results?

We referred here to the analysis and results presented at L515-526. To better communicate our conclusion, we have modified and improved the manuscript text in Section 3.3 and described this in more detail. We have also updated the manuscript text in Discussion to clarify this aspect (See detailed response below in reply to comment 26.)

1 Introduction

__3.__ L54: Krinner et al., 2005 -> The N version of ORCHIDEE is described in Vuichard et al. (2019).

Vuichard, N., Messina, P., Luyssaert, S., Guenet, B., Zaehle, S., Ghattas, J., ... & Peylin, P. (2019). Accounting for carbon and nitrogen interactions in the global terrestrial ecosystem model ORCHIDEE (trunk version, rev 4999): Multi-scale evaluation of gross primary production. Geoscientific Model Development, 12(11), 4751-4779.

Many thanks for pointing out the correct citation. We have fixed this in the updated manuscript.

__4.__ L55-56: 'evaluating and validating' -> What's the difference for you?

Depending on the scientific discipline, evaluating and validating can have different meanings. As our study focuses on evaluating a TBM, we considered it best to use only the term 'evaluate'.
We have now updated the sentence at L57-58 from:
"..remote sensing (RS) of the Earth's vegetation provides comprehensive data for evaluating and validating TBMs." to
"..remote sensing (RS) of the Earth's vegetation provides comprehensive data for evaluating TBMs."

2 Materials and methods

2.4 Remote sensing data

2.4.3 Post-processing of the RS data

__5.__ L241-243: 'In addition, the RS chl_{leaf} for the needle-leaved sites was multiplied by π/2. This was done to account for the half-hemispherical needle geometry in the remote sensing retrieval (Stenberg et al., 1995).' -> This is weird, why was this correction not applied in the native algorithm of Croft et al. (2020)? How can the users of chlorophyll products know whether this has been considered? For example, what about the OLCI product, do you know if such a correction is integrated in the processing chain?

In the Croft *et al.* (2020) RS chl_{leaf} processing chain, the leaf-level reflectance spectra is derived with the 4-Scale model, where needles are described as having a cylindrical shape.

The output is therefore expressed in terms of chlorophyll content per half the total surface area. The geometry correction, i.e. multiplying by π/2, is done in order to have the RS chl_{leaf} data corresponding to the projected area of needles. This way, RS chl_{leaf} is comparable to QUINCY chl_{leaf}. There are multiple options for geometry correction available, and we decided to go with the option recommended by Stenberg et al.

OLCI chl_{leaf} product by Reyes-Muñoz et al. (2022) is derived using the SCOPE radiative transfer model, where the needle-leaved trees are treated in a similar manner as other leaf types. Therefore, RS chl_{leaf} for conifer trees already corresponds to the projected needle area, and does not require a correction.

2.5 In-situ observations

2.5.1 Eddy covariance flux observations

6. *L256: 'Data from all years were used, and therefore, the GPP time series are not from the same time interval as RS chl_{leaf}.' -> Can you comment on this discrepancy? What are the possible implications regarding the accuracy and robustness of your findings?*

Our aim was to simulate and evaluate standard, average behaviour of each PFT, and also conduct analysis over some specific PLUMBER2 sites. Therefore we aimed to use as much data as there was available. Taking the average over all available data years enables us to compare the average seasonal cycle. If we'd have used only the years for which we would have both RS chl_{leaf} and eddy covariance GPP data, the analysis of the seasonal cycle might not have been representative for each site due to lack of data.
The discrepancy in time intervals between RS chl_{leaf} and eddy covariance GPP observations might cause differences in the seasonal cycle development for individual sites, for instance, due to extreme drought years. In addition, for some of the sites, the eddy covariance GPP time series are for a long period, and in these cases climate change might play a role. However, as most of the analysis is carried out over several sites and years, the impact of single drought years is likely to be minimized. The RS chl_{leaf} shows stronger interannual variability than QUINCY chl_{leaf} when analyzed over all sites (See manuscript Section 3.1.1). This indicates that the RS chl_{leaf} is potentially more sensitive to the choice of years compared to QUINCY simulated data.
For the GLOBAL sites, the QUINCY simulations were for a 30 year period, while the RS chl_{leaf} data was available only for nine years. To maintain comparability, for each GLOBAL site we filtered out QUINCY simulation data for those years where there were no RS chl_{leaf} data available. This was not described clearly enough in the original manuscript text, and we have modified the text to highlight this data processing step.

We have added to Section 2.6 (previously Section 2.7) at L317-319 the following:
"For the PLUMBER2 sites, we used all the available years from the QUINCY simulations, as well as from and RS and eddy covariance observations. For the GLOBAL sites, we used QUINCY simulation data for the years in which RS chl_{leaf} data was available for each site."

In addition, we have modified the sentence at L159-160 from:
"Data from all years were used, and therefore, the GPP time series are not from the same time interval as RS chl_{leaf}."

to
“Data from all years were used, and therefore, the GPP time series are not necessarily from the same time interval as RS chl_{leaf}.”

