Response to Referee #1(Benjamin Stocker)'s comments

We would like to thank Benjamin Stocker and the anonymous referee very much for their constructive comments. In the following, please find our point by point response to the comments.

- Reviewer's comments are in bold
- Modifications done in the revised manuscript are in blue
- All figure numbers, table numbers, and line numbers refer to the initial manuscript version.

Stocker (Referee)

This paper presents and evaluates a global model that simulates the spatial extent of peatlands and their C balance as a function of the environment. The peatland model is implemented as a module within the comprehensive land surface model ORCHIDEE. This is an important addition to this model as it allows to account for the effect of peatlands on the global carbon cycle, which is particularly important for long-term simulations, covering multiple centuries to the millennial time scale. The approach for simulating the spatial dynamics of peatlands across the globe is largely adopted from Stocker et al. (2014) GMD [thereafter referred to as ST14]. I don't want to hide the fact that this is my own work and that I am pleased to see that it has stimulated other researchers to follow the same approach.

The paper by Qiu et al. goes a step beyond ST14 in that it evaluates the model not only by its accurateness in simulating the spatial patterns across the globe and the total northern peatland C storage, but it evaluates peat depth using information from a set of 102 peat cores, distributed across the northern hemisphere (mostly in the boreal zone), and deals with the challenge of accurately simulating the history of peat C accumulation throughout the Holocene, which adds substantial complexity. This work is also a substantial advancement in simulating wetlands and the distribution of flooding. Their comparison to a new observation-based dataset by Tootchi et al. (2018) shows a very good agreement (Fig. S7 - worth including this in the main text?), and seems to suggest that their model works much better in this respect than, e.g., the model presented in ST14. This in itself is a very useful innovation. I was also intrigued by the clever approach to simulate vertical growth of peat as an effective downward transport of soil C (down along the soil profile, across the 32 layers resolved by the model). This is a very useful innovation beyond the models resolved by ST14 and Kleinen et al., 2012. I think this work can be a very valuable addition to the literature and that the model presented here will be a useful addition to the very small set of comparable models available today (only two models, as I am aware). However, before getting there, I would like to see a few critical (MAJOR) issues addressed. I also think that the paper could gain from a clearer presentation in general. Below, I'm listing specific points. I hope the authors find my suggestions useful and I am looking forward to a revised

version of the manuscript, and possibly a revision of the model and evaluation.

We thank the reviewer for his thorough reading of this manuscript and encouraging comments. We include Fig. S7 in the main text of the revised manuscript. Please see below our detailed response to comments.

MAJOR

Q1. * The code is not accessible under the given URL. Although it's not officially required by GMD, I personally try to resist to accept model description papers without having open access code. I also think that the model should be easily reproducible in a simplified setup (without having to run the entire ORCHIDEE) and instructions should be available to do so. Plug and play! Please make an effort to achieve this, it is greatly appreciated by the community and helps science to move forward (and it pays off for you).

The source code is freely available and accessible via the following address: https://forge.ipsl.jussieu.fr/orchidee/wiki/GroupActivities/CodeAvalaibilityPublication/ORCHIDEE_PEAT_V2

Moreover, we agree with the reviewer that a simplified version of the model using some kind of emulator will be helpful for interested readers. However, the ORCHIDEE-PEAT model simulates both carbon and area dynamics of peatland, which consists of the following hydrological and biogeochemical processes and their interactions (non-exhaustive): 1. Physically-based soil water flows and soil moisture constrain area development of peatland. Meanwhile, peatland receives water input from surrounding mineral soils, increases soil water storage and reduces runoff of the grid cell, thus exerts a feedback effect on soil water dynamics; 2. Soil moisture limits phenology, photosynthesis, transpiration and soil thermics, which in turn impact the water cycle; 3. Soil hydrology and soil thermics impact litter and soil carbon decomposition, while the long-term C balance of the peatland limits peatland area development. All those mechanisms feedbacks on each other and the design of an emulator will be a research project as itself.

Q2. * What the paper/model does not tackle/resolve, goes unmentioned. No tropical peatlands are simulated (?) nor evaluated. Are methane emissions from peatlands not resolved by the model? How does peat vs. mineral soil affect the extent of frozen soils (permafrost!)? The evaluation of inundation, particularly its timing is missing (or hidden in the SI).

We didn't simulate tropical peatlands in this study, because the model is parameterized and calibrated for northern peatlands. To clarify this point, we add sentences on Line657: "Being parameterized and calibrated for northern peatlands, our model can't be used for tropical peatlands. For tropical peatlands, the model needs to be improved to represent its tree dominance, oxidation of deeper peat due to pneumatophore (breather roots) of tropical trees, and the greater water table fluctuations as a result of the higher hydraulic conductivity of wood peats and tropical climates (Lawson et al., 2014). In addition, tropical peat is formed as riparian seasonally flooded wetlands with water coming from upstream river networks, whereas the TOPMODEL equations used here implicitly assume a peatland is formed in a grid cell only from rainfall water falling into that grid-cell.".

The methane module was not activated in this study because it has not been updated and evaluated since many years. We informed readers that methane emissions are not resolved by the model on Line484-485: "CH_4 and dissolved organic carbon (DOC) are not yet included in the model, both of them are significant losses of C from peatland (Roulet et al., 2007).". And then on Line660-661, we recalled the necessity of including methane and DOC emissions from peatland to draw a more complete picture of peatland C budget: "Including CH_4 emissions and leaching of DOC will be helpful to get a more complete picture of peatland C budget.". Actually, in parallel with this study, two projects are ongoing in our group to model CH_4 and DOC fluxes from northern regions with ORCHIDEE.

The model resolves one energy budget for all soil tiles in one gridcell, with soil thermal properties of the gridcell being defined as a weighted average of mineral and organic soil (organic soil fraction is prescribed from NCSCD in permafrost regions and from HWSD in non-permafrost regions) (Guimberteau et al., 2018, GMD). In the model, dynamics of peat vs. mineral soil will only affect soil temperature (and permafrost) indirectly: changes of peat vs. mineral soil in the grid cell impacts gridcell soil water content, then gridcell soil water content and water filled fraction of pores impact fusion and solidification heat fluxes in the soil; changes in soil moisture and its liquid/ice state also impact soil thermal conductivity.

Calibrated by the CW-WTD wetland map (Sect. 2.2.1), we compared simulated maximum inundation area of the Northern Hemisphere with CW-WTD in Sect. 4.2 (Fig. S7), on Line404-411. Now, Following the reviewer's suggestion, we move Fig. S7 to the main text. CW-WTD can't be used to evaluate timing of inundation because CW-WTD is a static wetland map. Therefore, in the following figure, we use GIEMS to evaluate inundation timing (Prigent et al., 2007 and 2012, JGR). Note that because wetland extent in GIEMS (the maximum wetland area for the northern hemisphere over 1993-2007 being ~7 million km^2 , with lakes are included) are much smaller than in CW-WTD (~13.2 million km^2 after excluding lakes) (Tootchi et al., 2019, ESSD), we normalize the data by dividing the simulated and observed total inundated area of each month by the simulated and observed maximum monthly value, respectively, to highlight seasonality of inundation rather than comparing absolute values. Accordingly, the following discussion is added on Line411: "…The model generally captures the spatial pattern of wetland areas represented by CW-WTD (Fig. 5). The multi-sensor satellite-based GIEMS dataset (Prigent et al., 2007, 2012) which provides observed monthly inundation extent over the period of 1993 − 2007 is used to evaluate simulated seasonality of inundation. Fig. 6 shows that the seasonality of inundation is generally well captured by the model, although simulated seasonal maximum of inundation extent occurs earlier than observations (except in WSL) and simulated duration of inundation is longer than observations.".

