Dear Reviewer One,

Thank you for taking the time to review our manuscript. We have used this feedback to correct and better explain several equations. In the revised manuscript, we will better explain model technicalities such as the model spin up, soil profile and the calibration process. Please find our responses to your comments below.

Peatland-VU-NUCOM (PVN 1.0): Using dynamic PFTs to model peatland vegetation, CH_4 and CO_2 emissions
The manuscript by Tanya Lippmann et al. provides a comprehensive description of the implementation of vegetation dynamics into the Peatland-VU model, based on the vegetation dynamics used in NUCOM-BOG. The manuscript documents the representation of vegetation dynamics that is used, and compares the new model with observations of greenhouse gas fluxes, observed cover fractions, and with model simulations with the unaltered Peatland-VU model.

I appreciate the detailed documentation of the equations that the model is based on, and I consider this well suited for GMD. In a few occasions, the rationale of these equations could be explained better – these are mentioned in the remarks below. Also, the simulation setup and driving data (frequency, timestep) ought to be described in more detail (see below). The results section is relatively straight-forward and provides first and foremost an overview of the basic behaviour for two Dutch sites, but I think that this is suitable for a paper that first and foremost documents a new model.

I consider this manuscript suitable for publication in GMD once these omissions have been corrected. I provide a list of detailed remarks below, which I hope can help the authors when revising the manuscript.

Response: We thank the reviewer for taking the time to review our manuscript and for the positive evaluation. We have expanded on the rationale used when choosing equations, as well as the input data, and simulation setup. Please see our responses below.

Major remarks
L 29: "Between 2005 and 2008, …" Please check this sentence – it is unclear which of the two studies the citation originates from, and the natural emissions that contributed 50% to the total CH4 budget are not all originating from peatlands, so the relevance is not clear here.

Response: This is now written as "Between 2005 and 2008, natural CH4 emissions (e.g. peatlands, lakes, other wetlands etc.) contributed approximately 50% of total CH4 emissions (Saunois et al., 2020)" (L30).

L 122: Please explain how the initialization of the carbon pools is done. Is this based on observations, or does the model require a spinup of some sort?

Response: We have included the sentence, "The length of the model spin-up was five years, determined by the time taken for the SOM pools, below-ground CO2 and below-ground CH4 concentrations to stabilise." (L350)

L 137-152, Eqs. 2-3: It would be nice if you could explain the competition for light a bit further. The equations for plant height (Eqs. 2-3) originate from applications to trees (with D being the stem diameter). Do you apply these equations to the grass vegetation from your sites? It would be good to assess how accurate these equations are for that purpose. Also, the description seems to indicate that all leaf area is assumed to be

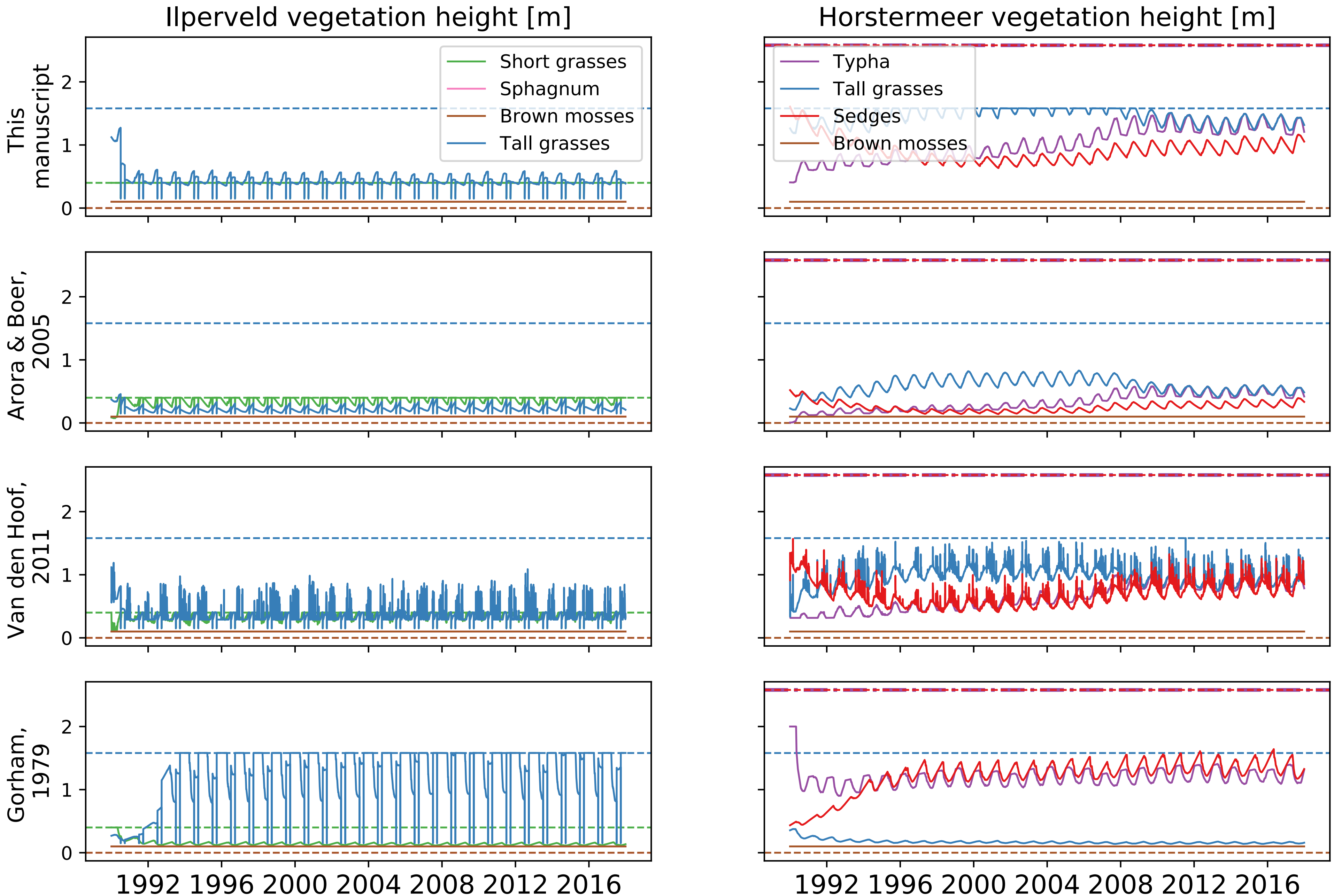

Figure 1. The results of the height functions listed above when included in the calibrated model simulations. The dotted lines are the PFT max heights (Table S3).

accessible for light if the PFT is highest, whereas the grassland vegetation you have at the sites will have leaf area distributed between the soil and the top of the plants (meaning that only part of the highest plant will receive light without shading by others, and part of the leaves may well be shaded). It would be good to see the rationale explained a bit further here.

Response: A similar point was also raised by the second reviewer. The purpose for calculating height in the model is to sort the plants in descending height order to replicate shading. We understand that this relationship was not initially intended to be used for grasses, sedges etc. However, it has since been used to calculate the heights of natural and agricultural grasses in a dynamic global vegetation model (Krinner et al., 2005). The relationship used here relates vegetation biomass to height. Heights estimated using this relationship are within the range of heights recorded in the literature (Chapin et al., 1996; Kattge et al., 2020). Biomass and density have been found to respectively explain 98% and 81% of the height variance in 65 plots of 29 different species (Gorham, 1979). This is because most plants are understood to be constrained by 'self-thinning' under crowding in natural stands or by a trade-off between height and foliage growth, reflecting a trade-off between structural and functional physiological development. We have added this text and these references to the revised manuscript (L155-L165).

