Dear Prof. Roulet:

Thank you for your truly helpful comments on the initial draft of the manuscript. We have carefully studied the comments and modified the manuscript following your recommendations. Our detailed responses are attached below.

General comments:

A nice paper that introduces some modifications to DNDC to apply it to assess the fluxes of CO_2 and CH_4 in a permafrost peatland. The authors use the very well-studied Stordalen peatland in northern Sweden as their test site. This is appropriate because the data sets exist to rigorously evaluate the model output but it would be worthwhile for the author to provide a little more explanation of the physical and climatological setting of Stordalen. It does not represent permafrost peatlands in general but a class of ice core peatlands called peat palsas or plateaus. These are normally found in sporadic or discontinuous permafrost regions over fine sediments and near good sources of water. These landforms have their own cycle of growth and decay regardless of climate change – they exist in the climate zone where normally variability creates their inherent instability, so small normal variations in temperature and/or snow accumulation cause expansion and thawing. This does not matter for this particular study too much because the work is focused on does DNDC capture the changes in carbon cycling that occur with thawing, but it does influence how much readers can take from this study and apply it to permafrost peatlands in general. This is not a criticism but a few words of caution in the introduction to contextualize this work are warranted. The paper represents a step in the development of a fully functional permafrost peatland DNDC. It examines the changes in the biogeochemistry and fluxes when the hydrological change is provided to the model. DNDC does not simulate the change in hydrology due to changes in surface elevation that occur with permafrost thaws, and does not include functions for the vegetation changes that occur because the moisture conditions change dramatically. Again this is not a criticism. The authors deal with the future needs for development of DNDC to make it fully functional for permafrost environments in the discussion, but it would be useful to acknowledge in the introduction what the modifications will do and what they will not do – i.e. be more explicit about the current assumptions involved in the application of DNDC to this current study. You do this in the methods but it would be very useful to qualify this work in a general way in the introduction (maybe right after the objectives). Also let the readers know what the final modeling objectives are and where this particular study fits along the route to those objectives. Many of my specific comments below come from wondering how this was going to work in the end. My final comment is that this paper assumes readers have a considerable amount of knowledge about how DNDC works and how it handles the relationship between hydrology, and carbon cycling in aerobic and anaerobic conditions. I am not sure those who do not have a good knowledge of DNDC will understand the description of the changes to DNDC, and what it means to couple DNDC to NEST? I fully recognize the authors do not want to reiterate a full description of DNDC with each manuscript using and developing the model. Think about the reader this is their first experience with DNDC. Because this is the first DNDC runs involving permafrost it is a new audience that will be reading this modeling work – not the main stream DNDC crowd. The paper is well written. The objectives are clear. The authors attain their stated objectives. The conclusions are justified given the results presented. The paper is a worthwhile contribution to the literature.

Response: We have added material to the Introduction to better set the context for our study. We have added text describing the study site and its environmental conditions, and indicated the representativeness of the study peatland in the introduction (Page 5 Lines 102-109). We also added sentences to explicitly clarify current assumptions (i.e., during simulations, different soil hydrologic conditions and vegetation characteristics of these land cover types were used as model inputs, therefore we focused on predicting the changes in soil thermal dynamics and C cycling along with thawing; Page 5 Lines 112-114) involved in the application of DNDC in this study.

In addition, we have added a sentence to explain what we mean by "couple NEST to DNDC" in the revised manuscript (Page11 Lines 245-248). We also now point out that it would be ideal to incorporate changes in soil water regime and vegetation along with permafrost thaw into the model's framework for further developments of the model (Page 30 Lines 724-733). We think that our efforts of incorporating a permafrost model (i.e., NEST) would have set a sound basis for the model to incorporate these processes in the future, although these efforts only represent a step toward developing a comprehensive biogeochemical model fully functional for northern ecosystems (as mentioned at Page 30 Lines 729-733 in the revised manuscript).

Specific Comments:

Pg 3971 line 4 - How similar are the climate variables between ANS and Stordalen. The temperatures and precipitation (see Olefeldt's work on the hydrology of Stordalen) are quite different and I believe solar radiation is different. When calibrating the model for the hydrology is it not fairly important to have local precipitation.

Response: Climate variables are different between Stordalen and ANS. However, it seems the differences in climate were inconsistent across different years. For example, Olefeldt and Roulet (2012) indicated that the differences between annual temperatures measured at Stordalen and ANS were small (about -0.1 °C) and summer precipitation at Stordalen was greater during 2007 to 2009. Rydén (1980) indicated that the monthly mean temperature during the May to September was 0.9-1.6 °C less at Stordalen than at ANS and monthly mean precipitation is probably similar at Stordalen and ANS based on the measurements from 1972 to 1976. We also believe solar radiation may be different between Stordalen and ANS, although we did not find publication reporting the differences.