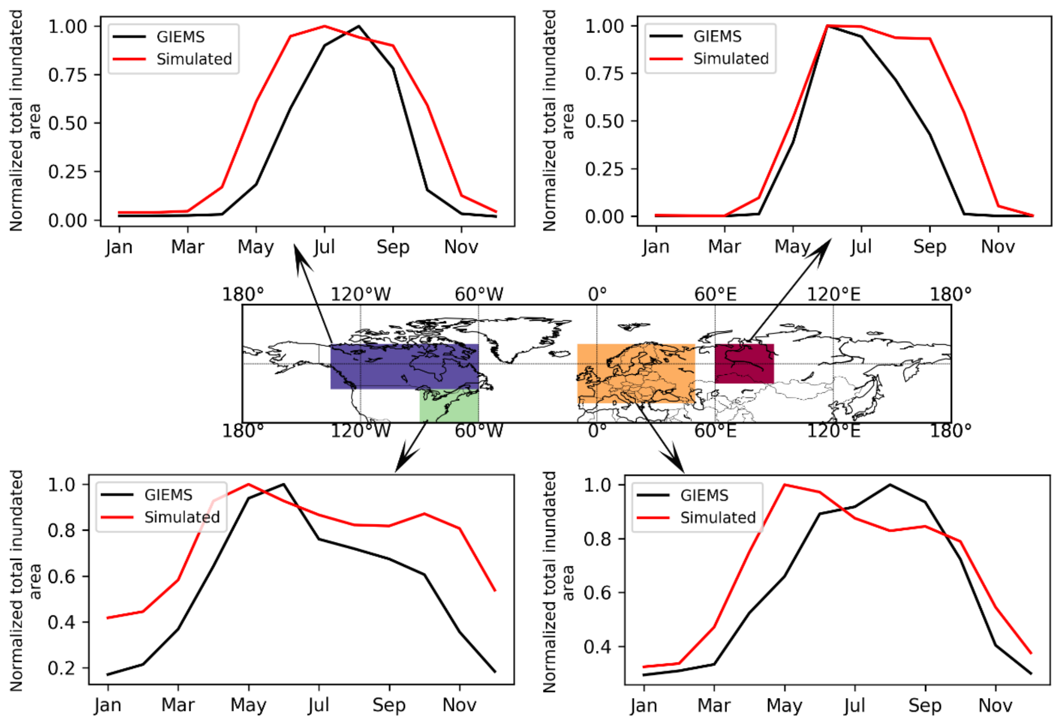


Fig. 6. Simulated and observed (GIEMS, (Prigent et al., 2007, 2012)) mean seasonality (averaged over 1993–2007) of total inundated area. Note that the simulated and observed total inundated area of each month is divided by the simulated and observed maximum monthly value, respectively, to highlight seasonality of inundation rather than comparing absolute values of inundated area.

Q3. * The simulated distribution of the peatland area fraction (Fig. 4) shows that the model is able to broadly capture the observed pattern, except that is quite strongly underestimates the peatland extent in the Hudson Bay Lowland (HBL). This reminds me of my own work, where the first version of my model (DYPTOP, ST14) also failed to simulate very high peatland area fractions (over 90%) across this large region. The HBL is, next to the West Siberian Lowland, the largest peatland region and therefore warrants special attention. The failure of the model by Qiu et al. in simulating large peatland fractions may be related to what one may call the "sponge-feedback" – the high efficiency of organic soils in retaining water (small runoff) which in turn increases persistency of flooding and the suitability for peat to accumulate - a positive feedback. I solved this by having (gridcell average) soil parameters that determine the soil hydrology depending on the internally simulated peatland area fraction, rather than using externally prescribed parameters from soil maps. I see that in the present model, some soil parameters are indeed prescribed for each gridcell separately from external data (soil bulk density, soil C fraction; l. 499). I would say that they should be affected by whether the model simulates peatland in the respective gridcell or not. This might be something worth looking into in order to better reproduce the observed Hudson Bay Lowland peatland area fractions. On l.131, it's mentioned that soil thermal (and hydrological?) properties are a weighted average of

mineral and organic soils, where organic soil fraction is prescribed from an external dataset (NCSCD and HWSD).

Actually, the “sponge-feedback” was considered in the present model. In the model, each grid cell is divided into four independent sub-grid hydrological soil unit (HSU): one for bare soil, one for all tree PFTs, one for all short vegetations and one for peatland. The peatland HSU is parameterized with peat-specific hydrological parameters (large porosity, large saturated hydraulic conductivity), while hydrological parameters of other non-peatland HSUs are determined by the dominant soil texture (Coarse/Medium/Fine) of the grid cell. This is described on L114-120: “ORCHIDEE-PEAT version 1 was evaluated and calibrated against eddy-covariance measurements of CO_2 and energy fluxes, water table depth, as well as soil temperature from 30 northern peatland sites (Qiu et al., 2018). Parameterizations of peatland vegetation and water dynamics are unchanged from ORCHIDEE-PEAT version 1: …… Vertical water fluxes in peatland tile is modelled with peat-specific hydraulics (Text S1 in the Supplement).”

As for the underestimation of peatland extent in the Hudson Bay Lowland (HBL), Glaser et al. (2004a and 2004b, Journal of Ecology) and Packalen et al. (2014, nature communication) proved that climate alone couldn’t explain the initiation and development of peatlands in the HBL, the glacial isostatic adjustment is a more fundamental control of HBL peatlands development. We add sentences on Line434 to address this issue: “……, though the ~~hotspot~~ world’s second largest peatland complex at the Hudson Bay lowlands (HBL) is underestimated and a small part of the northwest Canada peatlands is missing. Packalen et al. (2014) stressed that initiation and development of HBL peatlands are driven by both climate and glacial isostatic adjustment (GIA), with initiation and expansion of HBL peatlands tightly coupled with land emergence from the Tyrrell Sea, following the deglaciation of the Laurentide ice sheet and under suitable climatic conditions. The pattern of peatlands at southern HBL was believed to be driven by the differential rates of GIA rather than climate (Glaser et al., 2004a, 2004b). More specifically, Glaser et al. (2004a, 2004b) suggested that the faster isostatic uplift rates on the lower reaches of the drainage basin reduce regional slope, impede drainage and shift river channels. Our model, however, can’t simulate the tectonic and hydrogeologic controls on peatland development. In addition, the development of permafrost at depth as peat grows in thickness over time acts to expand peat volume and uplift peat when liquid water filled pores at the bottom of the peat become ice filled pores (Seppälä, 2006). This process is not accounted for in the model and may explain why the HBL does not show up as a large flooded area today whereas peat developed in this region during the early development stages of the HBL complex.”.

Unlike the configuration of the model for hydrology, which calculates water budget for each HSU independently. The model can only calculate one energy budget for all HSUs in one grid cell, soil thermal properties are indeed a weighted average of mineral and organic soils (with organic soil fraction being prescribed from NCSCD and HWSD) (Guimberteau et al., 2018, GMD).

Q4. * The explicit depth-dependence of the turnover rates is a bit obscure to me. While the rationale is defensible (l. 160 "priming effects, sorption of organic molecules to mineral surfaces"), it's not clear how important this factor is for the simulations here. Couldn't it be avoided? What's the e-folding scale in Eq. 2? (I see that the z_0 parameter is given later in the manuscript) And shouldn't this be accounted for by oxygen conditions, being subject to water content in different layers where the bottom layers will tend to be water-logged and thus have a very low turnover rate. From text S3, this is not evident.

We understand that in reality, bottom layer of peatland tends to be water-logged and water content of upper soil layers change with time due to the fluctuating water table. While the model resolves water diffusion between soil layers according to the Fokker–Planck equation, shapes of simulated soil moisture profiles depend on soil texture (hydrological parameters), amount and frequency of water input (snowfall, rainfall, runoff from non-peat soils) and water output (evaporation, transpiration, sublimation), water diffusion rates, etc. The figure below shows daily water inputs to a Sweden peatland (68.0°N, 19.0°E) in year1884 and the simulated daily volumetric water content profile for the peat HSU. Simulated soil water content at bottom soil layers are smaller than that at upper layers from Julian days90 to Julian days140, and bottom layers never reach saturation. So, the water content alone can't represent anoxic conditions of peat soil profile.