2.6 Feature importance analysis

7. *In this section we need more information on your training/test/evaluation datasets and the scores of your statistical models to trust your further analyses.*

We have updated the manuscript text to better describe the machine learning evaluation. In addition, we have added a supplementary table S6 to present the R^2 scores of each PFT-specific random forest model.
We have modified the text at L307-311 from:
“After training the RF models, we calculated the corresponding permutation feature importance values for each model.”
to
“First, we tested the performance of RF models by splitting the data using the *train_test_split* function in SciKit Learn. We used 75 % of the data for preliminary training and 25% for preliminary testing. The coefficient of determination (R^2) scores for the preliminary training and preliminary testing phases are reported in Table S6. Next, we used all the data (i.e., the preliminary training and preliminary testing data) for the final training of the models.

After the final RF model training, we calculated the corresponding permutation feature importance values for each model.”

2.7 Data-analysis

8. *L326-327: ‘We used the 90th percentile of LAI instead of the mean values to reduce the effect of seasonal variation.’ -> This is not clear, how is the mean value more impacted by the seasonal variation? What would be the impact on your study if you considered the mean LAI?*

The motivation to choose 90th percentile instead of mean values is due to the fact that QUINCY and RS LAI show different seasonal behaviour (such as differing growing season length or overestimation of seasonal amplitude in evergreen needleleaves) for some of the PFTs. In this part of the evaluation, our aim was to compare the magnitude of the growing season LAI between QUINCY and RS. The seasonal cycle will be analyzed separately, and here we wanted to focus on PFT-specific LAI magnitudes. As there are sites from both hemispheres (NH and SH), the growing season cannot be narrowed to a certain period (JJA, for instance). We chose to use the 90th percentile instead of the maximum values, as the 90th percentile is less sensitive to outliers and anomalies, which are often present in the RS data.

Figure R3 shows the mean LAI and 90th percentile LAI for the PLUMBER2 sites:

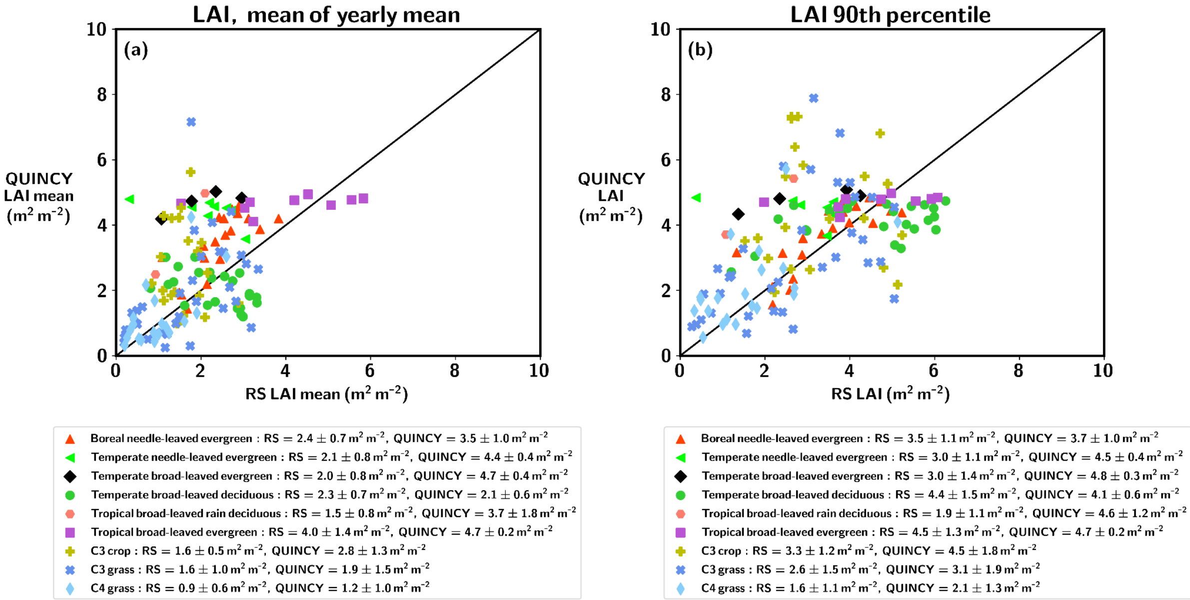

Figure R3: (a) The mean LAI and (b) the 90th percentile LAI for the PLUMBER2 sites

For instance, the PLUMBER2 temperate needle-leaved evergreen (TeNE) sites show a larger difference between QUINCY and RS LAI if we would use yearly average as a metric, compared to the 90th percentile. Using the averages as metric would therefore lead to a conclusion that QUINCY LAI is greatly overestimated (>100%) for TeNE sites, while using the 90th percentile, the overestimation of QUINCY is moderate (~50%).

