Borden paper review: Reply to Reviewer #2

(The comments by the reviewer are in magenta, the replies to the comments are in black, new text added or modified in the manuscript is written in italics.)

General comments

The study of Thum et al. investigates carbon and nitrogen interactions in the Ontario's Borden Forest Research Station, using in-situ measurements to parameterize the QUINCY model. It evaluates carbon flux simulations over 22 years, finding good alignment in some metrics like GPP but identifying key discrepancies in ecosystem respiration trends and legacy drought impacts, underscoring the need to improve TBMs.

The manuscript addresses an important topic: the representation of carbon and nitrogen interactions in TBM models. Overall, it is well-written, and the figures—despite a few editorial issues—are clear and effectively support the results presented. However, I found the study's objectives difficult to discern from the Abstract. The Introduction also requires substantial revision, as it sometimes lacks logical flow, and the paragraphs don't fully cohere. For instance, the first paragraph focuses on the importance of nitrogen, followed abruptly by a discussion on changes in the growing season due to warming, and then by a mention of the value of long-term observations for capturing anomalies (anomalies of what?). The Introduction feels like a series of loosely connected points without a clear narrative thread, which makes it challenging to understand the paper's aim until the objectives are listed in the final paragraph.

We thank the reviewer for finding the topic important and for all the in-depth comments that we think will improve the quality of the manuscript. We apologize for the shortcomings of the introduction and will improve its flow, following the guidance given by the reviewer.

Regarding the content of the paper, there are two key aspects that should be addressed:

1. Quality of Eddy Covariance Flux Post-Processing:
The study relies heavily on estimates of GPP and TER derived from eddy covariance measurements, which are not directly measured values. It is essential to assess the quality of the partitioning and gap-filling methods used. A study from 2004 is referenced for both flux partitioning and gap filling, yet there is no description of this approach, its advantages, or why it was chosen. Given that GPP estimates are highly sensitive to partitioning methods, it's critical to first establish a solid foundation, showing that the best possible gap-filled and partitioned fluxes were derived before making further interpretations about model structure or other underlying factors.

We thank the reviewer for this insight. We have added an explanation about the method developed by Barr et al. (2004) and would like to mention that it is the standard method for the Fluxnet-Canada sites (Pierrat et al., 2021). We have added this point now to the manuscript. This is also the method that has been used for the site in several earlier publications.

The post-processing of eddy covariance data is a challenging task and many decisions need to be made during it. We find that to test the different partitioning and gap-filling methods would be a study on its own. There are several papers from the site that have more concentrated on the observations and how they have been processed (e.g. Froelich et al., 2015). Our aim with this study was to use those data for model evaluation.

Studies that have compared gap-filling methods, have not found them to introduce large differences (Mahabbati et al., 2021). A study comparing partitioning methods found the influence on the annual balances to be less than 10 % (Desai et al., 2008), modest differences have also been found in other studies (Moffat et al., 2007).

Another difficulty in assessing what would be the best gap-filling and partitioning method is that we don't know what the truth is. Therefore studies using synthetic data are useful, since then the 'truth' is known. We've been involved and shared our model results from other sites to such a study and fully support these investigations (Vekuri et al., 2024). The field of studying partitioning methods is an exciting one with new ecosystem functioning related findings (Wohlfahrt and Galvagno, 2017) and applications of machine learning (Pastorello et al., 2020), but we consider applying these different methods to be outside the scope of our study.

While the gap-filling and partitioning do influence the annual balance values, for our purposes the seasonality of these fluxes was more on focus. If the annual balance values would shift a bit, the conclusions of our study would not change. We have now added to the discussion text on the uncertainty that the gap-filling and partitioning method cause to the measurements:

"*Another source of uncertainty in the observations are the use of gapfilling (Mahabbati et al., 2021) and partitioning methods (Desai et al., 2008), that cause uncertainty in the annual carbon balance estimates.*"

2. Snow Cover Effects:

Considering the site's geographic location, persistent winter snow cover is likely; if this is not the case, it should be explicitly mentioned. However, the manuscript does not address snow cover or its potential impact on carbon flux processes. This is especially relevant in the shoulder seasons, where the authors discuss discrepancies between modeled and observed fluxes. Factors such as snow effects, soil freezing and thawing, and changes in soil-air temperature decoupling due to snowmelt could significantly influence these processes but are omitted in the analysis.

We thank the reviewer for bringing this point up. The original model simulations did not have snow included (this was partly because of 'historical' reasons: QUINCY did not have snow included when this work started). We agree with the reviewer that it's a relevant process at this site and added a simulation with the snow, analyzed the snow depth against site level observations and its influence on the carbon fluxes.

