Borden paper review: Reply to Reviewer #1

(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

In the manuscript “Modelling decadal trends and the impact of extreme events on carbon fluxes in a deciduous temperate forest using the QUINCY model”, Thum et al. explore how using in-situ measurements of vegetation traits to parametrize the nitrogen cycle-enabled QUINCY model improves flux simulations at the Borden Forest flux tower. The QUINCY model is modified to allow for a delay in leaf chlorophyll development and the model runs produced in nine different simulations of varying parametrization and nutrient cycle implementations.

When using locally calibrated parameters and enabling the nitrogen cycle in addition to the carbon cycle, QUINCY performed better against observations, especially for GPP, but still lacked some key behaviors – namely it failed to capture drought response and legacy.

Overall, the manuscript is of a high quality, presenting a well-developed study in a clear and scientific manner. It is a substantial contribution to the literature. I recommend publication following minor revisions.

We thank the reviewer for the positive insight towards our study and thank for the detailed, in-depth comments that have helped us to improve the manuscript.

Specific Comments

1. I like the specific statement of the research questions in the study at the end of the Introduction. However, these are not referred to again. I would suggest a section of the Discussion is reformulated to explicitly restate these questions and then explore the evidence found for each one. This would provide a good narrative throughout the manuscript and help in synthesizing the findings of the study.

This is a good idea and we’ll follow it.

2. Among the limitations of the study is the level of representativeness of the chosen PFT for the flux tower site. According to my reading of Thum et al. (2019) and its supplementary material, QUINCY has the capacity to model an individual gridcell as nested tiles of different PFTs. Does this not provide the necessary flexibility for QUINCY to represent a more accurate mix of vegetation when modelling Borden Forest? What was the inter-species variability in the leaf-level parameters and how much did the species-weighted average differ from the species level values?

To run QUINCY at site level, we can only describe one PFT at a time. Therefore for sites with clearly two PFTs (e.g. tree-grass savannah) we have done simulations with two different PFTs. This approach has many issues, as there is no way for these simulations to communicate and the simulated ecosystems would end up having e.g. different soil

hydrological conditions and we need to add in the results from these two simulations, thus making an analysis more difficult. We did not have leaf level observations on the evergreen needleleaves, so we couldn't have compared our modelling results against observations.

We will add a table having leaf-level observational values for different species.

3. Studies have shown that a simple representation of carbohydrate pools can help to model drought legacies (Guo et al., 2020; Jones et al., 2020). QUINCY has a representation of these NSC pools and so might be expected to capture the drought legacy in 2008. As such, I'd like to see Section 4.4 in the discussion expanded to discuss in more detail, or at least more focused on, the reasons why QUINCY is unable to simulate drought and drought legacy. Exploring the processes behind the observations is interesting but this should be framed from the perspective of model evaluation and used to synthesize our understanding of QUINCY's performance.

Reasons for QUINCY not to capture the drought legacy could be caused by:

1) the drought response function of photosynthesis and soil moisture response of soil respiration incorrect
2) inaccuracies in the simulated soil moisture
3) the non-structural carbohydrate pool sizes are not correct

There are two ways in which the drought can influence the photosynthesis in QUINCY. The leaf stomatal control can be constrained by air or soil moisture and additionally there is a direct limitation to reducing the photosynthesis levels by soil water potential. This way to take drought effects into account in the terrestrial biosphere models is common, even though it has been criticized (Sabot et al., 2022).

To unveil these effects, we made plots of the drought responses for the simulations and the observations to see if the challenges would be in the way the drought responses are described in the model. Next, we made more thorough analysis on how well the soil moisture is simulated in dry conditions. Finally, we checked the simulated NSC pool sizes in the model in years 2007 and 2008 against regular years to see if there potentially could be an effect and also assessed if there was an influence on the simulated LAI values.

We added discussion based on this new analysis and hope that this will better answer the request by the reviewer.