Figure R4 shows the PFT-averaged seasonal cycles of the PLUMBER2 TeNe sites for QUINCY and RS LAI:

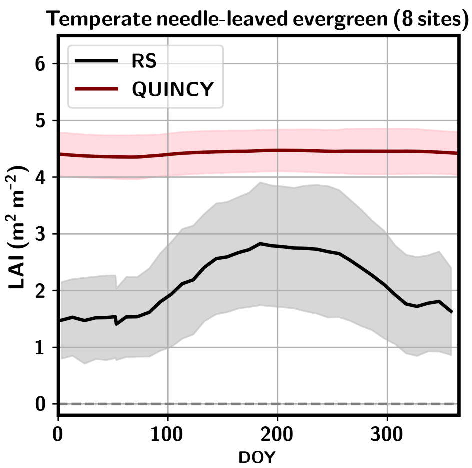

Figure R4: The PFT-averaged seasonal cycle for the PLUMBER2 TeNE sites for QUINCY and RS LAI.

As the figure above shows, QUINCY shows very little variation throughout the year for the TeNE sites, while RS shows a distinct seasonal pattern. The difference in seasonal pattern leads to larger differences in the mean values, while the 90th percentile values are closer to each other compared to the means. However, for many of the PFTs, the effect was not as pronounced. In addition, for some of the PFTs (TeH, TrH, for instance), the QUINCY and RS mean LAIs are closer to each other compared to 90th percentile values.

To clarify this, we have modified the text in L323-325 from:
"We used the 90th percentile of LAI instead of the mean values to reduce the effect of seasonal variation." to
"We used the 90th percentile of LAI instead of the mean values. This was done to reduce the effect of differences in seasonal amplitude and timing variation between QUINCY and RS and to focus on LAI values during the growing season."

__9.__ L330 : '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)' -> Why not show mean seasonal cycles per PFT? This would be justified as many parameters are PFT-dependent.

Thank you for the suggestion. We have now replaced the analysis of the seasonal cycle focusing only on Hainich with an analysis over the PFT-mean seasonal cycle for the temperate broad-leaved deciduous sites of the PLUMBER2 site set in Section 3.1.2. In addition, we have added the estimations of start of season (SOS), end of season (EOS) and length of season (LOS) for these sites. The SOS and EOS values are calculated using a similar approach as in Thum et al. (2025), and we have added the description of the calculation in the Methods section.

__10.__ L334-336: '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'. -> Again, why not study the complete seasonal cycle?

As mentioned above, we have updated the manuscript analysis to cover the PFT-mean seasonal cycles.

3 Results

3.1 Evaluation of simulated chl_{leaf}, LAI and GPP against observations

__11.__ These first paragraphs should be grouped under a first subsection '3.1.1 Mean values' (as opposed to the following subsection dealing with seasonal cycles).

This was a good point, we have added a subsection title for these paragraphs. We decided to use a subtitle of "Yearly values" to avoid confusion with the 90th percentile of LAI and the annual GPP sum.

__12.__ L353: 'For chl_{leaf} in all cases apart from TrBE and TrH, there is a lack of variation in the QUINCY chl_{leaf}' -> Looking at Table S5, TeBS seems to be quite good with QUINCY's statistics being close to those of the RS product.

Again a good observation, we have added TeBS in the list. We updated the sentence at L366 from:

"For chl_{leaf} in all cases apart from TrBE and TrH, …"
to
"For chl_{leaf} in all cases apart from TeBS, TrBE and TrH, …"

3.1.1 Seasonal cycle

13. *The whole description of Figure 3 is too vague, please provide some quantification to support your assertions.*

We have now replaced the analysis of Hainich with assessment over the PFT-mean seasonal cycle for the PLUMBER2 Northern hemisphere TeBS sites as suggested earlier in the comments, and added the Pearson correlation and RMSE statistics. Furthermore, we calculated the start of season (SOS), the end of season (EOS) values and length of the season (LOS) values for chl_{leaf} , LAI and GPP. Those are shown in a new supplementary table (S9).

14. *L383-384: 'The annual cycle of chl_{leaf} at the Hainich site (Fig. 3) is very similar when comparing QUINCY and RS' -> This seems somewhat optimistic, even accounting for the delay, as the shapes are different. Please provide some statistics.*

We have now added the Pearson correlation r and RMSE values for the PFT-mean cycle of PLUMBER2 TeBS NH sites.