Below, I provide more specific comments on the manuscript.

Specific comments

Line 10: Please also report the RMSE (and not just r2) when reporting the model performance.

This has now been added.

Line 10: You mentioned how you parameterized the model but not how the model was improved.

Thanks for noticing this, the phrasing had been unclear with "*The improved model captured observed daily gross primary production (GPP) well."*

We changed this into:
"*Model with the improved parameterization captured observed daily gross primary production (GPP) well."*

Line 11: Would be interesting to know the magnitude of this increase

Sure, we added the magnitude of the trend here.

Line 11: NEE not defined yet

Thanks, definition added.

Line 35: Grammar ("are" missing)

Thanks, added.

Line 42: Would be important to add here why the current representation of N limitation of photosynthesis is not sufficient (since this is such a key aspect in this paper)

The claim here originally was not that the representation of N limitation is not sufficient, it's that the models have different approaches that lead to different outcomes. We have added some more text here describing these differences:

"*N limitation may directly affect photosynthesis rates or its effects may be buffered via different stoichiometric related implementations (Thomas et al., 2015) and the different hypotheses and parameter values related to N cycle processes lead to differences between models (Medlyn et al., 2015)*."

Line 43: For example, which responses?

We've added here: "*A model intercomparison study of five CMIP6 models showed a wide range of response in net primary productivity for increased atmospheric CO_2 and atmospheric N deposition (Davies-Barnard et al., 2020).*"

These aspects are important to be clarified to convince the readers why "it is paramount that the effects of N constraints on plant productivity are accurately simulated".

We agree with the reviewer and are thankful for these remarks.

Line 50 and 52: Do these observations overlap? Both in terms of time and space? How is the
increase in LAI and the decline in N related?

The study by Chen (2019) uses global remote sensing data starting in 1981 and reaching up to 2016. The responses for central Europe show divergent trends in LAI development (Fig. 1), mostly small or larger increases. Jonard et al. (2015) noticed lowering of leaf N at the European forests during 1992–2009, so the studies do overlap temporally. The Jonard et al. (2015) study also included foliar mass observations and for some tree species significant increasing trends were included. They conclude that increases in tree productivity have led to higher nutrient demand by trees and as soil nutrient supply is not enough to meet this demand, the tree mineral nutrition deteriorates. Mason et al. (2022) have reported a decline in N availability in many terrestrial ecosystems, supporting also conclusions by Jonard et al.

We have added this point to the text by:

"*Climate change induced changes have caused a increases in LAI (Chen et al., 2019) and tree productivity (Jonard et al., 2015), and the changes in availability and demands for N have been leading to declining N availability in respect to demands in terrestrial ecosystems (Mason et al., 2022).*"

Line 79: Grammar

Thank you for noticing this. We have rephrased this sentence as the reviewer #1 also commented on this.

Section 2.1: Please add a description of the understory vegetation (species and cover). This information is relevant to your discussions of model limitations that we read later in the paper.

Sure. Unfortunately the exact species haven't been determined at the site to our knowledge. We added to the section the following text: "*The understory consists of short ferns, small shrubs and saplings (Halliday, 2010).*"

Lines 101-104: Is this the species composition within the flux footprint?

There is cropland in the northwest direction and these data were excluded and gapfilled. This has been explained in Section 2.2.1.

Line 107: Mean over what period?

Mean over 2000-2014, added .

Line 118: Used for what? For calibration of the model? For validation?

The CO_2 fluxes were used for model evaluation. The modified sentence is now: "*CO_2 flux data from half-hourly eddy covariance measurements sampled at Borden Forest tower at 33 m height between 1996 and 2018 were used for model evaluation.*"

Line 126: A brief description of the gapfilling and partitioning method should be given here and the justification why such method is selected over other existing methods.

We have added in here the point that this is the standard method used by Fluxnet-Canada (Pierrat et al., 2021) and have added a further explanation on the method as:

"*The procedure first derives the component fluxes from NEE and then uses simple empirical models constrained by the measured data for one year at a time. The other empirical relationship is between TER and soil temperature at shallow depth and the other is between GPP and photosynthetically active radiation (PAR) above the canopy. Parameters for this empirical relationship are first obtained for the annual analysis and after that one parameter per relationship is allowed to vary over time while other parameters stay constant. These time-varying parameters are determined by a flexible moving window approach*."