4. Table S1 is referred to throughout the manuscript at greater frequency (12 times by my count) than every figure and table in the manuscript except Figure 4 which is also referenced 12 times. Alone, it is referred to more times than all three of the tables included in the manuscript! I would suggest moving this table to the manuscript or otherwise incorporating this information in a manner that does not result in the jarring requirement of frequently referring to the supplement.

We thank the reviewer for this comment. We've moved Table S1 to the main text, as suggested.

5. Figure 2 shows the mean yearly values for NEE, TER, soil temperature and soil moisture. This is discussed in detail. My feeling, considering the soil temperature plot and the location of Borden Forest, is that snow cover may be playing a substantial role at this site over winter. Note that the observed soil temperature maintains a constant temperature through winter, consistent with an insulating snow cover. This may also explain the model's earlier TER response as QUINCY fails to simulate any snowmelt period suppressing soil respiration despite increasing temperatures (Teklemariam et al., 2009). Was this potential role of snow considered?

The reviewer has a good point about the snow cover. The soil temperatures will be influenced by the snow cover and also the springtime recovery of TER will be influenced.

In the original simulations we did not have the snow module turned on (we did not have snow included in the original version of the model we started to work with). To separately assess its effects we did not re-do all our simulations. Instead, we did a simulation with the CN:LAI&chl -version of the model with the snow module activated and now report the influence on the soil temperatures and carbon fluxes.

Technical Comments

1. L20: Delete "the" from before "ecosystems".

Thank you, deleted.

2. L27: Longer growing seasons compared to what?

We modified the text to:

"*At large spatial scales, satellite observations have shown a lengthening of the growing season in deciduous forests in recent decades that has been attributed to warming temperatures.*"

3. L30: Delete "the" from before "vegetation functioning".

Thank you, deleted.

4. L33: Delete "the" from before "forests". Note this is a recurring issue as per comments 1 and 3. Since we are not discussing specific forests or ecosystems, there should not be a definite article "the" in these instances.

Thank you, deleted.

5. L51: Add "an" before "increase" and a "the" before "land carbon sink". Modify "changing of" to "change in".

Thank you, we have corrected the sentence.

6. L61: GPP has already been defined.

Yes, we removed the definition from here.

7. L69: Delete the comma after “Borden Forest” or add a comma after “continuous data”.

Thanks, comma deleted.

8. L78-79: This sentence needs to be reformulated to make sense.

Apologies for unclear structure. The original sentence was: “*In our research, the data are combined with a terrestrial biosphere model QUINCY (QUantifying Interactions between terrestrial Nutrient CYcles and the climate system) (Thum et al., 2019), which simulates fully coupled cycles of carbon, nitrogen, phosphorus of terrestrial ecosystems coupled representations of the surface and sub-surface budgets of water and energy.*”

The modified version is: “*In our research, the data are combined with a terrestrial biosphere model QUINCY (QUantifying Interactions between terrestrial Nutrient CYcles and the climate system) (Thum et al., 2019). QUINCY simulates fully coupled cycles of carbon, nitrogen, phosphorus of terrestrial ecosystems together with budgets of water and energy.”*

9. L81: ChlLeaf has already been defined.

Thanks, we removed the definition from here.

10. L83 and throughout: Make sure references are formatted correctly, namely the brackets.

Thanks, we’ve corrected this line and checked the manuscript thoroughly in this respect.

11. L125: GPP has already been defined.

Thanks, we removed its definition from here.

12. L163: The acronym QUINCY has already been defined.

That’s right, we removed the definition from here.

13. L202: Define or explain “OCN”.

We have added explanation: “*terrestrial biosphere model OCN*”

14. L228: Delete “an” from before “the slope”.

Thanks, deleted.

15. L243: Add “and” between “humidity” and “wind”.

Thanks, added.

16. L247: Was a single random year used repeatedly in the spin-up or was it a 500 year time series constructed by randomly selecting a year of [CO2] and meteorology for each of the 500 years?