15. *L384-385: 'the simulated LAI increases approximately 20 days later in spring compared to the RS LAI. The delay is even more pronounced for chl_{leaf}' -> This is not what is seen on the subplots of Figure 3: RS chl_{leaf} and LAI seem to be starting around DOY 80, and QUINCY chl_{leaf} and LAI around 120, and we don't see any delay regarding chl_{leaf} as compared to LAI, the delay vs the RS products seem to be around 40 days for both variables. To settle this, explain how you determine the start of season for model and observations, provide the dates, and add the corresponding lines on the figure.*

Thank you for pointing this discrepancy out. We have now added the calculation of SOS and EOS values using the PFT-averaged seasonal cycle of the PLUMBER2 NH TeBS sites. From this analysis, we see that the start of the season for TeBS-averaged cycle is during the same DOY for both simulated and observed chl_{leaf}, while the SOS for LAI shows a difference of 13 days. We have now added the SOS and EOS lines in the updated figure, and added a supplementary table for the SOS and EOS values for QUINCY and observations.

16. *L388-390: 'However, despite the fact that QUINCY chl_{leaf} and LAI remain higher, their winter level is reached almost at the same time as in the Hainich observations, because the senescence occurs more rapidly in QUINCY than in the observations' -> That might be overstated, explain how you determine the winter levels for model and observations, provide them and the dates when they are reached, add all the corresponding horizontal and vertical lines on the figure.*

This part of the text is now modified to correspond to the PFT-mean seasonal cycle. In addition, we added the calculation of EOS values as mentioned in the previous answer. We modified the text in L402 from:

“However, despite the fact that QUINCY chl_{leaf} and LAI remain higher, their winter level is reached almost at the same time as the observed, because the senescence occurs more rapidly in QUINCY than in the observations.”
to
“However, senescence occurs more rapidly in QUINCY than in the observations.“

17. *L390: ‘Therefore, the overestimation in GPP is not as pronounced.’ -> Why “therefore”? I don’t see the link. Your later sentence on L395-396: ‘In addition, although the simulated LAI remains at the summer level until DOY ~280, the simulated GPP decreases due to the environmental conditions’ is more convincing.*

The referee is correct that the reasoning was not logical in this section. What we meant was that as GPP depends on the LAI in QUINCY, the delayed end of the season in the simulated GPP is partly minimized by the sharp decline in the LAI. In order to avoid confusion, we have removed from L402 the sentence:
“Therefore, the overestimation in GPP is not as pronounced.”

18. *L397-398: ‘For the TrH sites (Fig. 2i), the lowest PFT mean for QUINCY is in April, suggesting that the phenological cycle for these sites needs further tuning in QUINCY’ -> It’s not clear from the scatter plot what the seasonal cycles look like. It would be much easier to see the mean seasonal cycles for both model and observations than trying to imagine them from the scatter plot. Please add them, at least in supplementary, otherwise it’s very difficult to follow this paragraph. Also, you could add the corresponding RMSD and r.*

We have now replaced Figure 2 with three new supplementary figures (S6, S7 & S8), showing the PFT-averaged seasonal cycles of chl_{leaf}, LAI and GPP. The figures include the RMSE and Pearson correlation values, calculated between PFT-averaged seasonal cycles of QUINCY and observations.

19. *L402-403: ‘could be due to a drought’ -> Do you mean a modeled water stress? Please check whether this is the case in your simulations. Plus, wouldn’t that be the standard phenology for a tropical deciduous PFT, to be limited by water availability rather than temperature?*

This was an important point. We have now analyzed the simulated soil moisture levels for NH TrH sites. We observed that the soil moisture at the 1st and 2nd QUINCY soil layers were in the same magnitude when comparing winter (DJF) and spring (MAM) averages over sites. Therefore, we decided to remove the speculation of the role of potential drought affect chl_{leaf} magnitude.

The potential cause of the low spring chl_{leaf} magnitude of the TrH sites could be due to delay in autumn LAI, as the senescence occurs rather late in QUINCY for the NH TrH sites.
In QUINCY, the growing season of TrH sites is determined using three threshold values (soil moisture, air temperature and whether the weekly carbon balance is positive).
Phenological triggers in herbaceous systems are more varied than in forests, due to higher species diversity, so models such as QUINCY often struggle to capture start or end of season. For seasonally dry sites it is also possible that the simulated soil moisture does not match the PFT-level threshold parameters.

We have updated the manuscript analysis and added some discussion points regarding the delay in the Northern Hemisphere TrH sites seasonal cycle. We have moved this part of the text from Results to Discussion.

We have moved the text at L425 to the Discussion section, and modified from:
" This suggests that the lower QUINCY spring chl_{leaf} compared to the rest of the year for TrH sites could be due to a drought which reduces LAI."
to
"The lower QUINCY spring chl_{leaf} for NH TrH sites is most likely linked to simulated LAI biases. In QUINCY, the start of senescence is controlled by soil moisture and temperature thresholds. Given the high species diversity in herbaceous systems, both within and between sites, ecosystem-level models such as QUINCY often struggle to capture phenological variation. This is partially due to PFT-level parameters not reflecting diversity at the site level, and partially due to the difficulty of capturing an average response of diverse species."

