Response to the comment of Anonymous Referee #2

We thank Reviewer #2 for providing detailed, constructing comments the manuscript. We address each comment below, where the reviewer's comments are shown in italics. The line numbers refer to the original document.

Notes on a recent model update:
We discovered a minor bug in the model after the submission of the initial manuscript, related to the handling of rain-on-snow events. After correcting this issue, we re-ran all the simulations and updated the figures and tables in this study. We conclude that the main findings presented stay unchanged, except for a lesser impact on vegetation patterns. As we moved multiple figures from the supplement to the main text following the suggestions of both reviewers, we decided to transfer the vegetation dynamics related figures to the supplement.

Summary of the changes to Figures and Tables (numbers refer to the original document):
We merged Fig. 8, 9 and 10 to one figure.
We moved Fig. 11 to the supplement.
Fig. 12 and its captions are updated.
We moved Fig. S2 to the main text.
We edited and moved Fig. S3 to the main text.
Fig. S11 was updated and included in the main text.
We updated and moved Table S3 to the main text.
We added a figure on soil carbon simulations to the supplement.

General comments
This article presents the new snow module implemented in LPJ-GUESS and shows how this newly implemented module affects the simulation of snow depth, soil temperature and ultimately biogeochemistry and vegetation distribution. The study is focussed on the arctic and sub-arctic regions of the northern hemisphere.
Showing how physical realism is important for biogeochemistry although the traditional separation between the two scientific communities tends to disappear.
We are glad to see the relevance of this study acknowledged.

This paper is well written in a clear style. I have however a few major comments, followed by detailed comments and questions.

- *I understand the need to limit the length of the paper but the authors tend to describe differences in simulation results without explaining them, or the explanation given is not sufficient. The authors present results for snow depth, soil T and water content at 25 cm depth and C fluxes, but they don't clearly show how their change in snow model affects those variables. They show results for the winter and summer seasons but the explanation often has to do with what happens during the spring, and the reader doesn't have a figure about spring*

  We acknowledge the comments regarding the necessity to provide more details how snow structural changes influenced the studied physical and biogeochemical entities. To address this shortcoming, we moved Fig. S2 (Zackenberg site analysis) to the main text and added more details in Section 3.1 about how snow structure and physical properties changed and lead to the differences in soil temperature and biogeochemical processes. We edited Fig. S3, splitting the plot to represent permafrost and non-permafrost regions. This figure was moved to the main text to provide information on the seasonal pattern and relationship of snow depth, soil temperature, soil water content and carbon fluxes.

- *The authors present site level simulations that were forced by large-scale atmospheric forcing (CRU). I don't see the point since some of these sites, like Zackenberg, have very detailed meteorological data. It also makes me wonder if the authors did just 2 pan-arctic simulations and compared the results with local data or if they used local data (like soil texture, or else) to perform these site-level simulations.*
  We did not intend to calibrate the model to specific sites, but aimed at achieving an overall improved model-observational fit across the Arctic. The LPJ-GUESS model is most commonly used with the standard, gridded climatic forcing in studies, thus we found it relevant to use the global drivers in this study to provide information on the model's skill for future projects. All simulations in this study ran with CRU climatic input without using site specific data. This was done to be able to compare the model's behaviour on different scales using the same simulation set-up. (For reference, the simulations were set up similarly for the Russian regional simulations in the referenced Wang et al. (2016) study.)

- *I believe an analysis of the changes in water content might be necessary*
  Considering the comments of both reviewers, we included a figure on soil moisture in the main text and added a description on how the changes in the snow scheme influences soil moisture conditions.

- *There are quite a few errors in the figures, errors in the units of the equations*
  Figures, tables and equations have been revised and corrected where found necessary – see specific comments below.

Detailed comments:

Eq 6 : as written, the units of this equation don't match: Ik is supposed to be in kg/m2 and rw don't have units (according to Table A2), so Wcap can't be in mm.

Following general assumptions 1 kg of liquid water over 1 m^2 surface corresponds to 1 mm coverage, therefore these units could be applied in Eq. 6. We acknowledge that the used units can cause confusion and for the sake of an easier overview we changed the units of W and W_{cap} in Table A2 to kg/m^2.

Figure 2: sublimation is not taken into account?