Apologies for the unclear formulation. We have only used the observed meteorological data during the spinup. For the transient simulations starting in 1901 forwards we created a meteorological forcing file by randomly picking a year of the observed years, with the CO_2 concentration varying according to GCP (Friedlingstein et al., 2019). For the spinup we used the first 30 years of this forcing file repeatedly.

17. L249: What data was used for the meteorology between 1901 and 1996 in the transient runs? ERA-5 is mentioned earlier but this dataset begins in 1940.

We've only used the meteorology measured at the site in the spinup. We have clarified this in the text:

"*This was followed by a transient simulation starting in 1901, which was using meteorological data randomly picked from the observed meteorology.*"

18. L264: Change "showed" to "shown".

Thanks, corrected.

19. The model abbreviations are quite long and complicated – is there a way to condense them while maintaining the information?

Thanks for noting this. We decided to change the "C-only" to "C", as the simulation names will still keep their information content. We also changed "C-only,fix" to Cfix".

20. L272: As I understand it, LAI was a parameter that was calibrated and this can be seen by the improved seasonality of LAI in Fig 1c&d. Was there no capacity to correct the magnitude of LAI which is too high in summer and too low in winter?

We only modified the senescence parameter. The deciduous forests don't have leaves in winter in QUINCY, so the only way we could match the wintertime LAI values would be to have in addition evergreen coniferous species simulation. We decided not to do that, as it would complicate this work considerably (also discussed in reply to Comment 2.)

We don't have a simple way to change the summertime maximum LAI in QUINCY. We could try to change the leaf to root ratio, directing more of the biomass allocation going to roots instead of leaves. However, there were several reasons why we did not do this.

1) Had we lowered the LAI, the simulated GPP would have been even lower.
2) The other leaf level parameters would likely have degraded because of this change.

3) The continuously measured LAI was lower compared to other observations, probably being influenced by location of the sensors. Other estimates for the LAI at the site were more in line with our estimate and we therefore also thought that our simulation result would not be so off.

We have added to the text: "*The wintertime LAI will stay overestimated in our current modelling set-up, as only deciduous forest is being simulated. Changing the allocation pattern in QUINCY would have allowed lowering of the simulated LAI, but this would have led to lower GPP values* ."

21. L286: Figure S2 shows 2013, not 2014.

Thanks, we've corrected the text to the year 2013.

22. L297: "The observations show more shallow decrease" - I think this should be "The observations show a more gradual decline in increasing GPP before the peak" or similar?

Thanks, corrected.

23. L304: "parameterizations is in" should be "parametrizations are in".

Thank you, we've made the correction.

24. L305: It may be worth clarifying that this refers to different simulations within the "C-only" model framework only, not between the "C-only" and "C-only,fix" model simulations.

Thanks, that's a good point. We modified the text to: "*The different C model simulations using dynamic stoichiometry (i.e. not including simulations using fixed stoichiometry, Cfix) did not largely impact RMSE and r² values*."

25. L309: Perhaps specify that the "more accurate representation" is better timing of senescence?

Sure, we modified the text to: "*This resulted in a more accurate representation with better timing of senescence of the observed seasonal cycles of GPP, LAI and* Chl_{Leaf} *in the simulations*."

26. Figure 1: GPP and LAI have already been defined, and "leaf chlorophyll" has not been defined as "Chl" but as "Chl Leaf". I also do not think that the colors of each model simulation need to be spelled out in the caption as the legend already provides this information. It should be "... leaf chlorophyll WHICH has been smoothed...".

Thank you for the careful check on the figure and the caption. We removed the definitions of GPP and LAI from the caption, corrected the y-axis labels for leaf chlorophyll, removed the explanations for the colors and made the mentioned language correction to the caption.

27. L313: Perhaps a column should be added to Table S1 containing the percentage figures of under/overestimation.

That's a good idea, thanks! It has been added.