20. *L408: 'The mean IAV' -> Why do you present the mean IAV in the "Seasonal cycle" section, and not in the former one which presents the mean annual values?*

This was a good suggestion, we have now moved the text from L425 after L390.

3.2 Nitrogen limitations in QUINCY

21. *L455: 'indicating that chl_{leaf} is more influenced by other factors than leaf N levels, compared to BNE and TeBS' -> Provide a few examples of these other potential factors.*

We noticed that there was a small typing error: the purpose was to discuss leaf C:N levels as a factor, not leaf N. Examples of potential factors are temperature and precipitation, which affect both the carbon and nitrogen cycles.

We have updated the sentence at L468-469 from:
"indicating that chl_{leaf} is more influenced by other factors than leaf N levels,"
to
"indicating that chl_{leaf} is more influenced by other factors, such as water availability, temperature and precipitation, than leaf C:N levels,"

22. *L478:479: 'The US-MMS QUINCY leaf C:N is close to the lower leaf C:N threshold '-> You could provide the leaf C:N low and high thresholds per PFT in the supplementary material.*

Thanks for the suggestion, we have added a new supplementary table (S4) where the QUINCY lower and upper leaf C:N boundaries are listed. In addition, we have modified the sentence in L198-199 from:
"The ratios are constrained to an empirically derived range based on the TRY database (Kattge et al., 2011)."
to

"The ratios are constrained to an empirically derived range based on the TRY database (Kattge et al., 2011), and the lower and upper boundaries are presented in Table S4."

4 Discussion

4.1 QUINCY's ability to reproduce chl_{leaf} magnitude

23. *This section spans over two pages and lacks clear organization. Please add subsection titles to better guide readers through the logic of your argument.*

We have rearranged the Discussion section based on the comments by both Referee #1 and Referee #2, and added more subsection titles as suggested.

24. *L569-570: 'Overestimation of LAI can lead to too strong shading, which can result in too small GPP in lower canopy layers' -> I'm not sure about that, it will depend on the radiative transfer model.*

We have now updated wording at L614-615 to be more speculative. The sentence is modified from:
"Overestimation of LAI can lead to too strong shading, which can result in too small GPP in lower canopy layers."
to
"Overestimation of LAI can potentially lead to too strong shading, which could result in reduced GPP in lower canopy layers."

25. *L570-571: 'In addition, the radiative transfer model might play a role in the underestimated GPP' -> Why 'in addition', as this has a direct link to your former sentence?*

Thanks for pointing this out, we have removed "in addition" from the sentence. We have modified the sentence at L615-616 from:
"In addition, the radiative transfer model might play a role in the underestimated GPP."
to
"The radiative transfer model might therefore play a role in the underestimated GPP."

26. *L596-598: 'This indicates that the alternative N allocation scheme produces more in line with our current ecophysiological understanding of plant dynamics: increasing leaf N in chl_{leaf} does not decrease other photosynthetic fractions , but more structural part ($f_{N,struct}$)'*
-> I'm not sure what 'this' refers to. While you argue that your new model is more realistic, it is unclear how this is supported by your results.

Here we had "this" referring to emerging understanding from the current literature, and also to the increase of GPP due to changes in $f_{N,chl}$ and $f_{N,Rub}$. We have clarified the manuscript text in Section 3.3, and also the text in the Discussion section.

We have added after L522:
"In the QUINCY default simulation, increasing $f_{N,chl}$ resulted in decreased GPP, while in the alternative N allocation scheme, GPP increased. Furthermore, the fraction in the structural part $f_{N,struct}$ decreases in the alternative N allocation scheme when the $f_{N,Rubl}$ and,

consequently, $f_{N,chl}$ are increased. In the default QUINCY simulation, increasing $f_{N,chl}$ does not directly affect $f_{N,struct}$, but rather indirectly through its influence on leaf N, resulting in only a minor decrease of $f_{N,struct}$."

We have modified the text at L700-704 from:
"the advanced N allocation scheme provided a more realistic mechanism when $f_{N,chl}$ was increased. This indicates that the alternative N allocation scheme produces is more in line with our current ecophysiological understanding of plant dynamics: increasing leaf N in chl_{leaf} does not decrease other photosynthetic fractions, but more the structural part ($f_{N,struct}$)."
to
"the advanced N allocation scheme provided a more realistic mechanism when $f_{N,Rub}$ was increased by resulting in increases in both $f_{N,chl}$ and in GPP. This indicates that what the alternative N allocation scheme produces is more in line with the current ecophysiological understanding from the literature (Onoda et al., 2017; Evans and Clarke, 2019) regarding the relationship between $V_{c(max)}$ and chl_{leaf}: increasing leaf N in chl_{leaf} does not decrease other photosynthetic fractions, but rather the structural part ($f_{N,struct}$)."