In order to predict water table dynamics, DNDC uses several parameters to estimate lateral flows. We estimated these parameters by comparing the modeled and observed water table depth (WTD). Having local precipitation would be helpful for calibrating these parameters and reducing potential discrepancies between the modeled and observed WTD. However, using local and ANS precipitation data may not result in significant differences in water table, due to the fact that the differences in precipitation could be partially offset by different hydrological parameters (i.e., the model is calibrated). In addition, we used the observed WTDs if the measurements were available to reduce the influence of lacking local precipitation on soil thermal and biogeochemical processes.

Reference:

Olefeldt, D. and Roulet, N. T.: Effects of permafrost and hydrology on the composition and transport of dissolved organic carbon in a subarctic peatland complex, J. Geophys. Res., 117, G01005, doi: 10.1029/2011JG001819, 2012.

Rydén, B. E.: Climatic representativeness of a project period: epilogue of a tundra study, Ecol. Bull., 30, 55-62, 1980.

Pg 3971 Line 23 - Does this means you calibrated the hydrology of the model? Why not simple use the measured WTD?

Response: Yes. We have calibrated several hydrologic parameters to simulate lateral flows and WTD, because water table was not observed every day at the test sites; we use the calibrated model to interpolate between observations. This point is now explained in the text (Page 12 Lines 277-280).

Pg 3974 line 13 - Did you examine the structure of the residuals to see if there were any particular biases in the model.

Response: Thank you for this suggestion. We have decomposed the root mean squared error (RMSE) into systematic and unsystematic components to check the structure of mean-square errors in daily C fluxes. Please see the next response for details.

Pg 3974 line 19 - Can you look at the random versus systematic components of the RMSE? The random component could be reduced by better specification of parameters, but a systematic error indicates potential structural problems with model components.

Response: We have revised the manuscript by following this suggestion. The structure of mean-square errors in daily C fluxes has been checked through decomposing the root mean squared error into systematic and unsystematic components. The method used for decomposing RMSE, follows Willmott's work, and is described in the section of model application (Pages 13-14 Lines 309-318). A table (Table 5) summarizing systematic and unsystematic RMSE has been added in the revised manuscript. The results demonstrate that the discrepancies between the modeled and measured NEE could be primarily attributed to random components, because systematic errors accounted for 11%, 25%, and 23% of the mean-square errors in daily NEE at the Palsa, Sphagnum, and Eriophorum sites, respectively. Most of the mean-square errors in daily CH_4 fluxes were also attributable to random errors at both the Sphagnum (76%) and Eriophorum (89%) sites. We have provided these conclusions in the revised manuscript (Page 25 Lines 602-607 and Page 26 Lines 628-631).

Pg 3975 line 20 - Was this true for CH4 simulations or also for NEE? In the methods you discuss how DNDC hydrology was calibrated. It would be good in that section to mention you used the model hydrology only for days without measurements? In Figure 4 a-g there is no distinction between measured versus "infilled" wtd. How much is measured and how much is infilled?

Response: In this study, simulated water table was used only for the days without field observations; otherwise the observed water table was used to determine the soil water conditions. We have mentioned this setting in the section of model application (Page 12 Lines 277-280). Both the simulated and observed WTDs were used to support the simulation of all biogeochemical processes, including CH_4 and NEE.

In Figure 4, lines and dots are simulated and observed water table dynamics, respectively. This point has been mentioned in the title of this figure in the revised manuscript. Across seven growing seasons from 2003 to 2009, the days with the observed WTDs accounted for approximately one third at the Sphagnum and Eriophorum sites. We have added this information into the revised manuscript (Page 12 Lines 280-282).

Pg 3976 line 14 - In DNDC, what are the sources of substrate used for the production of methane? Strom et al. have shown that around the roots of Eriophorum there is a significant amount of acetate, presumably from roots exudates. Olefeldt et al. have shown that the DOC quality changes significant in the Eriophorum areas of Stordalen. Does DNDC include these pathways? Is there any correlation between CH_4 and NEE and is there a lag correlation?

Response: In DNDC, the substrates used for the methane production come from both decomposition of SOC and plant root activities including exudation and respiration. We have added this information into the revised manuscript (Page 9 Lines 213-216). During simulations, the Eriophorum site had higher plant growth rates and root mass, and consequently had more DOC from roots exudates, consistent with the study by Ström and Christensen (2007). However, the model currently doesn't simulate DOC quality, and so does not predict different DOC quality between Eriophorum and other sites. In DNDC, NEE is related to metabolism of plants and root activities although there is no direct connection between NEE and CH_4 in the model. In this study, we did not find any correlation between CH_4 and NEE. However, we found correlations between CH_4 and soil temperatures and/or WTDs. These relationships have been described in the revised manuscript (Page 19 Lines 451-455 and Page 20 Lines 479-482).