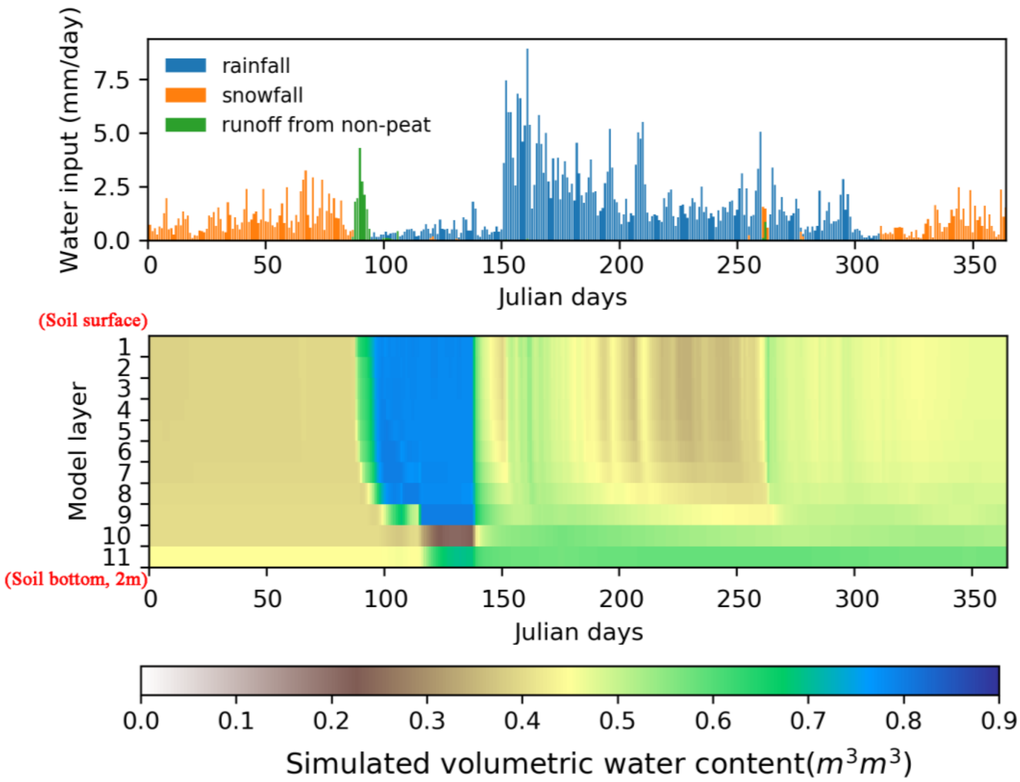
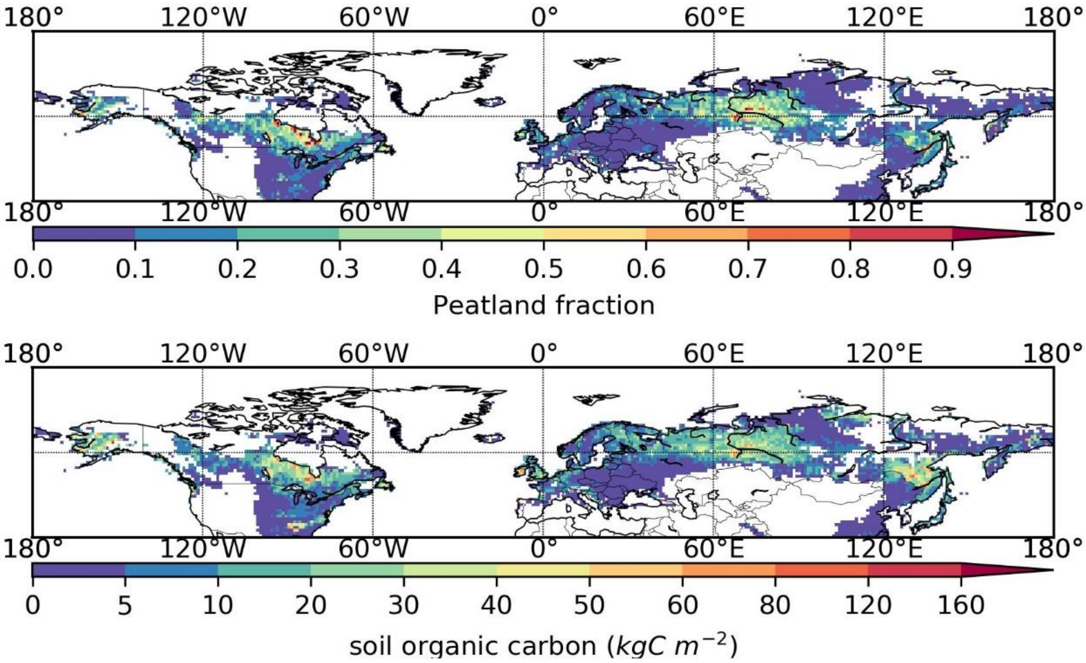


Fig. S13. (Top figure) Daily water inputs to a Sweden peatland (68.0°N, 19.0°E) in year 1884; (bottom figure) simulated daily volumetric water content profile for the peat HSU.

Without the depth modifier, as shown in the figure below, simulated northern peatlands area will not change (3.9 million km^2), but northern peatlands C stock will be underestimated (only 300PgC). We acknowledge that such kind of approach is somehow too empirical but at this stage we can't avoid it. These limitations were presented on Line602: "……The parameter z_0, by contrast, exerts a relatively strong control over C profiles. It is noteworthy that while our model resolves water diffusion between soil layers according to the Fokker–Planck equation (Qiu et al., 2018), simulated soil moisture does not necessarily increase with depth (Fig. S13). z_0 is therefore an important parameter to constrain peat decomposition rates at depth. With smaller z_0, decomposition of C decreases rapidly with depth, resulting in deeper C profile (Fig. S14). Regional scale tests verified these behaviors of the model: When f_{th}=0.9 is used (instead of f_{th}=0.7), changes in peatland area and peat C stock are negligible (Fig. S15). Without z_0, simulated northern peatlands area will not change (3.9 million km2), but northern peatlands C stock will be underestimated (only 300PgC). If z_0=0.5 m is applied (instead of z_0=1.5 m), the simulated total peat C would triple while the total peatland area would only increase by 0.2 million km2 (Fig. S16)."



(Top figure) Simulated peatland area fraction without the depth modifier (z_0), and (bottom figure) simulated peatland soil carbon density without z_0.