27. *L600-601: 'QUINCY chl_{leaf} for evergreen sites was driven by N deposition, with other environmental variables contributing less. The same was true for the RS chl_{leaf} for BNE and TrBR, but not for TeNE' -> Why is TrBR mentioned here, it's not an evergreen PFT. Do you mean TrBE?*

Many thanks for pointing this out, indeed we were referring to TrBE. This is now corrected in the text.

Code and data availability

28. *L774: 'RS chl_{leaf} by Croft et al. (2020) will be available by request from the authors.' -> Why not make it available on a public repository? This would ensure broader use.*

The RS chl_{leaf} product data has limited access due to intellectual property rights. However, we have now made the postprocessed RS chl_{leaf} data for the PLUMBER2 and GLOBAL sites to be publicly available via the B2Share repository of FMI.

Technical comments

29. *L77-78: Evans and Clarke, 2018 -> Evans and Clarke, 2019. To be corrected also L79, 155, 862.*

This is now fixed accordingly.

30. *L155: 'Site description -> Description of the sites'*

A good suggestion, this has been updated.

31. *L355: 'for TeC and TeH sites, which gives' -> 'for TeC and TeH sites, which give'*

Thanks for pointing this out, it is now corrected.

32. *L359: ‘PFTs.Whilst’ -> ‘PFTs. Whilst’*

The typing error is now fixed.

33. *L378-379: ‘for the boreal and temperate evergreen sites (Fig. 2a,b,c,d)’ -> That would rather be ‘Fig2. a, c, f’.*

Thanks for pointing this out, we have now corrected this when referring to the new supplementary figures.

34. *Figure 2e: ‘TrBR (1 sites)’ -> ‘TrBR (1 site)’*

Figure 2 has been replaced with new supplementary figures, and we have taken this comment into account while formatting the figures.

35. *L395: ‘between years the 2003–2011’ -> ‘between years 2003–2011’*

This correction was not needed as the text was updated to cover all PLUMBER2 TeBS sites.

36. *L401: ‘compared rest of the year’ -> ‘compared to the rest of the year’*

This part of the text was removed due to updates in the manuscript analysis.

37. *L405: ‘The April and May chl_{leaf} values are mostly underestimated by QUINCY for chl_{leaf} , LAI and GPP’ -> ‘The April and May values are mostly underestimated by QUINCY for chlleaf, LAI and GPP’*

This part of the text was removed due to updates in the manuscript analysis.

38. *L435: ‘Fig. S7 and S6’ -> ‘Fig. S7 and Table S6’*

Thank you for indicating this, we have corrected this in the text.

39. *L454: ‘p< 1×10−40’ -> ‘p < 1×10−40’*

Added the missing white space.

40. *L458: ‘For the TeBS site’-> ‘For the TeBS sites’*

Fixed the sentence accordingly.

41. *Legend Figure5: ‘Teh’ -> ‘TeH'*

We have corrected the PFT abbreviation in the figure caption.

42. *L486: ‘3.3 Alternative leaf N allocation scheme’ -> ‘3.3 Alternative leaf N allocation schemes’*

We decided to change the subsection 3.3 title from
“Alternative leaf N allocation scheme”
to
“Leaf N allocation schemes”

43. *L496: ‘PFTS’ -> ‘PFTs’*

This is now revised in the manuscript text.

44. *L567: ‘the QUINCY mean chl_{leaf} is underestimated at majority of the the TeBS sites’ -> ‘the QUINCY mean chl_{leaf} is underestimated at the majority of the TeBS sites’*

A good remark, we have fixed the text accordingly.

45. *L579-580: ‘Another missing processes in are fertilization and management of croplands’ ->’ Other missing processes in QUINCY are fertilization and management of croplands’*

Corrected the sentence in the text.

46. *L596-598: ‘This indicates that the alternative N allocation scheme produces more in line with our current ecophysiological understanding of plant dynamics: increasing leaf N in chl_{leaf} does not decrease other photosynthetic fractions , but more structural part (fN,struct)’ -> ‘This indicates that what the alternative N allocation scheme produces is more in line with our current ecophysiological understanding of plant dynamics: increasing leaf N in chl_{leaf} does not decrease other photosynthetic fractions, but rather the structural part (fN,struct)’*

Many thanks for the suggestion, the manuscript text is now updated accordingly, with slight modifications based on comments earlier.

47. *L599: ‘Our machine learning based analysis’ -> ‘Our machine learning-based analysis’*

Fixed the typo in the text.

48. *L626-627: ‘The Sentinel-3 chl_{leaf} shows the strongest seasonal cycle for the US-NR1 compared to other products’ -> ‘The Sentinel-3 chl_{leaf} shows the strongest seasonal cycle at the US-NR1 site compared to other products’*

This is now revised in the manuscript text.

49. *L633: ‘one of the state-of-the art TBMs that includes’ -> ‘one of the state-of-the art TBMs that include’*

Corrected the sentence.

50. *L653: 'overestimated at the certain sites' -> 'overestimated at certain sites'*

We have fixed this accordingly.