According to Barr et al 2004 "seasonal onset, rise and fall of photosynthesis from the FNEP time series based on the parameter Px in the rectangular hyperbolic model "was this the case too in this study?

No, here a bit different formulation for this equation was used. We will add a reference to the PhD thesis of C. Rogers.

Line 128: Grammar

Thanks, we changed the original sentence ("*measured from instruments on the flux tower*") to: "*measured by instruments on the flux tower".*

Line 133: How exactly was this scaling done?

The scaling was very simple. The difference between the annual precipitation from the Egbert weather station and the ERA5-Land product was estimated and this scalar was used to multiply all the values in the ERA5-Land dataset. We will clarify this in the text.

How well do the ERA5-Land precipitation product and measured precipitation at the site compare? Perhaps a comparison can be added to the supplementary.

So, we don't have precipitation measurements exactly from the site, but from the nearby Egbert site. We will add one comparison figure to the supplement, as suggested by the reviewer.

Line 193: Typo

Thanks, corrected 'metres' to 'meters'.

The line is "*The soil profile consists of 15 layers, reaching a depth of 9.5 metres. The depth of each layer layer increases exponentially as it*"

Line 274: Based on what was this level of T selected for the adjustments?

The selection was based on the best match of the simulated LAI to the observed LAI.

The original sentence was: "*To adjust the seasonality of LAI, the parameter controlling leaf senescence (t_{air}^{sen}) was modified from the default value of 8.5 C to 15.0 C.*"

We modified this into: "*The autumn decline of the simulated LAI was adjusted to match the observations by modifying the parameter controlling leaf senescence (t_{air}^{sen}) from the default value of 8.5 C to 15.0 C."*

There is a typo just before the caption of Table 2.

The reviewer might be referring to a "t" that has appeared in the pdf-version of this file. It's not in the original file, but likely a result of the pdf conversion.

Figure S2 has a typo in the legend of panel b (should be LAI not GPP)

Thanks, corrected.

In Figure S2 the panels already show GPP or LAI. I would not repeat this again in the legend. Also, the dashed lines can be removed from the legend (to make it less crowded) and keep their description in the caption.

Thank you for these remarks, they have been taken into account in the modified figure.

Figure S3- 5 the axis label should be "modelled" to be consistent with the x-axis which says "Observations". The figure title is already mentioning which model was used, not needed to mention this in the title and in the axis label.

Sure, that's a valid point, we have made this change.

Figure S3- 5 wouldn't it make more sense to display mean daily temperature as the third value rather than the number of the month? What is the reasoning to use month here? Maybe can briefly be added to the caption.

Also using daily temperature as a color code would be a good idea here. Based on these comments and the ones from reviewer #1, we decided to show the monthly values instead of the daily values, to make the figures easier to interpret and to deliver the message that we're using these to make the point that different parameterizations influence the monthly values.

Figure S3- 5 it is hard to judge quantitatively the comparison of different model performances. Maybe at least the r2 values can be displayed in the panels?

We agree with the reviewer. We have added the r^2-values in the panels, as suggested.

Figure S8 very hard to read the figure. Consider increasing the font please.

Apologies for the unclear plot. We have increased the font and improved its readability by shortening the titles of the subplots.

Line 339-345: Could this inaccuracy in modelling the soil temperature be because of the snow? What is the contribution of snow cover at this site? We see from colder sites that snow has an insulating effect on the soil that decouples its temperature from air temperature. If direct measurements are not available at the site perhaps you could explore available remote sensing products (e.g., MODIS/Terra (MOD10A2) and MODIS/AQUA (MYD10A2) (Hall and Riggs 2021) Snow Cover 8-Day L3 Global 500m SIN Grid, Version 6 dataset, which provides maximum snow cover extent at 8-day temporal resolution and 500m spatial resolution).

We made an additional simulation including snow and also show a comparison of snow depth against observations. Comparison against observations show that indeed the snow has a role in the springtime soil temperatures and that the model simulation is improved in this respect.

We're planning to do a more in-depth study of snow effects in a separate study including several boreal sites and using remote sensing data of snow cover data (Nagler et al., 2022) and freeze-thaw data from SMOS (Rautiainen and Holmberg, 2023), expanding results from Böttcher et al. (in preparation).

Line 460-464: Here the Results are repeated. Instead, there should be a discussion of what underpins the observation that although modelled LAI is overestimated, modelled GPP in summer is underestimated.

Sure, we'll do this.

Section 4.2 I suggest dividing parameters by structural from photosynthetic traits.