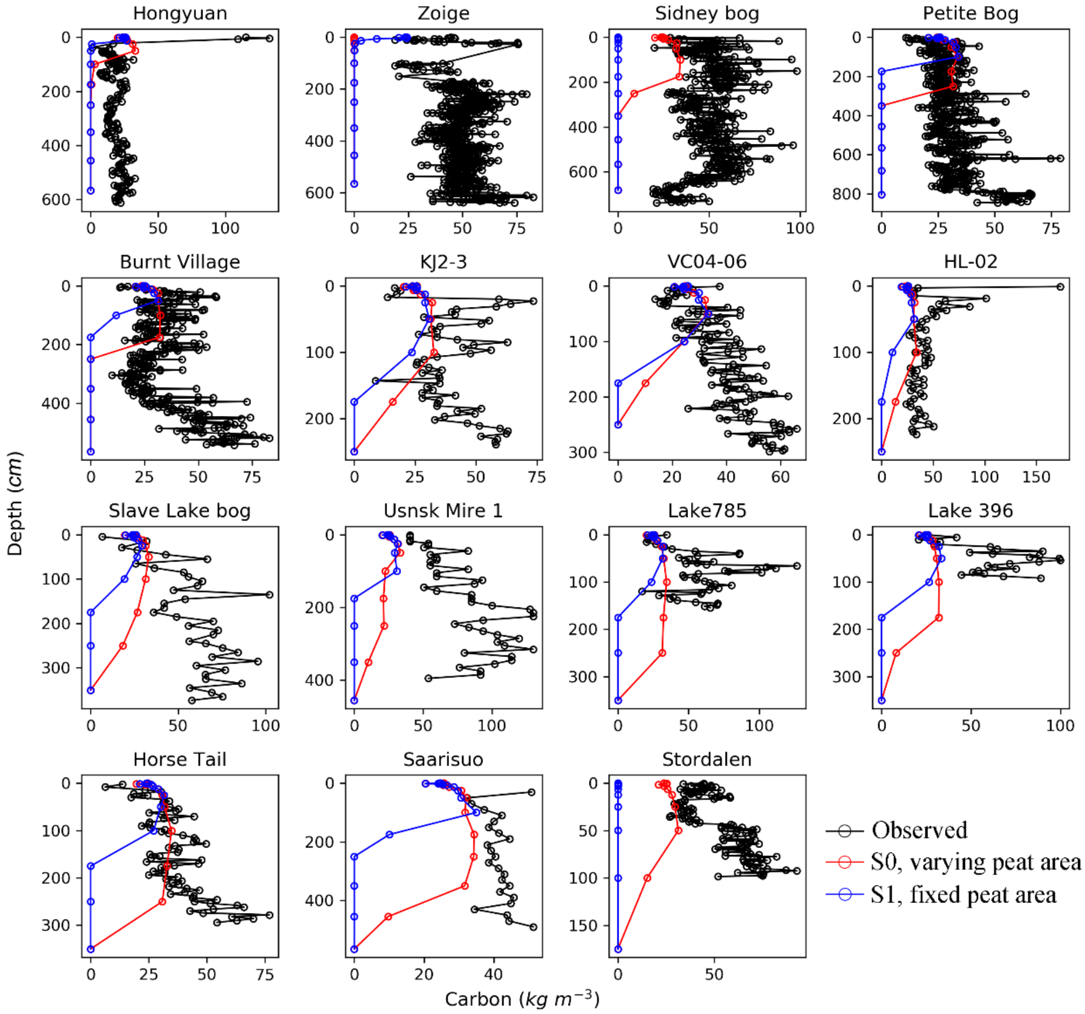
Q5. * Comparison with cores. I am not sure if the model presented here can be compared to peat cores. The reason is that, in order to conserve C mass, an expansion of the peatland area fraction has to imply a reduction of the peat C mass per unit area - peat C is effectively diluted over an increasing area. Hence, the vertical growth of peat should slow upon lateral expansion. This is implied by the simplification that the model doesn't explicitly simulate the horizontal dimension. In reality, a peatland has substantial lateral structure and tends to be deep and have the oldest layers towards the center. That's also where peat cores are commonly taken (in order to maximise the temporal coverage). I am

therefore not surprised to see that the model appears to generally underestimate peat depth. I suspect that separate simulations are required for this, where the peatland area fraction is held constant (no dilution!).

We agree with the reviewer that the expansion of peatland area fraction may dilute simulated peat C. Following the reviewer's suggestion, we run simulations with fixed peatland area fraction (with peatland area fraction in each grid cell being derived from the map of Yu et al. (2010, GRL)). However, as shown in the figure below, simulated peat C profiles with varying peatland area fraction (S0 in red line) match better (than S1 with fixed peatland area fraction, in blue line) with observations (black line). This can be due to: (1) in S0, the simulated peatland area fraction is quite small at first, and then it increases gradually. As we add surface runoff of all non-peatland soils in the gridcell into peatland, with a smaller peatland area fraction, S0 tends to create wetter peat soils than S1. (2) in S0, peatland encroach C from non-peatland soils when expanding, and the C is protected from oxic decomposition subsequently. Point (2) will be presented in a follow up study (Qiu et al., in prep). The below figure is not added in the revised manuscript, for simplicity, we add a section in discussion on Line543 in the revised manuscript to note the dilution issue:

Vertical profiles of peatland soil organic carbon

We note that caution is needed in interpreting the comparison between simulated peat C profile and measured C profile from peat cores (Fig. 3, Fig. 4). In reality, peat grow both vertically and laterally since inception, with the peat deposit tend to be deeper and its basal age tend to be older at the original nucleation sites / center of the peatland complex (Bauer et al., 2003; Mathijssen et al., 2017). As mentioned earlier, field measurements tend to take samples from the deeper part of a peatland complex and shallow peat are underrepresented. The model, however, only simulates peat growth in the vertical dimension and lacks an explicit representation of the lateral development of a peatland in grid-based simulations, thus simulated peat C (per unit peatland area) is diluted when the simulated peatland area fraction in the grid cell increases.



Q6. * The authors aim to model peat C dynamics during the Holocene (see title), but relatively little focus is given to forcing and evaluating the model with respect to this palaeo perspective. As far I understood, the model is forced with constant pre-industrial climate (although insolation and summer temperatures varied substantially during the Holocene, especially at high latitudes). Was a changing sea level accounted for? For applications in palaeo climate and -carbon cycle studies, the model is expected to reliably simulate the net C balance of peatlands. I am not convinced that the evaluation of C content across the soil profile, as presented in the paper, provides sufficient information to evaluate this aspect. Shouldn't a comparison be done against dated peat cores, where the amount of C (left today) per age bin is given? The model doesn't track age bins explicitly, but could be extended to simulate C14 decay and transport across the soil layers (so that lower layers would have an older C14 age, which could then be compared to the C14 age across depth in dated cores). Alternatively, one could write out soil C inputs and decomposition rates at all time steps and resolve age cohorts explicitly offline (diagnostically). I understand that this is a substantial challenge, but I am not fully convinced that the evaluation presented here is sufficient. At least a discussion of these points should be added.

Considering that there are significant variations in both proxy-based reconstructions of Holocene climate and climate models simulated Holocene climate by models, and a significant model-data discrepancy exists (Mann et al., 2008, PNAS; Liu et al., 2014, PNAS), we simply used looped 1961-1990 climate in this study to approximate the higher Holocene temperatures relative to the ‘pre-industrial’ period (Marcott et al., 2013, Science). Uncertainties induced by the climate forcing has been discussed on Line532-540, and one of our future work is to study impacts of different Holocene climate forcing data.

ORCHIDEE is a land surface model simulating CO_2, water and energy fluxes of terrestrial ecosystems. It is the land component of the IPSL-CM5 (Atmosphere-Land-Ocean-Sea ice) earth system model. In this study, ORCHIDEE-PEAT was run offline, sea-level changes were not accounted for. But changes in the exposed land area after the retreat of ice sheet were considered (see Sect. 3.2).

The reviewer is right that we can’t compare simulated peat C profile against dated peat cores because our model doesn’t track age bins explicitly. Tifafi et al. (2018, GMD) incorporated 14C dynamics in the soil into the ORCHIDEE-SOM model. Their work is in parallel with our model, but could be merged together in the future developments. A discussion on this issue is added following Q5: “……, thus simulated peat C (per unit peatland area) is diluted when the simulated peatland area fraction in the grid cell increases. In addition, while a dated peat core tells us net burial of peat C during time intervals, the model can’t provide a peat age-depth profile because it simulates peat C accumulation based on decomposition of soil C pools, rather than tracking peat C as cohorts over depth/time (Heinemeyer et al., 2010).

The above-noted discrepancies between the simulation and the observation highlight both the need for more peat core data collected with more rigorous sampling methodologies and the need to improve the model. In parallel with this study, ^{14}C dynamics in the soil has been incorporated into the ORCHIDEE-SOM model (Tifafi et al., 2018), which may give us an opportunity to compare simulated ^{14}C age-depth profiles with dated peat C profiles in the future after being merged with our model.”.

Q7. * I simply did not understand Fig. 1.

Fig.1 was used to show the RMSE of simulated and measured peat depth at 60 peatland sites. The transition from green to white indicates the measured mean depth of all 60 sites. Since information showed by this figure was already informed by Table1, we replaced it with a figure for modelled vs. observed depth at these sites. And the text from Line369 to Line 372 were modified accordingly: “…… Peat depths are underestimated for most sites (Fig. 1). Simulated depth of these 57 sites ranges from 0.37 m to 6.64 m and shows a median depth of 2.18 m, while measured peat depth ranges from 0.96 to 10.95 m, with the measured median depth being 3.10 m (Table 1). The root mean square error (RMSE) between observations and simulations is 2.45 m.”