We have performed model simulations using the following three height relationships and compared the results with our model simulation results (please see Figure 1 of this document):

1. Function relating biomass and height that has been applied to grasses and crops (Arora and Boer, 2005).
2. Function relating height and LAI used in JULES-SUCROS model that has been applied to grasses (Van den Hoof et al., 2011)
3. A height relationship using biomass and density was developed using observations of monospecific single stemmed plants, ranging in size from a moss to a tree. (Gorham, 1979).

We found very good agreement between our height relationship and the first height relationship (Arora and Boer, 2005). Applying this height function does not change the sorting of heights nor the model simulation results in this study. We found reasonable agreement with the second and third height functions tested, however, relation 2 and relation 3 do affect the sorting at times. We have included the three height relationships in the model code for possible future model applications (zenodo.org/record/8065235).

It is an interesting point to consider that plants in the 'top canopy' may be partially overlapped/shaded by plants in lower canopies. However, there is insufficient observational data available to verify to what extent this is happening in these systems, or how this might be represented within this model.

L 159: The "harvest scheme" should probably be explained further: It is described only later in the manuscript that the model is made to describe dynamics of two grassland sites, and "harvesting" will hence be a grass cut. Maybe it would be good to introduce the sites and the typical land use of these earlier in the manuscript. The concept of a "harvest height" makes good sense once one understands that these are grasslands.

Response: We have added more explanation and given this a separate section (Sect 2.1.7, L306-L311).

L 165, Eq. 7: I cannot understand the denominator in this equation. CB*SLA would result in a leaf area index, but it is subsequently divided by a long term including amongst others the growth rate of LAI. Please explain what this equation represents.

Response: The first reviewer raised a similar comment. We agree that this equation should be solved differently. In equation 7, the purpose of the denominator is to relate water availability with plant growth (GPP). We have therefore, moved the denominator to the calculation of GPP. Please find these changes on L128 (Eq 1, GPP) and L174 (Eq. 8, LAI).

Section 2.5 or 2.7: I would welcome more details on the description of the simulation setup. How many years are the simulations (30, I think), what is the temporal frequency of the input data (daily?) and the time step of the model? Is there a spinup, or how is the model initialization done? What is the vertical resolution/how many soil layers are simulated? Also, this would be a good place to introduce the different simulations that are undertaken: "Standard" simulations with the observed input data, and simulations to derive model sensitivities (Table 3).

Response: We have elaborated on the simulation setup both in the Model Calibration, Sect 2.4 (L347-L374) and Sect 2.7, Input data preparation (L413-L438). We have created three subsections in the 'Input data preparation', including Sect. 2.7.2 on the soil profile input data (L424-L429) and specified the soil profile input values (Table S2, Table S5, Table S8, Table S9). All the model simulations presented in this manuscript are presented in section 2.5 (Testing the PVN model) and also described in Table 2, Table 3, and Table 4.

Minor remarks

- Overall, the text is nicely written, but it would be good to check the placement of commas throughout the manuscript

Response: We have paid attention to this in the revised manuscript.

- **L 46:** replace "decompose" with "decomposes"

Response: Done.

- **L 90:** replace "systems"

Response: This sentence has since been removed.

- **L 97-98:** Check formatting of citations

Response: We believe this is correct.

- **L 99:** Check consistency: write both gases as chemical formula (CO2 and CH4) or write out both

Response: At the beginning of a sentence, it is incorrect to use a chemical formula.

- **L 113:** Explain what is meant here with "plant dynamics". Plant growth?

Response: This term is unnecessary and has been removed.

- **L 121:** Check sentence – one verb too much

Response: Done.

- **L 171:** PG should probably be explained here – its definition follows only in Eq. 11.

Response: Done.

- **Eq. 9:** Is the range of 1-P representing all PFTs or only the moss PFTs? Please clarify.

Response: Done.

- **Eq. 11:** FPAR should probably be explained here – it follows only in L. 220

Response: FPAR is first mentioned and described on L170 with its equation (Eq 7).

- **Eq. 17 and L. 220:** Please check the symbols here. I guess that AI*PAR is the total incident PAR, and not only AI? And if FPAR is the fraction, its unit should probably not be J m-2 d-1?

Response: FPAR is the unitless fraction of absorbed PAR. This equation was erroneous and has been replaced by what is now Eq 1 (GPP).

- **L 225:** Add "is" between "a" and "the ratio"

Response: Done.

- **L 228:** Check the unit here – earlier, you defined PAR fluxes per day, now per second?

Response: We have revised the photosynthesis equations and provided the correct units.

- **L 231:** Remove one of the occurrences of "where"

Response: Done.

- **L 254:** FSP appears to describe some sort of empirical function of the timing of root exudates. It would be good to provide the function, or plot the activity as a function of DoY

Response: This was also picked up by reviewer 3. We have changed the text so it now explains: "f(KSP) [-] is a function depending on PFT constant, KSP (Table 1), that can be used to determine stronger exudation occurs during spring" (L228).

- **L 264:** The equation seems to indicate a linear scaling with T(t). Does this give problems when T<0°C?

Response: We have clarified that litter does not decompose when temperatures fall below zero. (L247).

- **L 277:** Explain the SphagnumCarex here – is this Sphagnum peat, or is Carex somehow part of it?

Response: We have included a '/' here so it reads '*Sphagnum/Carex*' (L319).

- **L 321:** Check "photosynthetically active radiation"

Response: Done.

- **L 339 ff**: Please provide information on the frequency of the measurements. How long was the time in-between the closures of the chambers?

Response: Done.

- **L 343/344, 357:** It is not clear what "daily hourly average" or "daily monthly temperature" refers to. Are these daily or hourly / daily or monthly averages?

Response: Thank you for picking up on this. We have changed the text to: "From this data, the hourly average CO2 (net ecosystem exchange) and CH4 fluxes were calculated for each day. We compared calibrated site simulations against observed daily average CO2 and CH4 fluxes"(L404 – L406).

- **L 365:** remove "on"

Response: Done.

- **L 369:** Sentence is a bit repetitive ("to outperform the Peatland-VU model") – please check

Response: This was also mentioned by reviewer 3. We have removed this sentence because it is not needed here.

- **Section 3.1:** Before presenting the results of the sensitivity work, the authors might consider presenting the basic behaviour of the model, which is now only presented in Sections 3.3 and 3.4. It is hard to relate to the sensitivities without a good understanding of the overall seasonal and interannual variations.

Response: We can understand the rationale behind this suggestion. However, in this study, we feel it important to first explain how the model works by presenting the results of the sensitivity tests before showing the model results. This is consistent with other modelling studies (published in Geoscientific Model Development) that include sensitivity testing (Raivonen et al., 2017; Stocker et al., 2014; Wania et al., 2010).

- **L 376:** Explain what is meant here with "belowground CO2 emissions". Emissions from belowground pools to the atmosphere? And does this mean that the uptake of CO2 by plants is not accounted for in the results?

Response: This refers to soil respiration. We have changed the text to clarify this. (L448)

- **L 381 and other occurrences:** Replace "warming air temperatures" with "warming", "rising air temperatures" or "increasing air temperatures"

Response: Done.

- **L 382:** Put the reference to Figure 4 in brackets

Response: Done.