51. *L679-680: 'RS observations from the Sentinel-3 satellite could be included as it was tested for two sites' -> 'RS observations from the Sentinel-3 satellite could be included as they were tested for two sites'*

This is now corrected in the updated manuscript.

52. *L692-693: 'the low elevation angles of the sun, which limits the reliability of the measurements throughout the winter months and, in mid-winter, results in polar night' -> 'the low elevation angles of the sun, which limit the reliability of the measurements throughout the winter months and, in mid-winter, result in polar night'*

Thank you for pointing this out, we have corrected the sentence text.

53. *L708: 'Similarly, RS inversion algorithm does not consider' -> 'Similarly, RS inversion algorithms do not consider'*

Updated the manuscript text accordingly.

54. *L710-711: 'In addition, PFT can be a very broad category' -> 'In addition, a PFT can be a very broad category'*

This has been corrected in the text.

55. *L711-712: 'different tree species may have different characteristics, which is taken into account in our PFT-based modeling scheme and parameterization' -> 'different tree species may have different characteristics, which is not taken into account in our PFT-based modeling scheme and parameterization*

An excellent remark, the sentence is now corrected.

56, *L738-739: 'However, this would be possible only if other TBMs to provide' -> 'However, this would be possible only if other TBMs were to provide'*

We have implemented this suggestion in the manuscript text.

57. *L743: 'near-time' -> Do you mean 'near-real-time'?*

Indeed, this was our intention. Fixed the sentence.

58. *L768: 'las access' -> 'last access'*

Thanks, this is now corrected.

Supplementary material

<u>59.</u> *‘Table S2. Lanc cover’ -> ‘Table S2. Land cover’*

Fixed this in the caption text.

<u>60.</u> *Table S5.* chl_{leaf} *(μg cm−2 ->* chl_{leaf} *(μg cm−2)*

We have added the missing parentheses.

<u>61.</u> *Table S7: ("Qdef."-> ("Qdef.")*

Similarly, we have added here the missing parentheses.

<u>62.</u> *Legend Figure S2: ‘Subplot (h) has a different scale on the x- and y-axis than the other subplots’ -> Why h and not b or e, where values also are not higher than 60 mg cm-2?*
This was a good point. We have adjusted the subplot scaling, and updated the tick label coloring to have pink color indicating higher ranges. We have modified the caption in Figure S2 from:
"Subplot **(h)** has a different scale on the x- and y-axis than the other subplots." to
"Subplots **(a)**, **(c)**, **(g)** and **(i)** have a different scale on the x- and y-axis than the other subplots."

Responses to the comments by Anonymous Referee #2

__1.__ 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 have modified and streamlined the M&M section to give a more clear description of the harnessed data and methods as well as added a table describing all the different data sets used in this study. In addition, we have also moved some details of site measurements at FI-Sod to the supplement. We have also re-organized and improved text in the Discussion section to improve the readability, and added more subtitles for clarity.

Detailed comments:

Introduction:

__2.__ 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 text at L32 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 regarding plant productivity (Medlyn et al., 2015)."

__3.__ 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 a new parameter.

We have modified the text at L35-39 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 modelled by the current generation of TBMs (Kou-Giesbrecht et al., 2023)."

__4.__ 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 L78 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

__5.__ 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 have modified and streamlined Section 2.4 (previously 2.1) text describing QUINCY model processes to better serve the manuscript needs.

We have updated the text at L187-196 to:
"Plants in QUINCY respond to soil N availability. This includes a response in leaf N content, which decreases if there is not enough N is available. Leaf nitrogen is divided into structural and photosynthetically active components. The photosynthesis scheme explicitly accounts for the role of chl_{leaf}. Photosynthesis is calculated using the Kull and Kruijt (1998) model. According to this model, in the light-saturated part of the leaf, photosynthesis is the minimum of electron transport rate-limited photosynthesis (determined by the maximum electron transport rate parameter $J_{max,25}$) and the carboxylation capacity-limited photosynthesis (determined by the maximum carboxylation capacity parameter $V_{c(max),25}$). In the non-light-saturated part, photosynthesis is determined by the electron transport-rate-limited

photosynthesis. Chlorophyll partly determines the depth of the light-saturated layer in the leaf. Thus, all the three photosynthetically active components of leaf nitrogen influence the photosynthesis calculation in QUINCY, as described by Friend et al. (2009), Zaehle and Friend (2010), and Thum et al. (2019). The photosynthesis model by Kull and Kruijt (1998) is extended to cover C4 plants (Friend et al., 2009)."

6. *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 L202-204 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 plant's ability to grow new tissues, given the availability of C and N, as well as the fractional allocation to plant organs. This fractional allocation is constrained by allometric relationships and the availability of nutrients and water.

Also, we modified the text at L205-206 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."

7. *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 at L99-101 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."