28. L316: Should it be "with only a 1.4% larger value"?

Yes, it should be, and we've corrected that now, thanks.

29. L320: I think this should refer to Table S1, not Table S2.

Thank you for noticing this, this has now been corrected.

30. L322: TER has already been defined.

Yes, we removed the definition from here.

31. L330: "Table 1" should be "Table S1".

Thanks, we have now made the reference to the correct table.

32. Figures S3, S4, and S5 are often used to illustrate points that are difficult to parse from the plots (namely referring to fluxes being over/underestimated in certain months). The plots are too noisy for me to clearly and easily identify these statements so I suggest a different method of plotting this data to illustrate the points made.

We thank the reviewer for pointing this out. We modified the figures to show the monthly values instead of the daily values to make the plots less noisy. We also added r^2-values for these plots as suggested by Reviewer #2.

33. L339: "The early season pattern observed in the simulated NEE is attributed to heterotrophic respiration". Is not the overestimation of early season NEE due to a GPP that is too low from DOY ~75, as well as TER being too high between DOY 50 and 100? This is what I read from Figure S6.

Sure, the reviewer is right, we have changed this text into: "*The early season pattern observed in the simulated NEE is attributed to too late onset of GPP and heterotrophic respiration...*"

34. L348: Missing bracket after "(Fig. 2."

Thanks, corrected.

35. L355: This sentence is not properly formatted.

Thanks. The original sentence was: "*The continuous observations of LAI provide values (3.78 ± 0.43 m2 m−2 in summer, averaged the over June-July period, along with the standard deviation)*."

The modified sentence is: "*The continuous observations of LAI provide values of 3.78 ± 0.43 m2 m−2 in summer (averaged over the June-July period, along with the standard deviation).*"

36. L365 and surrounding paragraph: This paragraph could be condensed. It is also unnecessary to specify (C-only:LAI&Chl) and (CN:LAI&Chl) after each reference to the models as it has already been stated that these are the models referred to from Line 318 onwards.

Sure. We removed the specifics about the simulation runs and shortened the paragraph, as suggested.

37. L376: This statement about SLA is disjointed from the rest of the text.

Thanks for noting this. We tried to accommodate it better to the text by moving it and modifying it to:

"*In the model the specific leaf area (SLA) is an important factor in changing the leaf biomass to LAI and it is kept constant. In the observations the SLA exhibited a dynamic change, with higher values (~303 cm* g^{-1}*) observed in the early season and a subsequent decline to a summertime value of 162 cm* g^{-1} *within approximately one month.*"

38. L379: TER is discussed first in Figure 4 but in the plot, GPP comes first. I would ensure these align to improve readability.

Sure, we've made this change now.

39. Figure 4: In this instance, I think it might be better to have the observed and simulated on the same plots, with the different facets instead for the years – as it stands, the figure is better as a comparison between years than for assessing model performance, which I believe to be its main message.

We also wanted to show the behavior in the years 2007 and 2008 against the regular years. We have now modified the plot so that the measurements and observations are shown in the same plots and the regular year behaviour in thin lines.

40. Figure 4 and Figure S9: As in other plots, could the standard deviation be plotted as a shaded area around the observation means?

We modified the Figure 4 according to the comment #39 and also added in the

We have added the standard deviation to the line that is the average of several years in Figure 4.

41. L392: Move the sentences about GPP to the next paragraph which discusses GPP

more widely, rather than in this paragraph which is about TER. Section 3.4 in general could use some work to improve the flow of the section and maintain a narrative throughout instead of the current text which tends to jump between the discussed variables with little reason. Either discuss each variable in turn, or discuss each time period in turn.

Sure, we followed the instructions by the reviewer and moved sentences about GPP. Additionally we worked on improving the flow of this section and discussed each variable in turn, as was suggested.