- **L 393:** What does the "(5)" refer to? Figure 5?

Response: Done (Table 5).

- **Fig. 2 and 3:** Clarify what the individual points are presenting here. Annual means for each of the 30 years? Also, make sure to add a time unit to the CH4 and CO2 fluxes on the axis (I guess g CH4/y)

Response: We have amended the captions of Fig 2 and Fig 3 to clarify that each dot is the mean daily litter & root mass for each year. We have added the time unit to the y axes.

- **L 422ff:** The results present a number of additional simulations that have not been introduced before. It would be good to add the meaning of these in the methods section. In particular the PVN_FPAR_CONST is not intuitive to understand.

Response: We have included an overview of these simulations in Table 3.

- **Fig. 4:** What do the lines display in these figures, are these means for all years? Also, it is unclear what the "x0.15" in the harvests indicates. Is this the cutting height?

Response: We have edited the figure caption to make this clearer (Fig 4).

- **L 459:** Add "and" after CO2.

Response: Done.

- **L 490:** These are not histograms, as they do not display a frequency distribution. You can call them bar graphs or similar (but it is not necessary to describe the graph type in the text)

Response: Thank you for picking up on this. We have removed the reference to 'histograms' from this sentence.

- **L 536:** remove apostrophe after “series”

Response: Done.

- **L 572:** Clarify what data frequency the R^2 values are based on. I guess this is based on the daily data? There would be too few years to compute statistics based on the annual numbers, I suppose.

Response: We added “A simple linear regression was used to compare the model simulation results and observational data using all days with available measurements” to L576.

- **L 580:** add “of” after “role”

Response: ‘Understanding the role dynamic PFTs have on CO_2 and CH_4 emissions in peatlands’, is correct.

- **L 622:** write out “AC”

Response: This was defined in the methods.

- **L 635:** replace “limits” with “range” (or similar)

Response: Done.

- **L 642:** explain the parameters used here (MethanePType, LeafRespirationCoeff, BiomassSenescence) to clarify what they describe and how they impact the simulation results

Response: We have included an intext reference to Table 1 where these terms are described (L719).

- **L 660:** The last sentence seems to be unfinished – it would be nice and relevant to compare your work with the studies mentioned here.

Response: Revised (L742-L745).

- **L 666:** Explain the “order” – are these ordered by relative decomposition rates?

Response: We have changed the sentence to: “Decomposition rates have been found to differ between forbs, graminoids, deciduous shrubs, and evergreen shrubs”. (L7459)

- **L 676:** replace “large” with “thick”

Response: Done.

- **L 709:** clarify which uncertainty is referred to here. Uncertainty in the simulated CO_2 fluxes?

Response: Done. The sentence has been changed to: “Photosynthesis and leaf respiration mechanisms were the greatest cause of uncertainty in the model's ability to reproduce the net GHG budget” (L792 – L793).

- **L 726:** I appreciate the comparison with the PEATBOG model. Maybe you can add more here than just the net annual GHG emissions? As this model also has dynamic PFTs, how does this influence their simulations? And what do you expect when comparing the Mer Bleue site to your sites?

Response: Done. L810-L824.

We appreciate your detailed feedback that greatly improved our manuscript. We hope that these responses have resolved your concerns.

Sincerely,

Dear Reviewer Two,

Thank you for taking the time to review this manuscript. We appreciate your useful suggestions and comments. We have used this feedback to correct or better explain several equations. In the revised manuscript, we clearly introduce key peatland processes. We have improved the clarity of the manuscript by merging Table 1 and Table 2 and providing a table of the different SOM pools. Please find our responses to your comments below.

Original comment:
The manuscript by Lippmann et al. describes the new Peatland-VU-NUCOM (PVN) model. The new model has been assembled from two parent models, the NUCOM-BOG model and the Peatland-VU model. The specific aim of the new model has been to better represent vegetation dynamics and implement different plant functional types (PFTs) to improve simulated fluxes of CO2 and CH4. The authors then use the model implementation to simulate CH2 and CH4 fluxes and vegetation dynamics in two wetland sites. A substantial sensitivity analysis is also carried out, which I appreciate.

The authors' objective is sound, and the model does include a few new and novel improvements which could be a welcome addition to the scientific literature. Mainly, the inclusion of mosses and reeds (*typha*) is interesting. The model also includes different formulations of CH4 dynamics compared to the widely used scheme by Wania et al., which is interesting.

The text is generally well-written with clear language. The model description mostly provides good insight into the framework and theory of the model. However, I believe that the text would benefit largely from a major restructuring. Much of the description of vegetation dynamics is scattered over large parts of the text instead of following a clear line of presentation. An example of such a structure would be to start with the plant carbon source (photosynthesis), then move on to tissue turnover (mostly referred to as senescence in the text), and allocation of each PFT. Then follow up with competition and the general path/column vegetation dynamics. The implications of the vegetation dynamics for the carbon fluxes could then be described. Variables or functions included in equations sometimes lack descriptions in the text or are not mentioned. I understand that these formulations might be described in other sources, but they should be properly referred to so the reader can easily look them up.

Lastly, and most importantly, some of the assumptions and equations described in the manuscript are erroneous and/or poorly described. I will describe these in detail below, however, due to the severity of these errors I feel that the theoretical framework is not yet ready for publication. I thus recommend the editor reject the manuscript in its current form but encourage a re-submission once these errors have been sorted out.

I have further divided my comments into three sections. One deal with the model description, the second deals with major comments on the other parts of the manuscript, and finally technical notes.

Response: We thank the reviewer for their constructive and positive comments. We particularly thank the reviewer for their close revision of the equations, terms, and units. We understand and agree with the rationale to begin with the carbon source (photosynthesis), and move to plant turn over, and methane dynamics and have revised the order of the methods section accordingly (Sect 2.1.1 – Sect 2.1.7). We placed the competition functions after the photosynthesis & respiration functions and before belowground production because we thought that plant competition plays and important role on above- and below-ground biomass production, and this closely follows from photosynthesis and plant respiration. We have revised the manuscript to ensure that we define and describe all variables and all equations correctly. This point was also raised by reviewer 1 and 3. We have added more detailed descriptions to several of the equations (L120-L137, L155-L166, L121-189, L207-L217, L232-L237, L250-260, L276-L298, L306-L311). Please see our replies below.

Comments on the model description

Original comment:
The allometric equations used to partition the carbon are developed and parameterised for trees. The D in the model is the diameter of the tree and BD is the wood density of the tree. It is difficult – or even impossible – to transfer these allometric relationships to grasses, and even more so to mosses. The authors better motivate how this adaptation could be done and why these are valid assumptions. I believe that this assumption needs to be completely revised and filled with scientifically more sound parameterisations.

Response: Thank you for raising this point and providing an interesting discussion. A similar point was also raised by reviewer 1. The purpose for calculating height in the model is to sort PFTs in descending height order to estimate shading. Please note that this height relationship is not used for moss species because moss species are always considered to be the lowest plants, at surface level. We will amend the manuscript to make it clear that this relationship is not used for moss PFTs. We have revised the allometric height equation and corrected an error in the writing of constants, k1, k2, and k3. Equation 2 now reads:

H = k2 * D ^ (K3)

where, k2 and k3 are equal to 40 and 0.85, respectively (Smith et al., 2001).