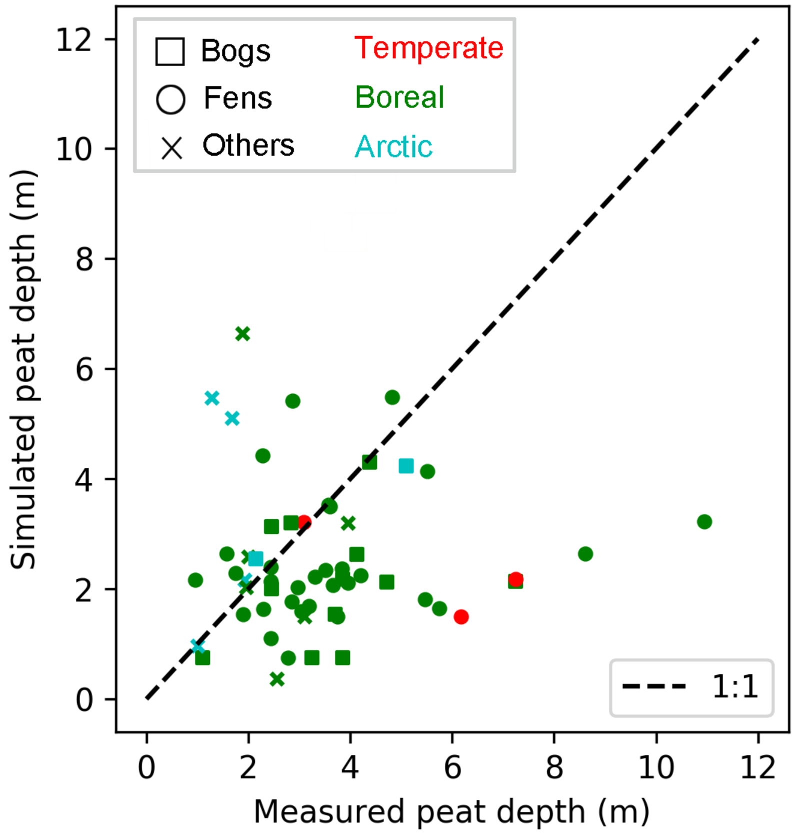


Fig. 1. Measured and simulated peat depth at 60 peatlands sites (Table S1). Shapes of markers indicate peatland types (bogs, fens, others), colors of markers imply climatic zones (temperate, boreal, arctic) of sites' location.

Q8. * Should become clear upfront what parameters are calibrated and what observational targets are used for calibration.

We add the following table to show parameters used and calibrated in the model. We generally described the model parameter, its calibration and observational target simultaneously in the main text. To retain readability (and not having to add redundant descriptions), we keep these descriptions as presented in the initial manuscript.

Table 1. Parameter values in peatland modules of ORCHIDEE-PEAT v2.0.

Parameter	Value	Description
$k_{0,i}$		the base decomposition rate of carbon pools, Eq. 1
$k_{0,i}$: i=*active*	1.0 a^{-1}	the base decomposition rate of the active pool at 30 °C, Eq. 1
$k_{0,i}$: i=*slow*	0.027 a^{-1}	the base decomposition rate of the slow pool at 30 °C, Eq. 1
$k_{0,i}$: i=*passive*	0.0006 a^{-1}	the base decomposition rate of the passive pool at 30 °C, Eq. 1
z_0	1.5m	The e-folding depth of base decomposition rate, Eq. 2
f	0.1	The fraction of carbon content in the model layer to be transported to the layer below, Eq. 4
f_{th}	0.7	The amount (fractional) of carbon content that the model layer need to hold before transporting carbon to the layer below, Eq. 5
m	gridcell specific	TOPMODEL parameter (the saturated hydraulic conductivity decay factor with depth), Fig.1, TextS4
CTI_{min}	gridcell specific	TOPMODEL parameter (the minimum CTI for floodability), Fig.1, TextS4
Num	gridcell specific	The total number of growing season months in the preceding 30 years, Fig.1, Sect. 2.2.2
SWB	6 cm	Minimum summer water balance, Fig.1, Sect. 2.2.2
C_{lim}	50.3 kg C m^{-2}	Minimum peat C density , Fig.1, Sect. 2.2.2

LESS MAJOR (BUT NOT MINOR)

Q9. * Better define the scope of the model and the evaluation, the scale at which the model is expected to yield reliable results, what simplifications have taken to get there, and where the model is not applicable. This can be achieved by more clearly stating upfront for what research questions the model is expected to be applied, and what it therefore needs to simulate with fidelity (and why these quantities). And then present the results with a focus and structure to address these quantities. This is largely done so already, but it would greatly help the reader to improve the structure of the paper in this sense. I would expect the following key quantities:
*** total (northern) peat C: ok**
***spatial patterns of peatland extent: ok, although the particularly extensive peat area in the Hudson Bay Lowland is largely missed by the model.**
*** basal age/inception, compared to first year of peatland establishment in model: It would be good to evaluate simulated and observed basal ages across space, e.g. with a map showing the simulated basal age across space and dots on top of it for observed basal ages from different cores.**
*** peat C accumulation/respiration history: The net C balance through time is what is relevant for the C cycle (what the atmosphere "sees"). I am not convinced that the evaluation presented here, looking at C content across depth, is giving us the right information to evaluate the model in this respect. The dimension time is missing (as mentioned above); there is no age scale of the cores factored into the analysis.**

We revise the summary on Line657-Line661 to make the scope of the study clearer for readers (not stating upfront because we don't want to cause confusion to readers, they need to get an idea of the model and the simulation protocol first): "As a large-scale LSM which is designed for large-scale gridded applications, ORCHIDEE-PEAT v2.0 cannot explicitly model the lateral development of a peatland. The model therefore aims to simulate average peat depth and C profile in a grid location rather than capturing peat inception time and age-depth profiles of peat cores. For tropical peatlands, the model needs to be improved to represent its tree dominance, oxidation of deeper peat due to pneumatophore (breather roots) of tropical trees, and the greater water table fluctuations as a result of the higher hydraulic conductivity of wood peats and tropical climates (Lawson et al., 2014). In addition, tropical peat is formed as riparian seasonally flooded wetlands with water coming from upstream river networks, whereas the TOPMODEL equations used here implicitly assume a peatland is formed in a grid cell only from rainfall water falling into that grid-cell. Further work to improve this simulation framework is needed in areas such as an accurate representation of the Holocene climate, higher spatial resolution, distinguish bogs from fens to better parameterize water inflows into peatland. Including CH4 emissions and leaching of DOC will be helpful to get a more complete picture of peatland C budget.".

Questions concerning these quantities had already been disclosed previously (or later) by the reviewer, please see our responses to Q3, Q6 and Q13:

*spatial patterns of peatland extent in the HBL – Q3
* basal age/inception – Q13
* peat C accumulation/respiration history – Q6

Q10. * Vertical peat growth model: I didn't intuitively understand the rationale for using bulk density data to formulate the vertical growth/downward transport model. Why didn't you use volumetric C content? Can your approach be described as a sequence of C-buckets that fill up by receiving inputs from the layer above (once this "spills over")? Then, spill-over is happening when the typical empirical volumetric C density at the respective depth, as measured in your 102 cores, is achieved. I'm just thinking out loud here, trying to make sense of the model. But maybe you can include such an intuitive description of your approach in the paper.

Actually, we did use volumetric C content in the vertical downward transport model. In Eq.3 (on Line182), we used observed bulk density and observed C concentration (%) to calculate an empirical amount of C (kg C/m^2) that each model layer can hold (M_l), then simulated C content is compared with f_{th}*M_l (f_{th} is a prescribed value, f_{th} = 0.7) to start downward transport of C. The reviewer's description generally matches our initial idea, although we calibrated the threshold to start downward C transfer and the amount of C to be transferred according to peat cores. Please see our responses to Eq. 4 and Eq. 5.

Q11. * While the striking performance in simulating inundation is definitely a plus, it remains unclear how this improvement over earlier publications (e.g., ST14) is achieved. Is it related to resolving the soil hydrology across layers instead of using a simple bucket model? The inundation sub-model is key for the peatland extent model and warrants a bit more attention in the paper.

We would like first to note that the soil hydrology scheme is not the only difference between our model and ST14. Representation of peatland vegetations, the soil thermal regime, and snow processes are all very different between the two models (Guimberteau et al., 2018; Stocker et al., 2014, GMD; Sitch et al., 2013, GCB; Ekici et al., 2015, The Cryosphere). All the above-mentioned processes can more or less have an influence on both the water fluxes and water content of a grid cell, and also affect simulated inundation.

