Response to Anonymous Referee #3 (June 2018):

Implementing northern peatlands in global land surface mode: description and evaluation in the ORCHIDEE high latitude version model (ORC-HL-PEAT).

We thank the reviewer for his thoughtful comments. In the following, the reviewer's comments and suggestion are typeset normally and our replies to these comments and changes applied to the article are written in blue.

General comments
This paper is some what improved however I feel that there are still some outstanding issues to be addressed and with some careful thought on slight restructuring and simplification it can still be significantly improved.
I feel the structure needs to be modified. In particular the modelling improvements (2.3.2 and 2.3.3) need to come directly after the model description so the reader hasn't forgotten the structure of the original model.
The modelling improvement section has been moved before the model experiments section. The sentences below have been added in this section:
P4 L1-3: This section describes the developments and methods used to incorporate peatlands into the model.
P5 L18: see site description section 2.3.1
L23: (see section 2.3.1 for site description).
Section 2.3.2. could be made clearer on exactly what has been modified for the peat pft – just two things? To be clearer, we have added the following sentence at the end of the section:
The new PFT peatland corresponds to a flood tolerant C3 grass with the properties of the PFT C3 grass, where we applied a lower productivity due to the lack of nutrient and with a reduction of the rooting depth.
The stated aim of the paper is to look at the hydrology which seems to have compensating errors so on the large scale there is a small net effect. However Figure 2 is definitely not hydrology and actually the pft changes are good. It is actually a really good result, so should probably stay in but then that still begs the question of what about the large scale nee/gpp? We have reduced the carboxylation rate in order to better fit with observations. The comparison of modelled and observed NEE at large-scale remains however difficult to evaluate. Therefore, we have chosen to compare the peatland NEE with other PFT.

Also the bit about CH4 in the first paragraph of the discussion makes me want to ask more about it. I am not sure that the second part of the paragraph is that relevant here.
This paragraph has been added due to the request of a reviewer. We have reduced the length of this paragraph and this has been moved at the end of the discussion.

The site descriptions can be put in a table to simplify things. We have added a table to summarise the site descriptions: Table 2: "Descriptions of peatlands sites used for site evaluation. The "Years" column corresponds to the available years of FLUXNET meteorological data.

I presume the soils are not compressible? What impact might including those have on the results?
No, the soils are not compressible on seasonal time scales. The representation of the compressibility of soils must be carried out in the case of a study over long time scales. The time scale used in this study is not really long enough to warrant that this process be represented explicitly.

Do you have any suggestions as to how to deal with the issue of resolution dependence? How might this impact the TWS results? The PFT map is re-interpolated into the resolution of the model and allows a different percentage of each PFT as a function of the size of the grid cell. Concerning

peatlands, the water supply is also dependent of the resolution. On the scales of interest here, peatlands mostly represent a small fraction of the grid, and moderate resolution increases will not necessarily lead to very strong variations of the peatland fractions at the grid scale.
The corresponding paragraph in the results section has been modified as follows:
"The water supply of peatlands that comes from the surface runoff of the other soils depends on their fraction within the grid cell. As a result, all other things being equal, the water supply increases as the fraction of peatland within a grid cell decreases. In an extreme case, in which peatlands would be spatially concentrated in a small area within a larger region (because of topographic constraints, for example), this phenomenon would make the peatland hydrology resolution-dependent; however, the large-scale hydrology would certainly also be resolution-dependant in such a case. »

Can you cite some literature to suggest that we expect peat soils to have reduced variability of water storage? The reduction seen in the model is negligible:
P25 L31: This reduction enhances a total reduction of 6 % of the total variations of terrestrial water, which can be neglected at this large scale.
In the literature, this trend is uncertain and depends of the peatlands sites (Bullock and Acreman, HESS, 2003, doi: 10.5194/hess-7-358-2003).

Also is there some evidence in the literature that minerotrophic peatlands might be more sensitive to precipitation than ombrotrophic peatlands in the real world? Minerotrophic peatlands receive both direct precipitation and runoff, which indirectly depends on the precipitation of surrounding environments. This additional factor lets us suggest that minerotrophic fen could be more sensitive to precipitation. Unfortunately, we didn't find other studies on this topic.

How different is the precipitation forcing between WFDEI and CRU-NCEP?
In this study, we have compared the river flow of the Ob basin with using two meteorological forcing in order to know the sensitivity to the meteorological input. We have used WFDEI and CRUNCEP forcing, where the precipitation in the CRUNCEP dataset is slightly higher in average along the Ob basin than WFDEI. In average, the precipitation with WFDEI forcing enhances a peak of runoff one month earlier than with CRUNCEP forcing. The resulting modelled river flow is not really sensitive to the meteorological forcing such as seen in Wu et al (2018) and we have chosen to illustrates the difference of mean runoff instead of the mean precipitation over the Ob basin.

