Dear Reviewers,

Thank you for comprehensive reviews of the initial draft of the manuscript, with numerous detailed and helpful comments. We have carefully studied the comments and modified the manuscript following the recommendations. Our detailed responses are attached below.

Reviewer #1 (N. T. Roulet)

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.

Reviewer #2

Summary comments: This paper provides useful modeling of coupled biogeochemical and thermal dynamics within a thawing peatland at Stordalen, Sweden. However, the field design (land cover site types) and model specifics are not always clearly presented, and the characterization and interpretation of the model results could use some reworking. There are grammatical issues throughout that weaken the paper but are likely easily fixed. I would be interested to see results for soil thermal conditions in relation to predicted fluxes – and particularly in relation to episodes where simulated and observed fluxes do not agree – more explicitly presented or discussed. Ultimately, the authors should discuss what Stordalen represents relative to other peatland permafrost environments.

Response: We have revised the manuscript by adding detailed descriptions and explanations on the environment conditions of the three land cover types, model applications, characterization and interpretation of the model results. Specifically, we have explicitly described environmental conditions (i.e., permafrost conditions, thaw of active layer, and soil water conditions) of the three land cover types, and introduced the representativeness of the land cover types as well as the Stordalen peatland. The assumption involved in the model applications has also been clearly presented in the sections of "Introduction", "Methods and data", and "Discussion". In the section of "Results and analyses", we have described the simulated and observed seasonal patterns to better present the characterization of the model results and discrepancies between simulations and observations. In addition, we further analyzed the model results regarding possible changes of carbon fluxes due to permafrost thaw at Stordalen, following the reviewer suggestions. The conclusion of this new interpretation has been added into the revised manuscript. For more details about these revisions, please see the responses to the detailed comments below.

Detailed comments

Page 3963 title: The peatland environment should be specified as this study is not representative of all permafrost sites.

Response: We have clarified the permafrost environment at Stordalen and that it doesn't represent peatlands in other permafrost zones after describing our objectives (Page 5 Lines 102-109).

Page 3965, Line 1: please consider other work here – e.g., Tarnocai et al.

Response: Thanks for your suggestion. The number (carbon stored in permafrost areas) cited in our manuscript (Schuur et al., 2008) is the same as that reported by Tarnocai et al. (2009), since both papers were written by same group of people. We have added the paper by Tarnocai et al. (2009) into the reference list.

References:

Schuur, E. A. G., Bockheim, J., Canadell, J. G., Euskirchen, E., Field, C. B., Goryachkin, S. V., Hagemann, S., Kuhry, P., Lafleur, P. M., Lee, H., Mazhitova, G., Nelson, F. E., Rinke, A., Romanovsky, V. E., Shiklomanov, N., Tarnocai, C., Venevsky, S., Vogel, J. G., and Zimov, S. A.: Vulnerability of permafrost carbon to climate change: implications for the global carbon cycle, BioScience, 58, 701-714, 2008.

Tarnocai, C., Canadell, J. G., Schuur, E. A. G., Kuhry, P., Mazhitova, G., and Zimov, S.: Soil organic carbon pools in the northern circumpolar permafrost region, Global Biogeochem. Cy., 23, GB2023, doi:10.1029/2008GB003327, 2009.

Page 3965, Line 13: Please provide more recent references on permafrost thaw in northern peatlands – there is ample recent work in this area.

Response: Additional recent references on permafrost thaw in northern peatlands (James et al., 2013; Quinton et al., 2011) have been added into the revised manuscript (Page 3 Lines 60-61).

Page 3965, Lines 19, 22, 28: dropped "the" in front of "C cycle", "C balance".

Response: corrected.

Page 3966, Line 16: grammar - "may be resulted".

Response: "may be resulted" has been corrected as "may arise" in the revised manuscript.

Page 3967, Line 1: Clarify why this is an improvement over just looking at the measurements?

Response: We have added "A validated simulation model provides a mechanism for not only interpreting observations but also predicting the impacts of future climate change on greenhouse gas emissions." (Page 6 Lines 119-121).

Page 3967, Line 15: grammar - "hydrology, vegetation, and subsequently".

Response: We have revised this to "…affected surface topography, hydrology, and vegetation, and thereby exerted a strong influence …".

Page 3967, Line 24: "Palsa" is indicated in parallel with species names when it is a term that indicates a ground ice landform category. Please clarify that.

Response: In this study, the "Palsa", "Sphagnum", and "Eriophorum" indicate land cover types instead of vegetation species, which has been clarified in the revised manuscript (Page 7 Lines 144-147).

Pages 3968, Lines 3-4: "different stages of permafrost degradation"; please be more explicit. The implication is they are not sequential, so what do they represent?