In addition, we use peat-specific soil hydrological parameters for peatland, while using another set of parameters (which depend on the dominate mineral texture of the grid cell) for mineral soils. In contrast, ST14 used grid cell average soil parameters in soil hydrology. De Rosnay et al. (2002, JGR) evidenced that a single ‘‘average’’ textured soil couldn’t adequately represent the “averaged” water fluxes for heterogeneous regions, a subgrid-scale representation of soil type is relevant for modeling of soil water movement and surface fluxes.

For reasons indicated above, we feel that a comparison between ST14 and our model would be unfair, and we couldn’t attribute the better performance of our model in simulating inundation than ST14 only to the multi-layer physically-based soil hydrology.

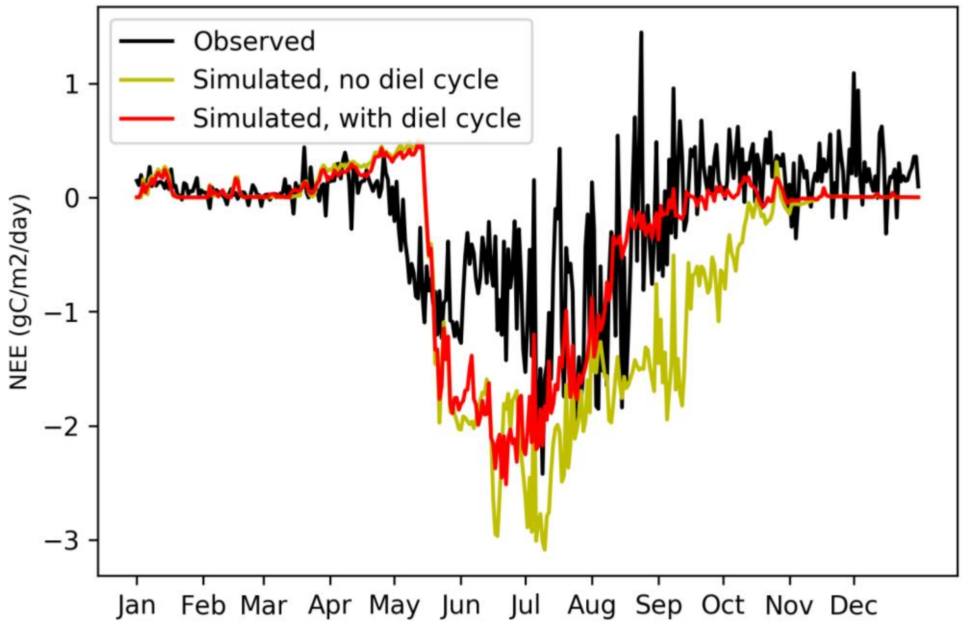
As for impacts of the 2-layer bucket scheme vs. the 11-layer physically-based diffusion scheme, while a comparison between the two schemes is out of the scope of this paper, we can get an idea of it from the study of De Rosnay et al. (2002, JGR) and Guimberteau et al. (2014, GMD). De Rosnay et al. (2002, JGR) showed that compared to the 2-layer bucket approach, the multi-layer diffusion scheme together with a subgrid-scale representation of soil type allow a more realistic representation of surface water fluxes, soil moisture profile and root water uptake, resulting in a better spatial and seasonal representation of evapotranspiration. Guimberteau et al. (2014, GMD) applied both the simple 2-layer scheme and the 11-layer diffusion scheme over the Amazon Basin, and showed that the 11-layer diffusion scheme simulates more dynamic soil water storage variation and improves simulation of soil water storage when compared with satellite observations.

Q12. * I don’t think it’s appropriate to require every model presented in GMD to be fundamentally novel. Furthermore, the model presented by Qiu et al. is largely an adoption of an existing model (ST14), which itself is based on Kleinen et al., 2012. Sufficient reference is made by Qiu et al. to this earlier work. However, the authors introduce and motivate their work with (l.94) “While both studies made pioneering progresses in the modelling of peatland ecosystems, they adopted a simple bucket approach to model peatland hydrology and peatland C accumulation, and neither of them resolved the diel cycle of surface energy budget.” However, it is unclear why the diurnal surface energy budget needs to be explicitly simulated in this context, and what limitations the simple bucket model approach incurs. It definitely needs more clarification what the model adds to our knowledge and our predictive power and I am skeptical that resolving the diurnal cycle of surface energy exchange adds a great deal. I am more curious about whether resolving soil hydrology across multiple layers helps better simulating relevant peatland-related processes, but the paper doesn’t provide this insight. I think it is

important that it becomes better clear what the merit of this model (over existing ones) is.

Actually, the diurnal cycle of surface energy exchange matters. We ran two simulations (with diel cycle vs. without diel cycle) to show the model performance in simulating peatland NEE and C stock at the Degerö Stormyr peatland site (Peichl et al., 2014). ORCHIDEE-PEAT v2.0 resolved energy processes in 30min time steps. In the first simulation (with diel cycle), we used measured half-hourly meteorological variables from the flux tower to force the model; in the second simulation, to mimic a run without diel cycle of meteorological variables, the daily mean of measured meteorological variables is used. In the figure below, observed peatland NEE (negative NEE: CO_2 sink) of the site in 2002 is shown in black, simulated NEE with diel cycle is shown in red, and simulated NEE without diel cycle is shown in yellow. Simulated NEE with diel cycle matches better with observations. Meanwhile, in the simulation without diel cycle, simulated C density is 50% greater than the simulation with diel cycle.

Regarding impacts of the multi-layer, physically-based soil hydrology scheme, please see our responses to Q11



Q13. * Observed (Mc Donald et al., 2006) and modelled inception age could be compared across space rather than just showing the numbers across time in Fig. 10. Actually, this comparison is subject to a possible sampling bias in Mc Donald. You want to test whether the model simulates the right inception time at a specific location, and not only the fraction of total number of simulated against the total number of sampled peatlands sampled in each age bin.

We couldn't transiently run the model due to the limitation of computational resources, so we spun up the model at discrete Holocene intervals with the soil C only part of the ORCHIDEE LSM being forced by archived litter input from a 100 years simulation with full ORCHIDEE (2000 yr each time) in this study. In other words, we first calculated peatland areas at 12,000 BP (Area0), then we assumed that peatland areas will not change in the following 2000 years, and we simulated C accumulated by Area0 in the following 2000 years. Then we updated peatland areas (Area1, at 10,000 BP) and simulated C accumulated by Area1 for another 2000 years…… As a result of this crude spinup acceleration procedure, we aimed to reproduce peatland areas and peat C stocks at discrete Holocene intervals rather than to capture inception time of peat cores. On the other hand, the inception time of peat cores couldn't represent the area development of a peatland, i.e. the simulated first initiation of peatland in a specific grid cell could be quite early, while the lateral expansion occurred much later. As the model was simulating peat C accumulation based on peatland areas (Area0, Area1…) at discrete Holocene intervals, we feel that the comparison at 2000-yr age bins is informative.

MINOR

*** l.21: I wouldn't subscribe to 'recently'.**

Deleted now from the text.

*** l.34: "270-540 PgC" Seems to be at the low end. What's the reference? On l. 44 references are given. But I suggest to use the latest (Yu, 2010) as the benchmark.**

We thank the reviewer for the suggestion. Considering that there are still large uncertainties in peat C stock estimates (Yu et al., 2012, Biogeosciences) and there is still no consensus in the soil (peat) science community, we feel that using only one benchmark is not rigorous enough (although the estimate by Yu (2010) is indeed the latest). We decide to report the range of peatland C stock estimates, as presented in the initial manuscript.

*** l.48 "in environments…" Make a new sentence, as this is not related to the first part of the sentence**

We rephrase this sentence as: "Due to water-logged, acidic and low-temperature conditions, plant litter production exceeds decomposition in northern peatlands. More than half of northern peat carbon was accumulated before 7000 years ago during the Holocene (Yu, 2012)."