We understand that this relationship was not initially intended to be used for grasses, sedges etc. However, it has since been used to calculate the heights of natural and agricultural grasses in a dynamic global vegetation model (Krinner et al., 2005). The relationship used here relates vegetation biomass to height. Heights estimated using this relationship are within the range of heights recorded in the literature (Chapin et al., 1996; Kattge et al., 2020). Biomass and density have been found to respectively explain 98% and 81% of the height variance in 65 plots of 29 different species (Gorham, 1979). This is because most plants are understood to be constrained by 'self-thinning' under crowding in natural stands or by a trade-off between height and foliage growth, reflecting a trade-off between structural and functional physiological development. We have added this text and references to the revised manuscript (L155-L166).

We have performed model simulations using the following three height relationships and compared the results with our model simulation results (please see Figure 1 of this document):

4. Function relating biomass and height that has been applied to grasses and crops (Arora and Boer, 2005).
5. Function relating height and LAI used in the JULES-SUCROS model that has been applied to grasses (Van den Hoof et al., 2011)
6. A height relationship using biomass and density developed using observations of monospecific single stemmed plants, ranging in size from a moss to a tree. (Gorham, 1979).

We found very good agreement between our height relationship and the first height relationship (Arora and Boer, 2005). Applying this height function does not change the sorting of heights nor the model simulation results in this study. We found reasonable agreement with the second and third height functions tested, however, relation 2 and relation 3 do affect the sorting at times. The height function used within the JULES4.6 model (Harper et al., 2018) uses a 'balanced LAI' function (Cox, 2001), which is not in the PVN model. We were therefore unable to implement this height scheme into the PVN model, nor compare our results with this relationship as we had proposed during the Open Discussion period. We have opted to continue to use the height relationship included in the original manuscript. However, we have included the three height relationships in the model code for possible future model applications (zenodo.org/record/8065235).

Original comment: FPAR, which is calculated in eq. 4, is usually calculated only as the Lambert–Beer law (i.e., $e^{-k * LAI}$). This could then be integrated over the canopy. I believe that is the intention with the latter formulation of CB * SLA in eq. 4. I do not understand why the plant's total biomass (CB) should be included in this calculation. A more reasonable approach would be to use LAI or leaf biomass * SLA. The authors need to motivate why total plant biomass is a good variable to include in the calculation of the fraction of absorbed light.

Response: We apologise that these parentheses were placed erroneously and thank you for picking up on this. We have also replaced CB* SLA by LAI, as per Eq 8. We have amended this equation in the revised manuscript to now read:
FPAR = 1 - e^(-LEC * LAI)
This approach is an application of Lambert-Beer's law taken from (Heijmans et al., 2008), which is aligned with previous applications of the relationship (Huisman and Olff, 1998; Prentice et al., 1993). Please find these changes: L171, Eq. 7.

Original comment: I am a bit confused by the formulations in equations 5 and 6. Does the senescence of leaf material happen daily? This sounds like a biomass turnover to me, which should be subtracted from GPP (photosynthesis) to obtain NPP. In that way, the turnover is double counted when SM is substituted into equation 5. Furthermore, NPP is a rate. This means that in eq. 6 the variable SM would also be a rate where RS will represent the partitioning of new biomass into the aboveground carbon pool. Since eq. 5 (and other functions) uses the dx/dt approach to denote change, SM in eq. 6 erroneously appears to be a state variable.

Response: We agree with you that the units of NPP are Kg C m-2 day-1. We have corrected the text in the revised manuscript. Equation 5 has been revised because it was missing a term:
d(CB)/dt= SM – BS * CB
where the units of SM and NPP are both Kg C m-2 day-1.
This removes the indication of double accounting. Please find these changes: L177, Eq. 9.

Original comment: LAI is commonly defined as just leaf mass * SLA (mass/area). The addition of a light extinction coefficient and water growth factor with the change in LAI makes no sense to me and is not explained further in the text. LAI is a state variable in the model and its temporal development is determined by the development of the leaf mass, sometimes modified by a phenology factor. If the authors make a new formulation for LAI it should be clearly derived in the text.

Response: The first reviewer raised a similar comment. We agree that this equation should be solved differently. In equation 7, the purpose of the denominator is to relate water availability with plant growth (GPP). We have therefore moved the denominator to the calculation of GPP in the form of a water stress factor (WSF). Please find these changes on L130 (Eq 1, GPP) and L174 (Eq. 8, LAI).

Original comment: A dynamic thickness of a moss layer is an interesting feature of the model and could potentially be of interest to other models and the vegetation modelling community. I appreciate the authors' work on this subject but have a few comments that I believe will strengthen the manuscript. First, I believe that this subject is not very well known in the broader modelling community and the theory could thus be expanded somewhat, perhaps even moved to its own section. Secondly, I am not sure why Gmax is used instead of NPP, please provide a justification for this in the text. The parameter DBD might potentially be important, and it would be good with a sentence or reference to how this value is constrained by data. Lastly, I cannot find any other mention of moss thickness in the manuscript. A sentence or two about how this is used in the would be good. There are several places where this could improve the model, but it is not stated.

Response: Thank you for your interest in this feature. We have added the sentence, "Spatial variation in the rate of vegetation growth and decomposition, particularly for bryophyte species, leads to the creation of microforms, such as hummocks, hollows and lawns which in turn impact the water level relative to the surface and spatially variable fluxes (Waddington and Roulet, 2000)" to the third paragraph of the introduction (L47—L49).

Unfortunately, the thickness of the moss layer is not yet used by the model. Future model versions will recalculate surface height and also the soil properties (DBD, pH, OM content of top soil layer(s).) We have added the text to L186 – L189: "The thickness of the living moss layer is not yet used by the model. Future model versions will use the thickness of the moss layer to recalculate land surface height, impacting the water level relative to the surface and also soil properties (such as DBD, pH, OM content of top soil layer(s))."

Gmax is used in the calculation of potential growth by the NUCOM model (Heijmans et al., 2008). To increase clarity, we have renamed the variable in Table 1 and included the definition at L195.

Original comment: Please provide the equation for TG as well. It is not obvious to me what temperature parameters are congruent to WL. Lastly, please consider reformatting the equation into the commonly used grouping by curly braces.

Response: We have amended the manuscript to include the equation for TG and used curly brackets for both the WG (Eq 14) and TG (Eq 15) relationships (L200-L203).

Original comment: Eq 13. Seems to have a few sign errors. The production of CH4 should of course be positive while plant transport, CH4 oxidation, and ebullition are losses of CH4 from the layer and should have a negative sign but are now positive. The diffusive flux could be either negative (more CH4 leaves the layer than what is added from lower layers) or positive, its sign is thus determined by what direction the flow of CH4 has and the sign convention used. Furthermore, diffusive fluxes generally need to be solved numerically in computer programs. Please include a sentence on how this flux is calculated.

Response: We have corrected the +/- signs in this equation (L280, Eq 29).

Original comment: The formulation of plant transport of CH4 described in Eq 14 and 15 contains at least four PFT-specific parameters. This would yield a large parameter space for calibration. Please described how these parameters are constrained by data or theory.

Response: Thank you for raising this point. We have elaborated on these terms in Sect 2.2.1. cP is a rate constant of the unit 0.24 day-1 (taken from (Walter and Heimann, 2000)). LAI (Eq. 8) and root mass (Eq. 18) are represented by the model and described in the manuscript. The unitless parameter, PlOx is the fraction of CH_4 consumed by rhizospheric oxidation (fraction of CH_4 oxidised when CH_4 passes through the oxic zone around the root tips) and the unitless parameter, vP, describes the plant's ability to conduit CH_4 through aboveground plant tissue. Shrubs and trees generally do not have aerenchyma whereas, grasses and sedges can have large or small aerenchyma (Ström et al., 2005; Walter and Heimann, 2000). The values for these PFT parameters are taken from the literature (values in Table 1, references in Table S3). To better describe these terms, we have amended the text L288-L298.