42. Table 4: The QUINCY LOS (GPP) is incorrect – it should be 146 days if calculated as the difference between the mean SOS and mean EOS as the other table elements seem to imply (or perhaps this is coincidence and it is actually the mean length of each individual growing season, in which case this should be clarified).

We thank the reviewer for such careful investigation of the values. We had added a wrong number to the table for the QUINCY SOS (GPP), it should have been 142. We have corrected this now in the table and text.

43. L417: “LAI based estimates” is misspelled.

Thanks, corrected.

44. L420: “takes place in average” should be “takes place on average”.

Thanks, corrected.

45. L429: Clarify what is meant by “make use of different spring and autumn periods”.

We have changed this into: “*The real forest with several species might have more resilience to different environmental conditions and therefore different species might be able to benefit from differing environmental conditions during shoulder seasons*.”

46. L438: There appears to be a significant breakpoint in GPP around 2009. Are there any explanations for why this might be? Is this a recovery from the 2007 drought? Is there any potential reason why every year post 2010 has higher GPP than any year before? Why were the final 5 years removed as a test?

Gonsamo et al. (2015) studied the trend in GPP at the site and found a significant trend in photosynthetically active radiation and attributed this change to cleaner air. They also saw significant trends in length of the carbon uptake period (CUP) and delay of the ending in the carbon uptake period. The delay in autumn might have to do with different responses to environmental conditions by carbon uptake and respiration. We did a simplified analysis of the CUP and noticed also a significant trend in the ending of CUP and length of CUP in our model simulations for the same time period as Gonsamo did in their observations, but when we extended the analysis period until the end of 2018, the significant trend disappeared.

In the observations the significant increase in GPP happens mostly during summer months, with some increase also taking place during autumn (Table S2). If this change was caused by environmental conditions, such as increasing atmospheric CO2 or air temperature, in theory we should be able to capture this increase by our model.

The increase in site observations could be caused by demographic changes in the forest. There are tree seedlings in the forest floor and as they grow, they might be able to photosynthesize more. Their influence would not be captured by the continuous LAI observations nor our model.

One thing that we had not addressed in the manuscript is the role of nitrogen deposition. Our model input data shows a top value in 1995 at the site and then a continuous decrease until our study period, 2018. This is in line with the sulphur deposition change that has been reported for the Egbert site, that is close to the Borden forest (Gonsamo et al., 2015). The increase in GPP could partly be caused by the recovery of the forest from ozone, nitrogen and sulphur depositions, but it would be expected that this recovery is seen as increasing LAI, which is not seen in the observations. Decreasing nitrogen and sulphur depositions might alter the soil pH, causing alterations to the nitrogen cycle. If we had leaf nitrogen observations over different time periods we might be able to assess this. We'll add these points to the new version of the manuscript.

Removal of the last five years was quite arbitrary. We have mentioned this now in the text.

47. L445: Why is LAI not plotted in Figure S12 if it is discussed here?

We have added LAI to plot Fig. S12.

48. L451: Can the differences in IAV be quantified somehow, for instance comparing the standard deviations?

Sure, we added in here the standard deviations from the Table 3 (earlier Table S1) and noticed an inaccuracy in the earlier formulation, as the standard deviations of GPP and TER are comparable in the observations.

49. L469: Incorrect parentheses for reference.

Thanks, corrected.

50. Section 4.1: I'd like to see more discussion here about how the continuous LAI measurements improved the model performance. What has been learnt from this study and how can these lessons be implemented in the model? Does the senescence parameter need to be modified in the standard implementation of the model? Do we need to test the model tuned with continuous LAI values at other sites?