8. *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} post processing 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 2.2.3 (previously Section 2.4.3).
We have reorganized the Materials and methods section, and add after L103 a text:
"(See Section 2.2.3)."

9. *L177: Input is in which period?*

We have modified the text at L255-257 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). Depending on the PLUMBER2 site, meteorological data was available from 1992 to 2018 (Ukkola et al., (2022), Table S1). For the GLOBAL sites, the meteorological data were obtained from the CRU JRA dataset, and covered the years 1989–2018."

10. *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 have a new Supplementary table (S1) summarizing the data utilized in the study, following the suggestion given by the referee.

11. *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 L263-265 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 used by repeating the values from the period between 190 and 1930. Meteorological data were taken from a random year of observed meteorological data. "

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

We have modified the sentence at 265-268 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 observed meteorological data were available. For the PLUMBER2 sites, the start of the period was site-dependent, while for the GLOBAL sites, the meteorological data began in 1989."

13. *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 L162-163 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.

14. *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 have re-organized Section 2 as suggested.

15. *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 L349 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 simulations, 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, we analyzed the seasonal cycles of LAI, fAPAR and GPP for the FI-Sod site and compared the 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 L328-342 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 (NH) sites separately for different PFTs. In addition, the analysis of the PLUMBER2 sites included GPP. First, we calculated the averaged seasonal cycle over years for each site and variable. Then, using these averaged seasonal cycles, we calculated the mean seasonal cycle per PFT across sites and the standard deviation between sites for each day of year (DOY). This was done for QUINCY simulated values and for RS and eddy covariance observations. Using the PFT-averaged seasonal cycles, we calculated the Pearson correlation (r) and root mean squared error (RMSE) between QUINCY and the observations.

For the NH PLUMBER2 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 by 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 at 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. Winter values were calculated using the mean values from January and February, and summer values were calculated using the mean values from June and July. 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 the difference between EOS and SOS.

Results

16. *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 in Figure RR1, 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 beige lines.

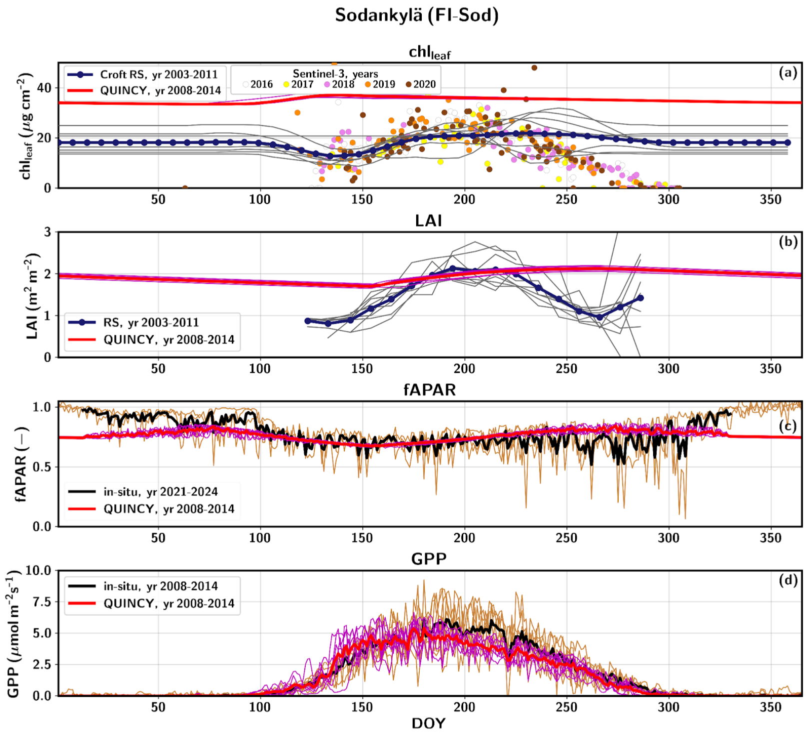

Figure RR1: Seasonal cycle of daily means for FI-Sod(a) chl_{leaf}, (b) LAI, (c) fAPAR and (d) GPP for QUINCY, remote sensing (RS) and in-situ observations.

As Figure RR1 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. The high variability of RS chl_{leaf}

made using individual years in the analysis too complex and therefore, we wanted to use averages over several years, to ensure realistic behaviour of the RS variables. In addition, the LAI cycles for one year showed somewhat abrupt behaviour, and this was also favoring use of averages over the years.

Discussion

17. *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 have restructured the Discussion section and added subtitles to improve the readability, and have done our best to take into account all the points raised by the Referee here.

18. *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.

19. *L627: Which assumptions?*

We have added the following sentence after L640:
"For instance, the assumptions made for the LAI seasonality and the effect of snow cover can affect the RS chl_{leaf} retrieval."

20. *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 L680 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:

21. *L496: PFTs*

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

22. *L505: due to*

This is now corrected.

References:

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