Section 4.2 discusses leaf chlorophyll and specific leaf area (SLA). SLA can be considered to be a structural trait, as it influences the LAI. As this section only discusses these two parameters in follow-up paragraphs, it was a bit unclear how to further divide these if we don't aim for very short sections.

Line 491-493: What explains it if the SLA was overestimated but GPP underestimated?

SLA changes the leaf carbon pool to LAI. The simulated LAI for the site is too large, if compared to the continuous observations, but compared to in situ -observations, it is close to the observed values. Even though the LAI is important in calculating the GPP, the figures 1 and 2 make the point that in the C-simulations the GPP increases compared to CN-simulations (25 % larger annual values, Table 3) because of the high leaf N content, not only because of the LAI (10 % larger, Table 4).

The underestimation of GPP is not pronounced in the early years of the time period, but becomes more pronounced in the later years, contributing to the 17 % underestimation in the annual values (Table 3), which is still within the measurement uncertainty. If we'd only consider the first five years, the observations estimate for annual GPP balance 1367 g C m^{-2} and the simulations 1222 g C m^{-2}, an underestimation of 11 %.

Line 500: "instead, we are estimating the tree traits per average individual for a deciduous forest." Not clear to me what this statement means. Because the methods section (2.2.3) only mentions "we used a species-weighted canopy average of the leaf-level parameters, based on the species composition of the forest „ and is not clear how the parameters are weighted and then aggregated (?). Adding a mathematical description here would be very helpful.

We apologize for unclear expression in Section 2.2.3. We're only having traits for one tree in the model, that is representing the whole forest.

The earlier unclear sentence was:
"*Our modelling approach does not allow for species separation; instead, we are estimating the tree traits per average individual for a deciduous forest*."

We re-worded the unclear sentence to:
"*Since we do not have the abiliity to model different tree species, we model a deciduous forest composed of trees with identical traits*."

We have now added in a table of the values for different species to the supplemental material and an equation showing how we've calculated the species-weighted average. This is the same approach that has been used in earlier studies making use of these data (Croft et al., 2015, Luo et al., 2018). We haven't really aggregated the values, but we have smoothed the lines for plotting purposes. The only purpose we used these lines was in delaying the leaf chlorophyll development. For comparison of other traits we only used summertime averages.

Line 510: Which drought occurrences?

We refer here to drought periods taking place in late summer. We modified the text to:

"*The increase in the simulations is more abrupt to the summer levels and decline from early summer values occurs quite early, probably due to dry periods occurring during summer*."

Line 515-517: Grammar check and re-writing needed. Sentence is not clear.

The original sentence was: "*Testing model performance a TBM designed for large-scale simulation at site-level is challenging as the model necessarily needs to apply generalizations in process representation in order to have a model that can be applied across sites and at large scales, due to limited knowledge and data needed for large-scale parameterization*."

We have modified this into: “*Testing the performance of a TBM designed for large-scale, site-level simulation is challenging. The model must necessarily make generalizations in the process representation due to the limited knowledge and data required for large-scale parameterization.”*

Line 525: “The tree species composition has undergone changes at the site during our study period, e.g., the red maple was reported to have coverage of 36 % in 1995 (Lee et al., 1999) and 52 % in 2006 (Teklemariam et al., 2009). The impacts that these changes in the tree composition have on the carbon fluxes could be studied by a demographic model with sufficient granularity in the description of tree functional diversity“
Since species compositional shift was not really addressed in this study would suggest to remove this context from the Introduction as currently it reads as if this is one of the aspects this paper addresses.

We did not find a statement regarding to this in the Introduction, but it was prominent in the Abstract with the line: “*However, how carbon and nitrogen interactions affect both carbon fluxes and plant functional traits in dynamic ecotones, which are experiencing disturbance and species compositional shifts remains unclear.*” and perhaps this was what the reviewer had in mind.

We have modified this into: “*However, how carbon and nitrogen interactions affect both carbon fluxes and plant functional traits in dynamic ecotones, which are experiencing biotic and abiotic changes remains unclear.*”

Section 4.4 it is not clear if the model fails to simulate the (legacy) effect of drought because of its structure (representation of the carbohydrate pools) or the lack of precise soil moistures
estimations which directly affect modelled CO2 fluxes. Could this not have been specifically tested (for this particular case of drought conditions) by first calibrating the model using observed soil moisture measurements and testing whether GPP simulations improved during drought? If the model´s limitations during drought is a focus of this paper it deserves a more systematic approach to test this.