For the standard implementation of the model, we have tested the parameters at several sites (against GPP observations) when deciding on the default parameters. In the current model development phase the QUINCY model developers use satellite observations at site

level for FLUXNET sites as well as the benchmarking system iLamb for global scale evaluations. Therefore we'd not change parameters based on one site observations. An on-going analysis based on flux tower and remote sensing data (Miinalainen et al., in preparation) shows several other sites with the same plant functional type are overestimating both GPP and (satellite-based) LAI in October, so the results from Borden are in-line with these estimates and with the uncertainties of phenological transition dates of remote sensing observations (Wang et al., 2024), are a valuable site-level verification for these results. We've added to the text:

"*The parameterization of QUINCY relies often on several sites, as the aim is to have successful large scale simulations. However, an on-going study evaluating the QUINCY seasonal cycles with flux tower and remote sensing data (Miinalainen et al., pers comm.) has shown that a similar bias in autumn phenology for temperate broadleaf deciduous occurs at several other sites, so this PFT could benefit from parameter tuning. However, the satellite observations have some issues in predicting the autumn phenology (Wang et al., 2024) and the long-term in-situ observations at the Borden site can act as a valuable verification resource.*"

51. Section 4.2: Again, I think there could be more discussion here about what the simulated ChlLeaf values can teach us regarding QUINCY and future improvements. What does it mean for the model simulations that Chl Leaf peaks early? What might this imply for QUINCY applied at other sites or globally? While not the objective in this study, what might be learnt if we did attempt to capture the timing of the maximum leaf chlorophyll? What would the tradeoffs be?

The peak in the observed LeafChl occurs around DOY 180-200 (Fig 1e). This is not seen in the Vcmax and the Jmax observations (Fig. 3). Our aim is to improve the photosynthesis and despite leaf chlorophyll being part in the photosynthesis calculation of the QUINCY model, the Vcmax and Jmax values play a larger part and the LeafChl is considered to have highest relevance because of its link to these parameters. Increasing LeafChl values for the time period visible in the observations (Fig 1e) would not probably improve our results. We could do sensitivity testing with the canopy module of QUINCY to see the influence on GPP of forcing LeafChl to a higher level for this time period. Considering the model structure, this influence would not be very large.

Earlier studies by simpler approaches have shown a more pronounced influence of using leaf chlorophyll in modelling. Croft et al. (2015) replaced LAI in a light use efficiency (LUE) model by leaf chlorophyll and they obtained a better seasonal cycle of GPP by this change. The r2 of daily GPP from the LUE model using LAI was 0.55 and with leaf chlorophyll 0.65 and with canopy scaled chlorophyll 0.69.

In the study by Luo et al. (2018), the LeafChl was directly used to estimate Vcmax and Jmax and they found improvements in model performance, r2 for GPP in 2013-2014 was increased from basic model formulation 0.84 to 0.91. This study was done by the BEPS model, which is usually run with LAI as input.

Our study was partly inspired by these works, as we're now having a model that explicitly includes ChlLeaf. Our approach has a direct coupling of LeafChl to the nitrogen cycle

modelling, therefore we find this variable very interesting. With both carbon and nitrogen cycles included our approach is more constrained than the modelling approaches in the two earlier studies. The impact we obtained was not as pronounced as illustrated by these two earlier studies. One reason is that our model is constrained by many processes. One difference between our study and the study by Luo et al. (2017) is that we don't have a direct linear relationship between the leaf chlorophyll and Vcmax, as has been shown to be the case at the Borden site (Croft et al., 2015). Despite this, our comparison with the biochemical model parameters showed reasonable values and didn't require changes for the formulation. However, the LeafChl formulation of QUINCY might benefit from linear relationship between LeafChl and Vcmax and this is one of the issues tackled in an on-going study (Miinalainen, T., in preparation), where remotely sensed LeafChl is compared to QUINCY estimates across several flux sites.

A recent study combining chlorophyll fluorescence observations with the LeafChl and biochemical model parameter observations at the Borden site reveal that the different light acclimation rates of Vcmax and LeafChl cause changes in their relationship during different seasons (Yu et al., 2024). We could test the influence of changing these relationships with the canopy version of the model, that is modelling only the leaf canopy, so this would be one potential way to further study this topic.