*** l.49: Change 'despite' to 'while'.**

Changed in the text

*** l.64/65: Weird sentence. The depth itself doesn't prevent oxygen supply.**

We rephrase this sentence as: "Water table is one of the most important factors controlling the accumulation of peat, because it limits oxygen supply to the saturated zone and reduces decomposition rates of buried organic matter (Kleinen et al., 2012; Spahni et al., 2013)."

*** l. 69: Unclear: "critical level [of WTD???]"**

We rephrase this sentence as: "…However, some studies showed that changes in soil water content could be very small while the water table was lowering, the drawdown of the water table caused only small changes in soil air-filled porosity and hence exerted no significant effect on ER (Lafleur et al., 2005; Parmentier et al., 2009; Sulman et al., 2009)."

*** l. 70: Isn't WTD linearly related to soil moisture content? Why the threshold?**

It is intuitive that WTD is closely related to soil moisture content. However, we were not talking about the total soil moisture content of the peat profile here. We were considering the relationship between WTD and the moisture content of soils above the WT, because oxic respiration above the WT contributes more to the heterotrophic respiration of peat than anoxic respiration below the WT.

The figure below (Figure 2 of the study of Lafleur et al. (2005, Ecosystems)) shows measured WTD and soil water content at the Mer Bleue peatland site (soil water content was measured with a profile of TDR probes). As shown by Figure2a, the soil water content at 0.28m depth decreased rapidly when WTD drops from -25cm to -33cm, however, it only decreased marginally with further drops of WTD (from -33cm to -70 cm).

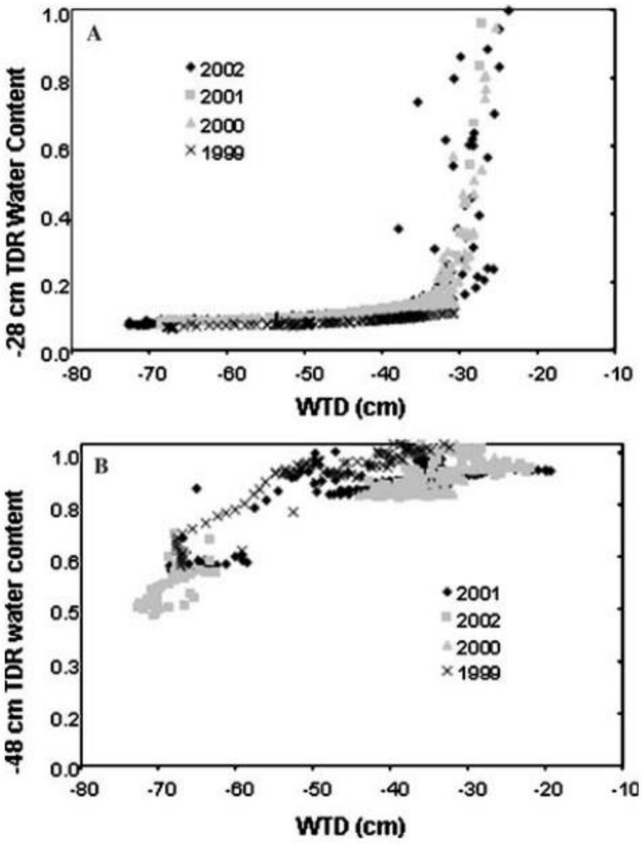


Figure 2. Relationship between peat moisture content as a percent by volume and water-table depth, WTD, below the hummock surface at **A**) −28 cm soil depth and **B**) −48 cm soil depth.

*** l.69-74: This sounds like the authors highlight a unresolved challenged here that the model/paper is going to address. However, it's unclear what is meant here (of course, WTD determines soil moisture or vice-versa),**

and how the model and results presented here address this particular challenge.

Yes, here we highlighted the fact that ecosystem respiration didn't always depend on WTD, there could be only small changes in soil moisture in the unsaturated part of the peat profile while WTD was significantly lowered (Lafleur et al., 2005, Ecosystems; Parmentier et al., 2009, Agric. For. Meteorol.; Sulman et al., 2009, Biogeosciences). Founded on a physically-based representation of hydrology of our model, the decomposition of peat C at each model layer is controlled by peat soil moisture (the soil volumetric water content - respiration relationship for organic soils from the meta-analysis of Moyano et al. (2012, Biogeosciences) were used), rather than by WTD.

We haven't ran a control simulation with decomposition controlled by WTD, and the aim of this study was to evaluate if the model can reproduce present-day peatland areas and C stocks, thus we didn't address this in the results. This particular issue (two-layered model vs. multi-layered model, WTD controlled vs. moisture controlled decomposition) can be addressed in future studies.

*** l.76: Style: don't refer to 'groups'.**

"groups" is deleted in the text.

*** l. 92: I would say that the key in ST14 was to account for peatland-specific water storage capacity in typical organic soils ("sponge" feedback) which enabled to accurately simulate the particular patter of peatland areas across the globe.**

We add a sentence to highlight this key improvement on Line92: "Stocker et al. (2014) extended the scope of Kleinen et al. (2012) in the LPX model. In their model, soil water storage and retention were enhanced and runoff was reduced by accounting for peatland-specific hydraulic properties. A positive feedback on the local water balance and on peatland expansion was therefore exerted by peatland water table and peatland area fraction within a grid cell. Areas that are suitable for peatland development were distinguished from wetland extent according to temporal persistency of inundation, water balance and peatland C balance."

*** l. 98: Unclear what "discrepancies" are referred to.**

Here, "discrepancies" referred to issues mentioned above; On Line54-Line74: decomposition doesn't always depend on WTD, soil moisture controls decomposition. On Line75-85: vertical heterogeneities in soil temperature, moisture and soil freezing can't be captured by two-layered bucket model. On Line86-Line97: previous models used two-layered bucket approach to model peatland hydrology and C decomposition without diel cycle of energy and water budget. To keep it as simple as possible (and not having to add redundant descriptions), we only rephrase this sentence on Line98 as: "To tackle these above-mentioned discrepancies and estimate the C dynamic as well as the peat area, we used the ORCHIDEE-MICT land surface model incorporating peatland as ……".

*** l.121: 'multi' instead of 'many'**

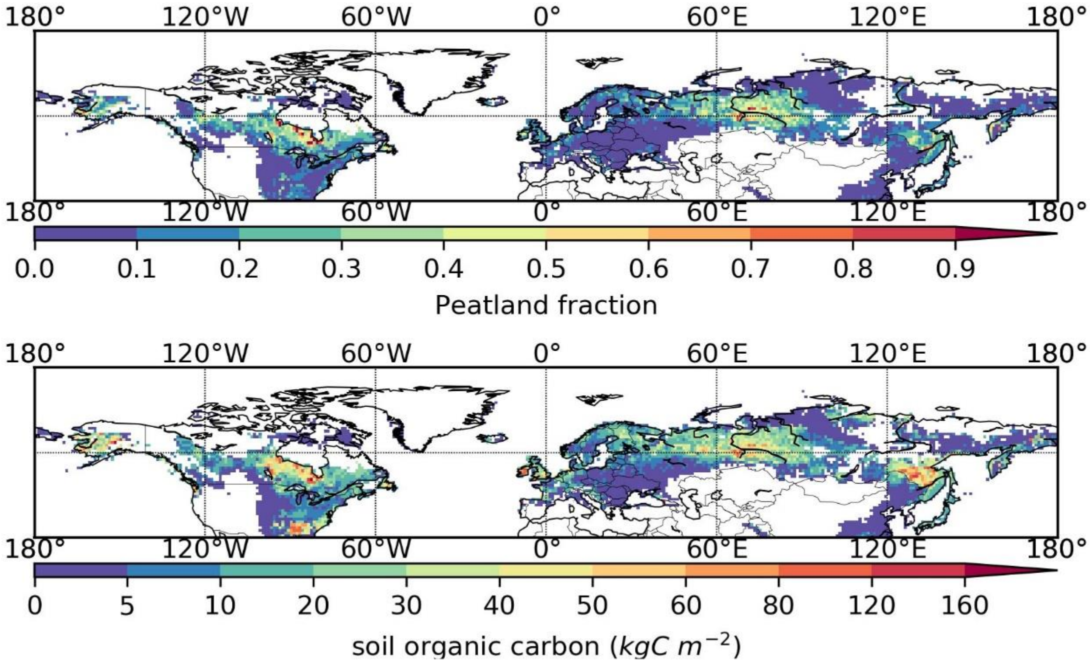
Corrected in the text.