Reference:

Ström, L. and Christensen, T. R.: Below ground carbon turnover and greenhouse gas exchanges in a sub-arctic wetland, Soil Biol. Biochem., 39, 1,689-1,698, 2007.

Pg 3977 line 3 - What fraction of the annual fluxes occur outside the growing season?

In other words how much of the annual simulated fluxes are for periods that you have not evaluated?

Response: We have summarized the simulated C fluxes during non-growing season. During 2003 to 2009, the means of accumulated CO_2 emissions over non-growing seasons were 342, 32.8, and 101 kg CO_2-C ha^{-1}, respectively, at the Palsa, Sphagnum, and Eriophorum sites. The average accumulated CH_4 fluxes over non-growing seasons were 9.8 and 13.8 kg CH_4-C ha^{-1} at the Sphagnum and Eriophorum sites. We have added these sentences into the revised manuscript and compared this to field results (Page 27 Lines 636-648).

Pg 3979 line 25 - Can you quantify these differences? The data exists to see if this is a reasonable explanation. There have been lots of measurements done at Stordalen over the years. You could see what the offsets are and use these factors to adjust the continuous record from ANS and see if this explanation stands up to the test?

Response: Thank you for suggesting this analysis. Climate variables are different between Stordalen and ANS. However, it seems the differences in climate were inconsistent across different years (see the response to the comment at Pg 3971 line 4 for details). We also did not find data showing consistent offset in climate variables on a daily basis, therefore can't adjust the record from ANS. We have added sentences (Page 25 Lines 599-601) to remind readers that meteorological conditions were different between Stordalen and ANS, and these differences inevitably affected model simulations.

Pg 3980 line 15 – 18 - There have been over winter measurements of NEE and CH_4 that you could use to quantify the fraction of the annual exchanges that occur in the winter. This would provide a better quantification than the qualification you make here.

Response: Thanks for the comment. The quantifications of NEE and CH_4 over winter have been added into the revised manuscript by following this suggestion (Page 27 Lines 636-648).

Pg 3981 line 20 – 21 -Does the CH4 exchanges partitioned by vegetation type match what Christensen et al. (2004) estimated? Or is this the same analysis that Johansson et al (2006) did?

Response: Our analysis on investigating the impacts of vegetation change on C fluxes is similar with the analysis by Johansson et al. (2006). We applied the modeled C fluxes to the areal changes of land cover types at Stordalen; while Johansson et al (2006) summarized observations of C fluxes during growing season and applied them to the areal changes of land cover types. In comparison with our analysis, Johansson et al (2006) also treated their 'wet' cover somewhat differently – equivalent to 'semiwet' (Sphagnum) for NEE due to similarity in vegetation composition, but with a higher value for CH_4 emission as it was an inundated area. Because the 'wet' area was nearly 30% of the study region and expanded from 1970 to 2000, Johansson et al. (2006) estimated that the mire was a GHG source in terms of 100-yr CO_2 equivalents to the atmosphere, and they reported an increase of 47% in net radiative forcing from 1970 to 2000 by considering the fluxes during growing season. Our analysis estimated that the mire was a GHG sink due to a lower value for CH_4 emission in 'wet' areas, and yielded an overall decrease of 27% in net radiative cooling from 1970 to 2000; i.e., a net warming climate impact in both cases but from a different baseline.

Pg 3981 line 24 – 26 - How will DNDC handle this? I do not believe it has dynamic vegetation? Wouldn't you also need to simulate the lateral redistribution of water because of the changes in elevation due to the presence or absence of permafrost? Ok this is discussed below - maybe some signal that you will discuss this below is warranted?

Response: In this study, different WTD and vegetation characteristics in different land cover types were used as model inputs, so we did not simulate changes in soil water regime and vegetation along with permafrost thaw at Stordalen. We have explicitly described this setting in the section introduction (Page 5 Lines 112-115) and discussion (Page 30 Lines 724-729) to emphasize the assumptions involved in this study.

Pg 3982 line 6- 10 - See comments above: hint that this discussion is coming here earlier in the manuscript. Knowledgeable readers will have these questions a lot earlier in the manuscript.

Response: Thanks. These sentences have been deleted in the revised manuscript due to duplication.

We appreciate your very constructive review of our manuscript. Please also note the attached revised manuscript to this comment.

Sincerely yours,

Changsheng Li and co-authors