We added these points to the text by: "*The peak in values in observed ChlLeaf during midsummer was not simultaneously accompanied by increases in the biochemical model parameters Vc(max) and Jmax (Fig. 3), which would have a more pronounced effect on the simulated photosynthesis than ChlLeaf. We could test the effect of increased midsummer ChlLeaf on photosynthesis using the canopy module of QUINCY, which calculates only the canopy part of the model. Given the structure of the model, this influence is likely to be small. Luo et al. (2018) used the fact that ChlLeaf and Vc(max) have a linear relationship at the Borden site to improve their model results for GPP and evapotranspiration. In QUINCY we currently do not have this linear relationship, although it has been observed in some studies (e.g. Qian et al. 2021). A further study using remote sensing data and more sites will explore the implementation of such a description of leaf N partitioning in QUINCY. At the Borden site the linear relationship between ChlLeaf and Vc(max) was found to be off due to their different rates of light acclimation (Yu et al., 2024). Once we have a linear relationship between ChlLeaf and Vc(max) implemented in QUINCY, we can use the canopy module to estimate what effect this would have on photosynthesis in our model.*"

52. L511: "observed transitions is more smooth" should be "observed transitions are more smooth".

Thanks, corrected.

53. L514: I'd change "the model used here" to "QUINCY".

Thanks, we did this change.

54. L515: Missing word between "Testing model performance" and "a TBM designed".

Thanks, corrected now to: "Testing the performance of a TBM designed…"

55. L570: “It occurs most pronounced” is not grammatically correct – replace with “It occurs most prominently” or “It is most pronounced”.

Thanks, replaced to: “*It is most pronounced in the year following the drought, 2008 (Fig. S10e).*”

56. L571: “Based on the data available then…” reads as if referring to the 2008 drought due to the prior sentence. I’d recommend “Based on the data available to Lee et al. (1999), they found …” or similar.

Thanks, we used the formulation suggested by the reviewer.

57. L583: Delete comma after “exudates”.

Thank you, comma is deleted.

58. L587: Typo in “whereas is was”.

Thank you, corrected to “whereas it was.”

59. L598: “QUINCY does simulate” should be “QUINCY does not simulate”.

Thanks, corrected.

60. L600: There are quite a few statements throughout the manuscript similar to the sentence here: “One additional cause of model failure might be that the canopy light-saturation point does not reflect the observations, however, there is not robust evidence that this is the case.” These require at least some explanation of what potential sources of evidence were explored and discounted.

The point we wanted to make here was that the responses to light might not be similar in the model because e.g. we don’t have the understory saplings in our model set-up. Since we have made this point elsewhere in the manuscript, we remove this point from here and add discussion about the potential recovery of the forest from nitrogen and sulphur deposition, as discussed in point #46. We will try to identify other such statements and add justification.

61. L605: Add “an” before “impact on the N cycle”.

Thanks, added.

62. L619: Add “in” after “cause a change”.

Thanks, added.

63. L623: Add “an” before “unrealistically low value”.

Thanks, added.

64. L630: Add “the” before “N saturated case”.

Thanks, added.

65. L631: Delete “and” before “in line with”.

Thanks, deleted.

66. L633: Delete “of” in Section 4.8 header.

Thanks, deleted.

67. L642: Add “A” before “rain gauge”.

Thanks, added.

68. L648: Specify that the “long time spans” is referring to long time series of observations.

Thanks, we modified the sentence to: “*This work demonstrated the usefulness of using different data sources and the importance of observational long time series.*”

69. L648: Specify that the “use of leaf chlorophyll content and LAI” is in parametrizing the model.

Thanks, the sentence is now: “*The use of leaf chlorophyll content and LAI in parameterizing the model improved simulated GPP in the CN simulations.*”

70. L658: Change to “attributed to an increase in PAR which is not visible in the shortwave radiation forcing for QUINCY”.

Thanks, changed.

71. L661: Delete “is” from before “paves the way”.

Thanks, corrected.

References

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