*** Eq. 4: Why isn't it flux = f * (C_l - M_th,l)? The way it's formulated, the C content may drop below the threshold after transfer. Shouldn't it stay "saturated" after accounting for downward transport?**

This question and the question following this one are actually asking the same question: why didn't we keep the soil layer "saturated" after downward C transfer. Please see our responses to the question following this one.

*** Eq. 5: What's the rationale for introducing parameter f_th? Why isn't it 1?**

We calibrated f and f_{th} in Eq.4 and Eq.5 to match the simulated vertical C profiles with peat cores. Actually, we have also tried to formulate the flux as flux = C_l-M_l so that the layer stay "saturated" after C transfer, in this case, simulated vertical C profiles in site level simulations don't match with peat cores as well as with Eq. 4 and Eq5; and in regional simulations, the simulated peatland C in West Siberia and southeastern US are worse than with Eq.4 and Eq.5 (see the figure below). The formulations of the downward C transfer model will be tested for the next steps of model application and development.



*** l. 216: …than what? Explanation would be helpful: More computationally efficient than determining water table depth for each sub-grid pixel.**

We improve the sentence on Line216 as suggested: "……, which is more computationally efficient than determining water table depth for each sub-grid pixel (Stocker et al., 2014)."

*** l. 227: Ambiguous formulation. Do you mean max(monthly values)?**

Yes, we meant max(monthly values). We correct the text on Line 227 as: "…… We therefore compare simulated maximum monthly mean wetland extent over 1980−2015……."

*** Section 2.2.2.: Put Fig. S2 into main text and highlight difference to ST14.**

We move Fig. S2 into the main text in the revised manuscript, its difference to ST14 is highlighted on Line284: "The difference between our model and the DYPTOP model in simulating peatland area dynamics can be summarized as follows: (1) TOPMODEL calibration: TOPMODEL parameters are globally uniform in the DYPTOP model, but grid cell-specific in ORCHIDEE-PEAT v2.0. (2) Criteria for peatland expansion: In the DYPTOP, the "flooding persistency" parameter is globally uniform, being 18 months in the preceding 31 years. And the ecosystem water balance is expressed as annual precipitation-over-actual-evapotranspiration (POAET). In ORCHIDEE-PEAT v2.0, the flooding persistency parameter is grid cell-specific, being the total number of growing season months in the preceding 30 years. And peatland expansion is limited only by summer water balance. The relative areal change of peatland is limited to 1% per year in DYPTOP, but not limited in our model. (3) Peatland initiation: DYPTOP prescribes a very small peatland area fraction (0.001%) in each grid cell to simulate peatland C balance condition. Peatland can expand from this "seed" once water and carbon balance criteria are met. In ORCHIDEE-PEAT v2.0, no "seed" is needed because only the flooding persistency and summer water balance criteria need to be met for the first initiation of peatland (Fig. 1b), carbon balance is only checked after initiation (Fig.1c)."

*** l. 238: Not quite correct. I don't know what the authors refer to here.**

We revised the sentence on Line238 as: "Stocker et al. (2014) introduced a 'flooding persistency' parameter (N in Eq.12, Eq.13 in Stocker et al. (2014)) for the DYPTOP model to represents the temporal frequency of inundation. N is a globally uniform parameter in DYPTOP, being set to 18 months during the preceding 31 years."

*** l. 249: Question: Are non-growing season months discarded or do they count towards N?**

Non-growing season months are counted.

*** l. 254: What's the difference to ST14? Using only water balance during summer months instead of entire year? Might be worth mentioning explicitly. What's the rationale for this choice? Note that winter precipitation is relevant too as summer snow melt is effectively delayed winter precipitation.**

Yes, only water balance during summer months are used in the ORCHIDEE-PEAT v2.0 model. This difference to ST14 now is added on Line284 in the revised manuscript (please see our response to the previous question regarding Section 2.2.2.). Summer dryness was proved to be a key factor in limiting *Sphagnum* growth and peatland expansion in western Canada (Gignac et al., 2000, Journal of Biogeography) and in Western Siberia (Alexandrov et al., 2016, Scientific Reports). Based on the abundance of *Sphagnum* species on 640 peatland sites located in western Canada, Gignac et al. (2000) evidenced that *Sphagnum*-dominated peatlands do not occur in areas having summer moisture index (P–PET) values ≤ -6 cm. A similar climate characteristic, warm precipitation excess (P-0.7PET), was reported by Alexandrov et al. (2016), to explain the present-day distribution of peatlands in Western Siberia, their absence during the Last Glacial Maximum, and their expansion during the mid-Holocene.

*** l. 258 "May-September": Warning: this would mean that the model is not applicable in the south.**

A warning is added on Line261: "……SWB = 6 cm is selected so that the model captures the southern frontier of peatland in Eurasia and western North America (Text S5). Note that the definition of summer (May-September) and *SWB* are not applicable for tropical regions and the Southern Hemisphere."

*** l. 270: Clarify: C_lim "is defined here as…"**

C_{lim} is clarified on Line270 in the revised manuscript: "…… C_{lim} is defined here as long-term peatland C balance condition, it's a product of……"

*** l. 278: "SubC": What's this?**

SubC is a stand-alone soil carbon sub-model of ORCHIDEE, it simulates only soil carbon dynamics using monthly litter and soil C input, soil water and thermal conditions from the preceding full ORCHIDEE run. This part is described in Sect.3.2, L312-315.

*** l. 280-283: Add reference to ST14 as this is the same procedure as chosen by them.**

Reference is added in the text: "Therefore, parameterizations of the "old peat" pool is identical to mineral soils, following the study of Stocker et al. (2014). When peatland expansion happens, the peatland will first expand into this 'old peat' area and inherit its stored C (Stocker et al., 2014)."

*** l. 350: Missing references**

References are added: "Simulated peatlands SOC is evaluated against: 1. The WISE database (Batjes, 2016); 2. The IMCG-GPD (Joosten, 2010)."

*** l. 371: Figure for modelled vs. observed depth would be instructive.**

A figure for modelled vs. observed depth is added, please see our response to Q7.

*** Fig 1/Sect. 4.1: I didn't understand Fig. 1 and how I can read the RMSE from that figure. I expected a comparison of modelled and observed peat depth (or total column C), possibly split by temperate/boreal/arctic and/or bog/fen.**

A figure for modelled vs. observed depth is added, please see our response to Q7.

*** l. 411: Worth including figure in main text.**

The figure (Fig. S7) is moved to the main text now.

*** l. 425: Are leptosols and agricultural peatlands simply deducted from simulated peatland areas?**

Leptosols and agricultural peatlands were deduced from both simulated areas and simulated C stocks. To clarify it, we modify the sentence on Line425: "After masking Leptosols and agricultural peatlands from the simulated peatland areas and peatland C stocks, ……"

*** l. 440: Abbreviations introduced?**

These Abbreviations were introduced on Line357: "……, from literature data on peatlands in North America (NA) and in the West Siberian lowlands (WSL)."

*** l. 455: "we can only make the..." I don't understand this part.**

We couldn't do transient spinups due to the limitation of computational resources with the full ORCHIDEE LSM, so we designed an accelerated multiple spin-up strategy (Sect. 3.2): For regions that were unglaciated during Holocene, we ran SubC (a stand-alone soil carbon sub-model that only simulates soil C dynamics, without having to run the full ORCHIDEE) for 6 times, with SubC simulates C decomposition and accumulation over 2000 years each time. Therefore, we only know simulated peat depth at 2000-year intervals in regional simulations, and we can only make the comparison (observed vs. simulated peat depth) at 2000-year intervals. For example, for a peat core with its age being 8500 years, we compare its observed depth with simulated peat depth after the fourth SubC run.

*** l. 489: "several": delete**

Deleted.