Original comment: Eq 16 seems to have the wrong sign for the integral of BCO2.

Response: We have corrected this sign (now in Eq. 4).

Original comment: The photosynthesis algorithm used is derived from (Haxeltine et al., 1996) which is a general light-use efficiency model. The potential photosynthesis (AP) described in eq. 17 has however been modified with the addition of a temperature function and instantaneous photosynthesis (AI). While I can see the value of an additional temperature modifier, it is difficult to understand what the AI parameter adds. The modification is also not derived or referenced in the text. Please also provide a reference for the temperature function.

Response: We acknowledge the confusion regarding the light-use efficiency model (Haxeltine and Prentice, 1996a) because there were three photosynthesis models published in 1996 by Haxeltine et al: the light-use efficiency model (Haxeltine and Prentice, 1996a), BIOME2 (Haxeltine et al., 1996), and BIOME3 (Haxeltine and Prentice, 1996b). We would like to clarify that our reference should actually be BIOME3 (Haxeltine and Prentice, 1996b). Based on the reviewer's comment, we have come to learn that the incorrect model was referenced in two prior Peatland-VU publications (Budishchev et al., 2014; Mi et al., 2014). This led to the initial inclusion of the incorrect photosynthesis equation in our submitted manuscript. We have rectified this in the revised manuscript by providing the correct equations, adapted from BIOME3, and reference (Haxeltine and Prentice, 1996b). The PVN model code remains unchanged and is still based on BIOME3 (Haxeltine and Prentice, 1996b). Please find these changes in Sect 2.1.1, L120-L149.

Original comment: The description of root exudates in eq 23-26 seems mostly ok. However, I think the same issue with double counting of the turnover is present here. It is an interesting feature with the spring addition of exudates. The manuscript would however benefit from describing the theory behind this more as well as how the parameters are constrained and how the data for this is obtained. In Eq 26 the growth of RM seems unnecessary to describe the addition of new root biomass.

Response: Thank you for highlighting this equation. We are happy to amend the manuscript to elaborate on the role of root exudates. We have added the following text to the introduction, "Root exudates are a diverse group of organic compounds secreted by plant roots into the nearby soil. The composition and quality of root exudates varies between plant types, leading to the attraction of particular microorganisms, influencing community composition and function, CO_2 (Crow and Wieder, 2005) and CH_4 fluxes (Schipper and Reddy, 1996)" (L62-L67). We have also added the following text: "Exudates develop at a prescribed rate per PFT, dependent on root and shoot growth. Photosynthesis rates are enhanced during spring and summer and are accompanied by the highest levels of root and soil respiration (Högberg et al., 2001). There is strong evidence to suggest that enhanced photosynthesis fuels exudate production, causing seasonal variation in exudation (Saarnio et al., 2004; Whipps, 1990)" at lines 214 – 217.

We have included in the text of the revised manuscript (L217) that the root growth and die off functions are adapted from the Peatland-VU model (van Huissteden et al., 2006). We have amended Eq 16 (L215) so that RM is omitted and we have included a term to describe the root distribution instead. We believe this removes the indication of double accounting. We have elaborated on the variable definitions in the text on lines 207-227.

Original comment: Finally, I would recommend the authors add mass balance to the code to make sure that carbon is not lost through double counting.

Response: We think this is a good idea and whilst formulating the model, we checked the carbon mass balance by combining the results of carbon mass lost and outgoing fluxes. Unfortunately, this check was not included within the model code. We agree with the reviewer that a mass balance would be useful and are working on incorporating this into the model code in future versions.

Major comments

Original comment: Note that LPJ-WhyMe by Wania et al. (2010) and LPJ-GUESS (Smith et al., 2014) are not the same model. Especially the version used by Chaudhary et al. (2020) and earlier work has an advanced peat accumulation scheme, dynamic vegetation and CH_4 dynamics. The statements on lines 55-58 need to acknowledge this.

Response: We are aware of these differences. Thank you for highlighting this. This sentence in the introduction specifically refers to models that include representation of peatlands, dynamic vegetation and CO_2 & CH_4 cycling. LPJ-GUESS (Chaudhary et al., 2020) is a dynamic peatland vegetation model that simulates carbon accumulation but not CH_4 fluxes. LPJ-GUESS (Smith et al., 2014) is a vegetation model that simulates nitrogen cycling but does not simulate peatlands or CH_4 fluxes. We understand the statements on L83-L87 to be representative of the literature.

Original comment: Please provide a table of all the model inputs and their units that are required. I have a bit of difficulty understanding what might be simulated dynamically within the model and what is prescribed through input. This could be added to the supplementary material.

Response: We have provided a table describing all model inputs and their units (Table S4 and Table S5), and the input values used for the Horstermeer and Ilperveld model simulations (Table S6, Table S7, Table S8, Table S9).

Original comment: Under section 2.2 it is stated that the PFTs have bioclimatic limits. Why include such limits in a site-based model where the PFTs are prescribed? Furthermore, none of the included PFTs is strictly evergreen. Please consider changing this language to say 'lifeform' or similar.

Response: The bioclimatic limits do not determine geographic extent but are used by the photosynthesis function (adapted from (Haxeltine and Prentice, 1996b)), and the potential growth function (adapted from (Heijmans et al., 2008)). We have included the following sentence for clarification, "Bioclimatic limits are used by the photosynthesis function (Sect. 2.1.1), and the potential growth function (Eq. 13)" at L110.

It is unclear whether the reviewer means that some PFTs can be either deciduous or evergreen or does the reviewer mean that the evergreen PFTs do litter *some* old (but not all) leaves throughout the year. We have included the following text for clarification of both contexts," For deciduous vegetation, biomass is adjusted to represent the leaf senescence that occurs when daily temperatures fall below minimum tolerated temperatures (Eq. 22). For evergreen vegetation, biomass is adjusted to represent the death of old leaves (Eq. 9).” (L112-L114)

Original comment: I believe that Tables 1 and 2 could be merged. This would provide a better overview of the parameters for each PFT and the reader would not need to know in which table to look for e.g., units. Also, please ensure that parameter names are uniformly used in the text. For instance, Gmax is referred to as both its parameter name, ‘maximum growth’, and Gmax in the equation.

Response: We have merged Table 1 and Table 2 and we thank the reviewer for this suggestion. We have made several small edits to correctly use the parameter names in text.

Original comment: Why start the model description with how the model is initiated? This could be done later after the description of the equations. Please also include a section on model spinup including what forcing data that is used and what steady-state condition you have (or the number of years that you run the spinup).

Response: We appreciate these suggestions. We have moved the model initiation information into the ‘Model calibration’ (Sect 2.4) and ‘Input data’ (Sect 2.7) sections. We have added details regarding the model spin-up, the forcing data and the length of the simulations to the description of the model calibration section.

Original comment: Please move the description of the harvest scheme from section 2.2.2 (competition among PFTs) into its own section. Also, the statement on line 161 that biomass is uniformly distributed is wrong since you use the allometric equations by Smith et al. (2001).