We agree with the reviewer, that this topic would benefit from a deeper dive into it. However, calibrating the model by using observed soil moisture measurements is not straightforward. Instead we made a more thorough analysis to see if the lack of drought effect is caused by: insufficient soil moisture description, wrong drought response of the carbon fluxes or insufficient influence of drought in the non-structural carbohydrate pool. We will include this analysis in the new version of the manuscript.

Line 556: What is meant here? Isn’t a depth-resolved soil texture provided to the model? Otherwise, what description of the soil physics exactly is lacking here?

We don’t have a depth-resolved soil texture in the model, we are here referring to needs to change model structure. Without a deeper analysis it is difficult to say whether we’d need changes in the water-retention curve, infiltration properties, pedotransfer functions or something else.

We apologize for unclear formulation and have changed the sentence into: "*The drought response at the site could potentially be improved by calibrating the soil moisture response functions in the model, but probably some structural changes in the description of soil physics would also be required. These changes might involve changes in water-retention curve, pedotransfer functions (Weber et al., 2024) or infiltration properties (Vereecken et al., 2019).*"

Line 570-576: Yes, this hysteresis in TER response to temperature (early season lower respiration than late season) could reflect the seasonal patterns of photosynthate allocation to roots. Tree girdling studies have shown that seasonal pattern of below-ground C allocation may be more important than soil temperature in determining root respiration (see for example Högberg et al. 2003) and that earlier in the season respiration from the soil is mostly due to heterotrophic respiration.

We thank the reviewer for this point and have added the mentioned reference to the text.

Line 591: Increase over time? Sentence reads incomplete.

Apologies for the incomplete sentence and thanks for noticing it. The original sentence was: "*Froelich et al. (2015) found a significant increase in summertime GPP and Gonsamo et al. (2015) significant increase in carbon uptake.*"

We have modified this into: "*Froelich et al. (2015) found a significant increase in summertime GPP and Gonsamo et al. (2015) significant increase in carbon uptake between 1996 and 2012.*"

Line 597: So the PAR that was used as an input to the model was not correct? The model would clearly use PAR, so why give it the wrong forcing?

We have used shortwave radiation, not PAR, as the input for the model, and these two variables have been measured by different sensors. The trend in PAR reported in the Gonsamo et al. (2015) was significant, as was stated in the paper. However, when looking at the PAR dataset more closely (we are doing this for the PAR available from the AmeriFlux database, not the gapfilled data from the Gonsamo study), we noticed that the increasing trend was less than 1 % in a year for the time period of their study (1996-2012). Having this kind of increase in model forcing would not lead to that kind of strong trends in GPP as seen in the observations. Furthermore, when we calculated the trend for our study period (1996-2018), the trend was not anymore significant.

In the forcing that we used, the shortwave radiation, we instead had a small declining trend. Since the trends in these radiation variables are so small, we wouldn't expect them to be the cause for large increasing trends in GPP. We instead hypothesize in the current version of the manuscript that the role of understory with potential other effects, e.g. declining nitrogen and sulphur deposition rates.

Line 603: You mean at this site? It is not clear how these findings are relevant to this study.

Apologies for unclear impressions. The study by Gonsamo et al. has been done at two sites (Harvard and Borden) and these conclusions were true for both sites. We have now clarified in the text, that these results are for the Borden sites.

Line 600-607: The discussion on the ozone effect comes out of blue here, unless my previous comment is clarified.

Yes, thanks, we hope the new version of the text makes it clearer in this aspect.

Lien 614: What is meant by "differences between the annual GPP and carbon balance"?

We apologize for the unclear statement. We've corrected this to:

"*It is interesting to note that the differences between the measured and simulated annual GPP are not apparent in the early years of the record, but emerge in the later years (Fig. S12)*."

Line 619 and 620: Grammar

Thank you for noting this. The original sentence was: "*Including the nitrogen cycle in the simulations did not cause a change the net carbon balance of the ecosystem, …"*

We corrected this to: "*Including the nitrogen cycle in the simulations did not cause a change in the net carbon balance of the ecosystem, …"*

Section 4.8: I would not finish the manuscript on listing technical shortcomings. What have we learnt from long term ecological response of such an ecosystem, observations and model results combined? And what is the outlook under further changes in the climate? (e.g., predicted potential increase in temperature and dryness).

The idea of section 4.8 was not to list technical shortcomings (for that we have Section 4.3), but to describe what kind of new observations would be beneficial to better understand the processes at the site.

However, following the idea of the reviewer, we have added a new section 4.9, that discusses the points mentioned by the reviewer.

The Conclusion section can be shortened.

We've shortened the Conclusions.

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