No, sublimation is not taken into account in the *Dynamic* snow scheme. The only way snow layers (snowpack) lose volume (SWE, snow water equivalent) is through the melting process.

as represented there is no snowfall on bare ground and I don't understand why the rain on bare ground affects the thermal properties. Are those boxes related to snow only or to snow and soil processes? If soil processes are represented, then why would thermal properties be based on density ? Is it an average of soil, water and ice density?

This figure shows processes related to the snow cycle. We made some adjustments to the figure and updated its captions for a more straightforward understanding. We now refer to the pathways as "snowpack present" or "snowpack absent" instead of the previous wording.

S1: typo : I don't think the Q10 was changed from 200.5 to 2.9 !

It is not a typo, the adjustment was made using recently published literature suggestions (Natali at al 2019.)

Natali, S.M., Watts, J.D., Rogers, B.M. *et al.* Large loss of CO_2 in winter observed across the northern permafrost region. *Nat. Clim. Chang.* 9, 852–857 (2019). https://doi.org/10.1038/s41558-019-0592-8

L190-191: as shown in Figure 3, soil temperature is higher than observations in summer only for 2 sites out of the 5: Kytalyk and Samoylov. Also, it is not really discussed in the following sections.

The related and following sentences have been removed from the text.

L192-193: I agree that soil temperatures have a smaller variance with the Dynamic than the static run but that is not true for snow depth, except in Abisko.
We removed snow depth from this sentence, as a response to a comment from Reviewer 1.

Figure 3 : are the site statistics calculated with monthly values ? or monthly anomalies ? (departure from the average seasonal cycle). Since the average seasonal cycle is shown, I would show the statistics from the anomalies.
We updated this figure and excluded summer months from the statistical panel. We are showing the absolute numbers of average monthly values, and we prefer to keep the range of absolute values in this figure.

Figure 4: The Y axis of the observed soil T and static snow results are mixed up: The observed soil T axis should be -40 to 0 like the simulations. Similarly, the Y axis from the snow depth for the static scheme should go from 0 to 35 like the other 2 snow panels.
Thank you for pointing this out, this figure has been updated.

L222 : Figure 5a doesn't show at all large changes in Scandinavia and in Western Russia. The blue in coastal Norway can be mistaken for the coastal line and lakes (like in Canada and Finland) and western Russia seems less red than central Siberia for instance and shows much less change than N-E Canada (Baffin etc) or far East Siberia. I suggest other colors (especially the blue that is impossible to distinguish from lakes) and may be give longitudes instead of "western Russia"
All the figures were revised and lakes are now shown in white to avoid misunderstandings. The reason behind changes in the coastal region is the high amount of precipitation in the region and newly introduced compaction scheme affecting snow depth.
To address the second half of the comment, we reviewed and changed how we refer to the most affected regions: we write coastal Norway (instead of Scandinavia) and Western Siberia (instead of Russia).

Figure 5c: the TTOP PF rectangle in the legend should be solid grey (not just the border)
This detail is now corrected.

L235: This explanation is wrong. Maximum ALD happens at the end of the summer, early fall. The authors can't use the warmer winter temperatures as explanation for the deeper ALD ! According to figure 6, Dynamic has cooler temperatures in summer, not warmer. I would assume that it is more related to the speed at which the soil refreezes in the fall. Static refreezes earlier and faster, hence stopping the summer melt earlier and reducing the increase in ALD.
We see the point by the reviewer that this explanation is plausible but we suggest keeping our current discussion on the drivers of ALD changes. We acknowledge that there are multiple factors that can account for the changes in ALD and define the maximum ALD. We adjusted Section 3.4 to provide a more thorough explanation on the complex relationship between the potential drivers i.e. soil temperature, soil moisture, timing of snow melt etc. We can observe in the presented figures (e.g. Fig. S6 top row) that the largest changes in winter soil temperature occur at the edges of the permafrost region. Even though summers are cooler, we can see the direct winter impact on the mean annual soil temperatures that define the estimated permafrost extent (see Fig. 5 (c)).