Response: We have created a separate subsection for the harvest scheme (Sect 2.1.7, L 306-L312). We have clarified the phrase ‘biomass is uniformly distributed’ by writing, Living biomass decreases according to the amount of biomass harvested because biomass is assumed to be uniformly distributed with height and is not partitioned into organs (L310).

Original comment: For the soil model in section 2.3.2 it would be interesting with either a conceptual figure or a table describing how the pools are related to each other. Also, the k-values for each pool should be provided.

Response: We have included the SOM, their respective rates of decomposition (k), and their contributing sources in Table S2.

Original comment: The model calibration is well described and follows standard protocols. However, I would like a few sentences on what data was used for calibration. Was the dataset split up into a calibration and a test set or were all data used for model calibration? Also, what parameters were used in the calibration and how were these selected?

Response: Thank you for the suggestion to include these details. We have provided these details in the Model Calibration section (Sect. 2.4) of the revised manuscript and highlighted the calibrated model parameters in Table S4.

Original comment: The model sensitivity test is also well-designed and performed. This scheme should be kept for future testing.

Response: Thank you for this feedback.

Technical comments
General

Original comment: There is sometimes the use of parenthesis when referring to the opposite. For example lines 139-140 "… and increase (decrease) of foliage of taller PFTs …" This is a bit confusing to me, please consider using other expressions such as 'vice versa' or similar.
Response: We have adjusted this.

Original comment: The placement of some references feels odd, for example on line 59. Please review this.
Response: Done.

Original comment: On lines 101-103 the model time-step and simulation duration seems a bit confused. Please consider a different formulation that clarifies this.
Response: Done.

Original comment: In Figure 1, are the different background pictures intended to represent different types of peatlands? In such a case, please add a description/statement of this and how it is reflected in the flowchart.
Response: Done.

Original comment: Please consider using the capital letter delta in differential equations.
Response: Thank you for the opportunity to reflect on the possible notations used for differential equations. Whilst there is no uniform notation for differentiation, a capital 'D' is not often used for partial differential equations. To maintain consistency with partial differential equations, and to follow the notation style used in the original CH4 (Walter and Heimann, 2000) and Peatland-VU (van Huissteden et al., 2006) models, we opt to use a lowercase 'delta' for all differential equations.

Original comment: Please consider changing the heading of section 2.2.1. It mostly describes soil carbon pools. If these are inherited from a parent model, please also provide the reference for this.
Response: By restructuring the methods, this section has been dissolved into Sect 2.1 (The PVN model), Sect 2.1.3 (below-ground production), Sect 2.1.2 (Competition among PFTs). We have added the sentence, "The root growth and die off functions are adapted from van Huissteden et al. (2006)" (L217).

Original comment: The RD function mentioned on line 204 is not described in detail. Please reference the source of this function
Response: We have added a simple representation of the root distribution (Eq 17), and added the sentence "f(z,p) [m−1] represents the exponential root distribution (Eq. 17) (van Huissteden et al., 2006; Walter and Heimann, 2000)" at L300-L301.

Specific
18 Please add a reference for the first statement.
Response: Removed.

36 GHG has not been used before in the text, please spell out
Response: Done.

58 Table Fig. S1? Please clarify what you refer to here, table/figure

Response: Corrected.

74 plot-scale ecosystem competition model
Response: Corrected.

75 process-based plot-scale peatland model
Response: Corrected.

116 Is threshold the correct word here? Should it not be range?
Response: Corrected.

120 Exudates seems to appear twice in this sentence
Response: Corrected.

123 DBD has not been used before in the text, please spell out
Response: Done.

124 The number of what? I assume soil layers but the word is missing
Response: "The number and depths of the site's soil horizons can be adjusted in the soil input file" (L426).

128-129 This introduction of subscripts is done later on as well. One is enough.
Response: This is now done once (L115-120).

236 I believe the word formation should be used instead of storage.
Response: Corrected.

246 I believe NPP is intended instead of NEE.
Response: Yes, this has been corrected.

265-269 This text should be moved to the acknowledgement or data statement.
Response: Done.

We are appreciative of this detailed feedback that greatly improves the quality of our manuscript. We hope that these responses have resolved your concerns.

Sincerely,
Tanya J.R. Lippmann et al.

Dear Andrew,

Thank you for taking the time to review this manuscript. We have used your valuable feedback to explain the motivations for developing the model and how this model may be used for wetland management. We've reflected on the model's parameter space and we will carefully check all equations in the revised manuscript.

Review of Lippmann *et al.*: 'Peatland-VU-NUCOM (PVN 1.0): Using dynamic PFTs to model peatland vegetation, CH_4 and CO_2 emissions' submitted to *Geoscientific Model Development*.

Overview

Original comment: This paper reports on the coupling of a peatland vegetation dynamics model (NUCOM-BOG) with a model (Peatland-VU) that simulates CO_2 and CH_4 dynamics in peat soils. The paper seems suitable for the journal. The model description is reasonably clear (but see later comment). The sensitivity analysis and the assessment of model mechanisms are clearly presented, as are the model-data comparisons. Nevertheless, I do have some reservations about the paper in its current form and recommend that it undergoes reasonably substantive revision before being considered for publication. My concerns are articulated below.

Response: We thank the reviewer for the constructive and positive comments. Please find our replies to your comments below.

Model purpose and model complexity

Original comment: The authors start by saying that CO_2 and CH_4 fluxes to and from peatlands are strongly mediated by plants and that it is necessary to include key plant processes in peatland CO_2-e flux models. Later they suggest that they weren't expecting their new model to outperform the existing Peatland-VU model. For example, on lines 84-85 they say: "All three models (NUCOM, PeatlandVU, and PVN) depend heavily on calibration using (often limited) observational data and for this reason, we do not expect to reproduce observed CH_4 and CO_2 more accurately." (see also lines 367-370). This leads the reader to question the purpose of the new model. The implication in places is that it can help guide restoration efforts, but an arguably over-complicated and over-parameterised model is perhaps not what is needed by wetland managers. The authors do discuss model equifinality and suggest the new model is less prone to this problem than Peatland-VU because it contains fewer optimisable (i.e., non-measurable) parameters. However, the new model contains many parameters/inputs, and measurements for all of these are unlikely to be available for many sites, so the problem of model equifinality must surely remain. If that's the case, I would also question how much the new model can provide insights into the effect of plants on CO_2 and CH_4 fluxes in peatlands beyond what is already known through numerous observational and experimental studies. In short, I'd like to seek a stronger justification for the new model. In writing such a justification the authors may wish to comment on the importance of plants and plant dynamics relative to water table position, which we know is a first-order control on annual emissions of CO_2 and CH_4 from peat soils.

Response:

Thank you for the invitation to better explain our motivations for this work. The model is capable of estimating the greenhouse gas balance in response to environmental changes (changes in temperature or radiation or water levels) or new management decisions (changes in harvest regime or vegetation management) for peatland sites. Therefore, the process understanding derived through model results can serve wetland management by estimating changes in the greenhouse gas balance of peatland sites in response to management decisions. We have highlighted this in the introduction of the revised manuscript (L95-L100).