Please can you get a native english speaker to read the final version through – there are some awkward phrases particularly in the introduction, but also throughout the text.
Yes. The changes were made in this paper. We hope that the revised version will be satisfying.

Minor comments
Page 3, line 25 – one soil parameter is misleading. This has been changed into: one soil texture
Page 6, first paragraph not relevant. Also I am not quite sure of the meaning of the last sentence in section 2.3.1. Not all of the peatlands observed have been identified? The peatlands have been added at the expense of the fraction of PFT grasses only.
This sentence is confusing and has been removed.
Section 2.3.2 line 36 is repeated information. I don't see any line 36 in section 2.3.2
Page 8 line 1 – lateral water flow comes from runoff and indirectly from precipitation. I am a bit confused about whether there is any difference in treatment between surface and subsurface runoff. This has been changed: Peatland water inflow comes from precipitation, surface runoff and from nearby soils. In the case of peatlands, we have blocked the sub-surface runoff. The water inflow

corresponds to the direct precipitation and from the surface runoff to other soils which is redirected into peat soils.
Page 10 line 31 – what about surface runoff? Why cant we add simulated subsurface runoff?
Since we cannot measure the observed sub-surface runoff flows, we couldn't consider these flows in the model. The surface runoff represents the precipitation and the redirected runoff from other soils.
Page 11 line 4 – these three sites? Just talking about two sites in the previous sentence.
We have changed these three sites into "For the 3 sites of this study"
Why are the differences in the Siikaneva not snow related?
The difference in winter is related to a poor representation of the snow scheme, where the percolation of water is not taken into account. In this study, the single-layer snow scheme is used. When the soil is frozen, the modelled water flow towards the soil is blocked. For the Siikaneva site, snow cover starts in early December until April. This coincides with the period when the modelled WTD is underestimated. We changed the explanation in the text as follows:
"The opposite is observed at the Siikaneva site, where the WTD is overestimated during the summer, which could come from an outgoing flow such as a low drainage rate.
In winter, the modelled WTD is underestimated when the soil is frozen. The simple-layer snow scheme used in this study does not represent percolation of water. When the soil is frozen, the infiltration of water is blocked, which leads to underestimate the water content in the soil leading to an underestimation of the WTD."

Figure 1b is separate from Fig 1a so should be separated. Also figure 4a can be separated and made figure 1. These Figures have been separated.
I still think figure 2 looks a bit odd. Aso toy want the new model results to stand out not the old ones. The Fig. 2 (now Fig. 4) shows the impact of the reduction of the productivity applied for the PFT peat compared to the original PFT grass and this match well with the observed NEE of 3 peatlands sites. This type of figure using a 10 day running mean is frequently used to better describes the mean daily profile of the NEE free of the daily perturbations (such as Krinner et al 2005, Suyker et al 2003, Ukkola et al 2017).

Figure 3. change axes for precipitation. For the site of Degero, the observed precipitation is unknown in winter. We have chosen this axe for a question of visibility and a comparison for the 3 sites of peatlands.

Figure 7 – the differences between the dashed and solid line need to be more clearly defined in the caption. The caption has been changed as following: "Mean annual cycle of river discharge, (b) runoff of peat soils and (c) evapotranspiration of peatlands, for the Ob river basin. The observed river discharge is represented in red. The simulation are given with the standard version of ORCHIDEE-HL (STD) (black), with peatlands scheme without (PEAT) (blue) and with the reduced (PEAT-LOWET) (green) evaporation, using the meteorological forcing CRUNCEP (full line) and WFDEI (dashed line).

Figure 8 still ahs too many lines and too many seasonal cycles on it to see it clearly. There is an interesting time offset in the TWS between the model and observations which will become clearer when the x-axis is expanded. Move (b) in the figure caption to the beginning of the sentence. This has been changed as following: (b) including the water reservoir variations of total soil moisture of the soil and (c) the variations of total soil moisture of the peat soil column only.
Please change humidity to soil moisture. This has been changed.
STD is used twice for two different simulations. The term STD corresponds to the simulation where the properties of the PFT peat are not taken into account (i.e. corresponding to PFT grass properties with standard hydrology).
Page 20 line 27 – how did you define the presence or absence of frozen soils and snow – this is going to be different depending on the model or observational data set, so it is hard to make the two

comparable. Yes of course. In this study, we applied the comparison when the amount of the modelled snow is set to zero and when the surface temperature of the soil is above 0°C.

Page 22, line 15 – this sentence or two can probably be removed. This has been added following the request of a reviewer. This paragraph has been reduced as following:
For this, an adaptation of the CH_4 sub-model for flooded wetlands by Ringeval et al (2010) and based on the methane flux density model of Walter et al (2001) has been implemented taking into account WTD variations of peatland soils (not shown in this paper) (Largeron et al 2016).