Section 3.3.2: in all this section (text and figures) and in the supplementary material, the units of the C fluxes are wrong. They should be in mass of carbon per unit area ***per unit time.*** *The authors wrote g/m2 but the reader has to wonder if it is per year, per month, per season…*
Figures in this section show the mean spatial pattern of variables averaged over a season (winter or summer) as stated in section 3.1.1. All of the Pan-Arctic figures and related sections show average conditions over the winter and summer seasons for the period 1995-2015, if not stated otherwise. We adjusted figure captions to make this clear.

L266: "soil carbon outputs were used to normalize Rh ..": the authors should say how here or in the legend of Figure 8 because I don't understand the units of the normalized Rh (g /m2)

See the related comment of Reviewer 1. We now added a more thorough explanation on how the normalisation was done and included this information in the figure captions. In this case, the plotted R_h represents the fraction of soil carbon respired per season (winter or summer). Considering this comment we reviewed the units and changed it to "fractions". We hope that these changes collectively can help to better understand Fig 8.

L 268: "winter respiration … increased, except for …" : this is totally invisible on Figure 8a with the current colormap

The color map of Fig 8 (and all other figures) has been revised and adjusted for better readability.

L 270-280 : - the authors should refer to the figures in Annex Fig S7 to S9

We noted this comment and made sure refer to the supplementary figures in an orderly manner in the main text.

- Figure S8 winter (and Figure 9a) : There is something wrong here Static has values around 0.250 g/m2 (dark green) where Dynamic only has 0.125 but the difference doesn't show anything as if a mask had been applied. Also, white for negative NPPs is not very visible.

We re-plotted this figure with a different, diverging color map for increased readability. This cleared up the confusing pattern caused by the use of inappropriate color map.

L 275 : "increased autotrophic respiration" : why ? I know winter temperatures of the soil at 25 cm are higher. But how does that affect autotrophic respiration? I guess the question is how is vegetation temperature calculated ?

This change can be attributed to higher soil temperature prompting increased root R_a. Other contributing factors involve more roots due to increased vegetation carbon and higher maintenance respiration.

Root respiration is calculated as function of the carbon biomass of roots, soil temperature, root C:N mass ratio:

$$root\ resp = \frac{\text{maintenance resp. coeff} \times \text{scaling factor} \times \text{root C mass}}{\text{root C: N mass ratio} \times \text{respiration Q10 T response}}$$

L275 "but higher in summer". Again why is that ? T is lower in summer. Could it be linked to water content ?

We revised this paragraph and considering the received comments we now edited Fig. S3 and moved it to the main text (see response above). By doing this, we give a more detailed information on NPP, NEE, soil temperature and soil moisture relationship. During the summer, the Dynamic scheme simulates higher NPP in non-permafrost region and lower in permafrost underlain areas. Changes during the winter months are on a much smaller scale, where indeed we can observe a marginally lower NPP. The changes seen in NPP can be derived from the GPP and Ra. The balance of the changes in these variables govern the changes we note in NPP.

L279-280 : when discussing NEE, it would be interesting to have annual values too.

We added annual values for NEE in Table S3 that has been moved to the main text.

Fig S9: the grey is really disturbing because it can be confused with light blue. Is Nee decreasing in large parts of Siberia and Canada in winter ?

The figures' colour palette has been revised and changed for better readability. NEE is now presented using a diverging scale and unified scale for both winter and summer seasons.

L 295 – text section S3.5: typo Fig ??
This typo has been corrected.

Figure 12: The signs on the figure are not explained. A typo may be ?
I don't understand the colors of the arrows – I think the caption is not completely correct : it states that "The colour of the arrows show whether there has been a net increase or decrease in the particular variable" but this is shown by the color of the box it-self. I assume the color of the arrow means how the change in one variable affects the change in the other (higher soil temperature favors higher heterotrophic respiration for instance). But that doesn't seem the case either because increasing Rh should favor increasing NEE, but the arrow is blue for the 2 left panels. I might have missed something but the figure should be better described.
This figure and its captions has been updated for improved readability – addressing the comments of both reviewers. We listed the presented variables and their units and reviewed the connecting arrows as well.

L 320: 3N mineralization decreased in wintertime": why is that ?
See our answer to a previous comment above.

L344 : "the model-observation fit may be improved by using site-specific climatic forcing" : yes indeed ! why not do that ?
See the response to this comment in the general comments sections.