While the effects of groundwater table on peatland GHG emissions are extensively described (Evans et al., 2021), the impacts of plant type and plant community composition on GHG emissions are less understood (Malmer et al., 2003). With this model we open up the possibility to explore the combined effects of groundwater and plant species composition on GHG emissions. Plant functional types have been found to explain uncertainties in GHG emissions from wetlands in response to warming in a meta-analysis of wetlands exposed to warming (Bao et al., 2023). Changes in vegetation composition have been observed in long running water table manipulation experiments (Peltoniemi et al., 2009; Strack et al., 2006). Generally, sedges and mosses establish during wetter conditions and shrubs and trees develop during dryer conditions. Enhanced Sphagnum growth during warming experiments have been found to outcompete shrubs (Dorrepaal et al., 2006). Changes in vegetation have been accompanied by changes in fungal and bacterial biomass (Jaatinen et al., 2008) as well as decreases in methanogenic and methanotrophic community diversity (Yrjälä et al., 2011). Following changes in plant community composition, changes to CO2 (NPP) have been observed, further impacting root exudation (Ballantyne et al., 2014). Peat mineralization rates were observed to decline as readily decomposable material is already mineralized (Davidson and Janssens, 2006; Dorrepaal et al., 2009). Differences in vegetation composition within the same site and with the same water levels have been observed to lead to differences in CH4 fluxes (Bubier, 2016; Jackowicz-Korczyński et al., 2010). We have included this text, justifying the development of the new model, in the introduction of the revised manuscript (L51-L67).

We have included the sentence, "Vegetation composition change directly impacts litter inputs, which impacts the quality and quantity of fresh SOM contributions (Malmer et al., 2005)" to L230.

In the development of the model, we made steps to minimise the calibratable parameter space. The introduction of PFTs allowed several Peatland-VU parameters that were previously calibratable to become observation-informed parameters, whilst introducing few new parameters, thereby the net result is a reduction in the parameter space. We have included the following text in the discussion: "The introduction of PFTs allowed several Peatland-VU parameters that were previously calibratable to become observation-informed parameters, whilst introducing few new parameters, thereby the net result is a reduction in the breadth of the parameter space." (L669 – L671).

Model equations and explanations
Original comment: I found the subsections in which the model equations are presented quite difficult to follow in a few places. As someone who is not familiar with the parent models, it would have been useful to have had more written information on the equations and their derivation or even some simple plots showing the form of the equations. Several equations also seem to contain errors – some are not dimensionally homogenous and the signs of the terms in others don't make sense (to me, at least). I have indicated some examples in a marked-up copy of the paper supplied separately. I recommend the authors check the equations carefully to ensure they are rendered correctly and are dimensionally balanced.

Response: This point was also raised by reviewer 1 and 2. We have added more detailed descriptions to several of the equations (L120-L137, L155-L166, L121-189, L207-L217, L232-L237, L250-260, L276-L298, L306-L311). We have carefully revised the manuscript to ensure that we define and describe all variables and all equations correctly.

Peat accumulation and water table inputs
Original comment: Some aspects of the new model didn't really make sense to me. It is not clear how net peat accumulation or loss is accounted for in the model. Can the designated soil layers expand and contract as the size of their SOM pools varies? Also, I am confused about why only mosses can apparently contribute to peat formation as suggested in Figure 1 and noted on line 236: "Non-moss PFTs do not contribute to the storage of the peat." If the new model is to be applied over timescales of many decades, then changes in peat thickness and mass should surely be considered. Also, peat properties such as permeability are important at these timescales because they feed back into a peatland's hydrological functioning and the position of the water table which, as noted above, is a first-order control on CO_2 and CH_4 fluxes.

Response: We have amended the manuscript to specify that there is no feedback between peat loss/accumulation and the soil layers (such as dry bulk density, or recalculation of the pF curve) (L185-L189). This is a feature that we plan to incorporate in future versions of the model. This is considerable work and was beyond the aims of this study. The introduction of calculating the thickness of the living moss layer was one first step towards doing this.

Vascular plants have significantly faster decomposition rates than bryophyte plants in both undisturbed and harvested fens (Graf and Rochefort, 2009). Vascular plants are more likely to contribute to SOM pools such as labile organic matter, exudates, microbial biomass, litter, and dead roots and therefore, we have divided their OM products between these belowground pools. We have amended the manuscript to include this explanation (L252-L260).

Original comment: The authors note that site water tables were provided by the Netherlands Hydrological Instrument. This doesn't seem to be an accurate way of obtaining water tables. It would have been better to have measured water tables directly or to have used a site-specific water-table model.

Response: We agree with the reviewer that the use of in situ water table measurements would be ideal. Unfortunately, such measurements are unavailable.

"The NHI is a reasonably high spatial resolution water level product (250mx250m). One aim of developing the PVN model is to eventually develop a model of all Dutch peatlands in conjunction with the NHI product. For this reason, we've used the NHI product in this application of the model. The NHI water level output was converted to relative surface height using a 5m x 5m digital elevation map of the Netherlands, Actueel Hoogtebestand Nederland (Alhoz et al., 2020)." We have added this text (L430-L436).

Original comment: I have made more comments (mostly minor) on a pdf of the paper and this is posted separately for the authors and the editor.

Response: We thank the reviewer for their time. We have read their valuable comments in the attached pdf and have incorporated many of the suggestions into the revised manuscript.

<u>More comments (mostly minor) from a pdf of the paper</u>

L4: and groundwater levels (water table position)?
Response: We removed the word 'ground' from this sentence.

L16: Nicely written abstract – very clear.

Response: Thank you.

L20: ‘is’
Response: Done.

L24: I recommend a bigger stop. A semicolon or a full stop.
Response: Done.

L31: particularly ‘from’ wetlands
Response: Done.

L35: two words when being used as a verb?
Response: Edited.

L40: net emissions or net warming effect? Not necessarily the same.
Response: We have changed this to ‘particularly whether peatlands function as a carbon source or sink’…

L46: Too general a statement. Not true of some bogs, fens, tropical peat swamps.
Response: We have changed to ‘in many peatlands’.

L46: Spelling: ebullition.
Response: Done.

L68: ‘varies’
Response: Done.

L68: Just say exudation
Response: Done.

L69: Just say carbon
Response: Done.

L71: I’m not sure I agree. Some work has shown that CH4 emissions can be predicted quite well using water-table depth alone, across a range of peatland types. It may be worth nothing the relative importance of different controls in the discussion here.
Response: We have added ‘during periods of environmental change’ to the end of this sentence.

L94: punctuation not quite right
Response: Edited.

L95: Some of this section repeats the introduction
Response: To avoid repetition, we have edited this section and also the previous paragraph.

L98: punctuation
Response: Sentence restructured.

L105: It may be worth putting model data requirements into a table
Response: We have included this in the supplementary in Table S4, Table S5, and Table S6, Table S7, Table S8, Table S9.

L119: Does peat development depend only on mosses? Vascular plant litter will also form peat
Response: As we have said above, the introduction of calculating the thickness of the living moss layer was one first step towards doing this. Vascular plants are more likely to contribute to SOM pools such as labile organic matter, exudates, microbial biomass, litter, and dead roots and therefore, we have divided their OM products between these belowground pools. The model has two inert carbon pools. Vascular plants contribute to the slightly faster of these two slow-decomposition pools. These decomposition rates are listed in Table S2. We have included this explanation in the subsection, below-ground SOM decomposition (L250-L260).

L120: it is unclear how these are distinguished from each other.
Response: We have included Table S2 which describes the decomposition rates of the different SOM pools.

L128: Is this always appropriate?
Response: This is an interesting point. In general, 30%, 50%, and 75% of roots are observed in the top 10 cm, 20 cm, and 40 cm, respectively (Jackson et al., 1996). We have provided this reasoning in the methods section (L210-L211).

Land surface models have, for the most part, used exponential relationships to describe root distribution (Smithwick et al., 2014; Zeng, 2001). Root distribution is an important structural representation to reliably simulate CO2 and CH4 fluxes, where processes such as leaf:root allocation, turnover, root stocks, and root distributions have been down to be dependent on climate, species, and land cover type (Smithwick et al., 2014), particularly in Arctic and boreal systems (Iversen et al., 2015). Whilst, linear and dynamic alternatives exist, this research has focused on forested and arid systems (e.g. (Li et al., 2012; Sivandran and Bras, 2012)). Advances have been made developing knowledge and observational data suitable for improving root representation in boreal peatland systems indicating that the exponential relationship is representative for several plant types whilst an alternative root representation to the exponential relationship may be relevant for certain plant types (Clemmensen et al., 2013; Iversen et al., 2015). Future model versions may consider introducing alternative root representations for some plant types. We have expanded the text in the discussion where we already discuss root representation limitations (L759-L769) and renamed Sect 4.1.6, 'Root distribution representation'.

L130: Is the model capable of simulating net peat accumulation and loss? Or is the soil profile kept at constant mass? Or constant thickness but variables mass?
Response: The model simulates net peat accumulation and loss using peat SOM reservoir. The soil layers (depth, thickness, DBD) are kept constant throughout the simulation. We have amended the manuscript to reflect these distinctions (L247-L250).

L131: is
Response: Done.

L133: What does this mean? Is it the fraction of above ground biomass that becomes litter (dies) per day?
Response: Yes. We have re-worded this in the revised manuscript.

L139: This style of writing is clunky.
Response: We have adjusted this.

L150: Punctuation not quite right
Response: Adjusted.

L154: Equation is not homogeneous. Terms on RHS have different dimensions.
Response: This was also mentioned by reviewer 2. This equation has been modified (also for other reasons). However, the relevant component remains in the numerator of Eq 9:
d/dt(CB) = SM – BS * CB
where the units of SM and NPP are both Kg C m-2 day-1.

L158: Should this be ratio of shoot to total (shoot plus root) growth?
Response: There are different ways of partitioning shoot and root growth. Here, allocation of root and shoot growth is a fixed fraction of NPP so that the fraction of shoot and root growth sums to 1.0. We have amended Table 1 to clarify this.

L168: wording not quite right.
Response: Adjusted.

L187: flux is a rate. Just say flux.
Response: Adjusted.

L190: Are these signs correct? Why do production and removal have positive signs?
Response: This was also mentioned by the second reviewer. We have corrected the +/- signs in this equation (now Eq 29).

L200: Style is a little clunky
Response: Adjusted

L202: conduct
Response: We feel conduit is the most suitable word here.

L222: italics?
Response: This is a maths symbol.

L231: repetition of 'where'
Response: Removed.

L236: Why not? This seems like an important limitation
Response: As we have clarified in an above response, the introduction of calculating the thickness of the living moss layer was one first step towards doing this. Vascular plants are more likely to contribute to SOM pools such as labile organic matter, exudates, microbial biomass, litter, and dead roots and therefore, we have divided their OM products between these belowground pools. We have clarified our reasoning in the revised manuscript.

L252: what is this?
Response: The term f(KSP) [-] is a function depending on PFT constant, KSP (Table 1), that can be used to determine stronger exudation occurs during spring. We have amended the manuscript to clarify this (L228).

L253: Just day of year.
Response: Amended.

L268: I'm not sure this information is needed unless the journal requires it.
Response: Moved.

L275: nutrient-rich
Response: Adjusted.

L321: photosynthetically active
Response: Adjusted.

L326: But after you had already optimised/calibrated the parameters? Also you already introduced the 2 sites so you could just end the sentence halfway.
Response: Yes, these are the results of the calibrated model simulations. Unfortunately, we do not have enough observational data to separate the dataset into a calibration set and a test set. We have included the text, "Unfortunately there was not enough data to split the observational data into separate datasets for calibration and validation."

We have ended the sentence halfway as suggested (L386).

We have reworded the sentence, "In order to demonstrate that the PVN model reproduces CH4 and CO2 fluxes within the spread of observed fluxes when driven by realistic input data, we compared the calibrated model simulation results and measured CH4 and CO2 fluxes for two sites, the Horstermeer and the Ilperveld field sites." (L386 – LL388)

L334: Los Gatos make several types of analyser. Say which one you have.
Response: We have added that this was the the Ultra-Portable Los Gatos Gas Analyser Model 915-001 to the revised manuscript.

L335: There aren't standardised flux chamber protocols – many protocols exist
Response: We have removed this reference.

L335: cylindrical?
Response: We have added this detail to the revised manuscript (L393).

L339: unclear. Were measurements taken every hour?
Response: We have clarified this text.

L360: This doesn't seem like a very accurate way of measuring water levels. Were any water-table wells installed at the sites?
Response: As above: We agree with the reviewer that the use of in situ water table measurements would be ideal. Unfortunately such measurements were unavailable. The NHI has a reasonably high spatial resolution (250mx250m). One aim of developing the PVN model is to eventually develop a model of all Dutch peatlands in conjunction with the NHI product. For this reason, we've used the NHI product in this application of the model. We have included this explanation in the revised manuscript (L430-L437).

L370: This seems like an odd thing to say. If plants are important than surely the new model should outperform the old one.
Response: We understand that both models do a good job reproducing the observed fluxes. Both simple and complex models can perform well reproducing observations. We have developed a model capable of reproducing the observed fluxes whilst enhancing functionality and the representation of processes. We have removed this sentence.

L419: Should this say the other simulations?
Response: Yes. Adjusted.

L420: A negative water table depth implies a water level above the surface.
Response: We have changed this to 'water level'.

L463: axes labels and numbers very small

Response: We have corrected this.

L476: was
Response: Done.

L546: inconsistency in font size on y axes
Response: We have corrected this.

L598: OK good point. However, in absolute terms, is the model still over paramerterised? Why not start from the bottom upwards and build a model that is only as complicated as it needs to be? What can the new model be used for?
Response: The introduction of PFTs allowed several Peatland-VU parameters that were previously calibratable to become observation-informed parameters, whilst introducing few new parameters, thereby the net result is a reduction in the breadth of calibratable parameter space. We have produced a model capable of reproducing the observed fluxes whilst improving functionality and the representation of processes to allow for the investigation into the impacts of environmental processes on vegetation community composition. (L669-L671)

L619: Punctuation not quite right here.
Response: Adjusted.

L639: do mosses have roots?
Response: Whilst, mosses do not have root structures in reality, we allocated moss PFTs to have a presence in the top 10cm of the soil layer because in the presence of bryophytes, there is often no clear separation between the living moss layer and the soil surface. In this way, we intended to replicate a transition zone. We have added this text to the revised manuscript (L714-L716).

L641: This sentence could be clearer.
Response: We have amended this sentence to make it clearer.

L652: style is clunky.
Response: Adjusted.

L661: Not a sentence
Response: Re-written.

L677: of
Response: Changed.

L681: comma not needed
Response: Removed.

L743: How can the new model be used to guide policy?
Response: We have included this sentence: By including plant-environmental feedbacks, this model can be used to understand how peatland GHG emissions respond to environmental change (L826-L827).

We again thank you for your valuable feedback which greatly improved the quality of our manuscript. We hope that these responses have resolved your concerns.

Sincerely,
Tanya J.R. Lippmann et al.

References

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