Response: We have explicitly described environmental conditions (i.e., permafrost conditions, thaw of active layer, and soil water conditions) of the three land cover types in the revised manuscript. Generally, the Palsa sites are underlain by permafrost, and the seasonal soil thaw rate is relatively slow with the active layer depth (ALT) < 0.7 m in late summer; the Sphagnum sites are also underlain by permafrost, and are intermediate thaw features, where ALT is generally thicker than 1.0 m in the late summer. The Eriophorum sites have no permafrost and the soil thaw rate is relatively rapid. Therefore, these three land cover types have different permafrost regimes and soil thaw rates, and represent a gradient of permafrost thaw. These explanations have been added into the revised manuscript (Page 7 Lines 147-156). In order to clearly describe representativeness of the study sites, we have reworded "different stages of permafrost degradation" into "a gradient of permafrost degradation" throughout the manuscript.

Pages 3968, Line 10: Seems premature to mention the sign convention here – move to where it is first used.

Response: Thanks for your suggestion. We have moved the descriptions of sign convention to Page 13 Lines 299-302.

Page 3970, Lines 12-16: grammar - “as well as” should be “or” or similar.

Response: corrected.

Page 3970, Line 27: grammar error.

Response: corrected.

Page 3971, Line 1-2: grammar “and their effects”.

Response: we have revised this to "as well as their impacts".

Page 3972, Line 7: “ran” should be “run”.

Response: corrected.

Page 3972, Lines 9-10: Do the vegetation and biogeochemistry feed back to the thermal module? This seems important to mention here and discuss later.

Response: In DNDC, the vegetation and biogeochemistry modules feed back to the thermal module. However, soil initial conditions have only a small influence in DNDC as compared to other factors; therefore, although we did not turn on the vegetation and soil biogeochemical modules during the initialization of soil climate conditions, potential errors in soil initial conditions due to this probably had a very small influence on the modeled results. This is now mentioned in the revised manuscript.

Page 3972, Line 19: missing “the” before “permafrost thaw gradient” – related grammar issues not always identified but entire manuscript should be edited for this.

Response: Thank you for your careful reading. We have corrected this grammar error as well as others throughout the manuscript.

Page 3972, Lines 21-24: Is GWP the best approach? Why not use the approach of Frolking et al., (2006)?

Response: We use the GWP approach so that we can compare our results with the earlier Stordalen analysis of Johansson et al. (2006). It is also a conventional approach for quantifying the impact of multiple greenhouse gases and so will be familiar to most readers. Frolking et al. (2006) and Frolking and Roulet (2007) used a simple atmospheric model to develop a radiative-forcing impact calculation for sustained and/or varying emissions, in contrast to the assumption of pulse emissions in the GWP methodology. This approach becomes particularly important for longer time scales (significantly longer than methane’s ~10-year residence time in the atmosphere), which were not our focus in this study.

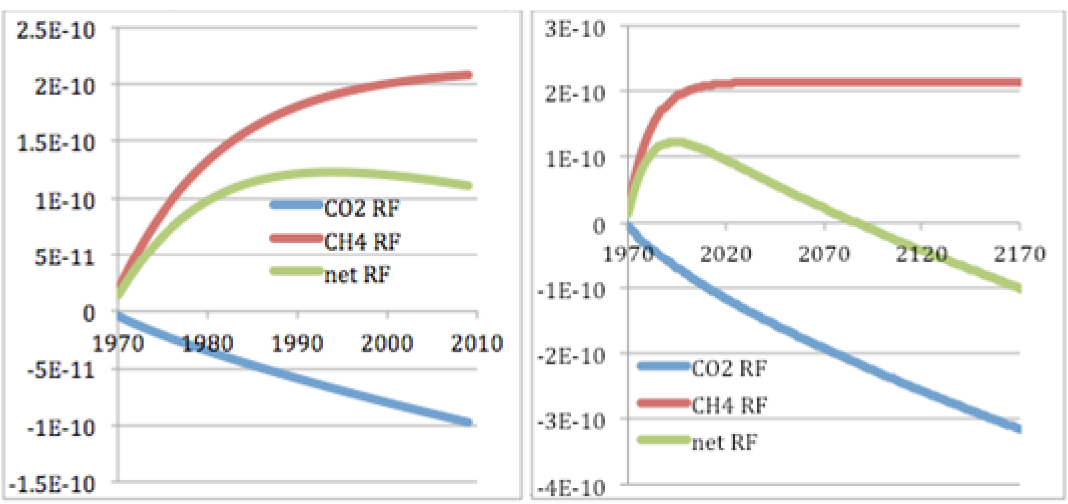
We did a quick analysis using the method of Frolking et al. (2006), as follows. We assumed that the three land cover types (Palsa, Sphagnum, Eriophorum) had constant annual emissions equal to the means simulated by DNDC from 2003 to 2009. In one case, we gave the land cover types their 1970 areas; and in the second case their 2000 areas (more Eriophorum and Sphagnum, less Palsa). These constant annual emissions from 16.5 ha total Stordalen mire were input into the atmospheric model (5 boxes for CO_2, 1 box for CH_4); and in each case, annual time series of radiative forcing were calculated (following Frolking et al. 2006) along with a difference (2000 area radiative forcing minus 1970 area radiative forcing, see the attached Figure). The changes in vegetation area resulted in increased CO_2 uptake and increased CH_4 emissions, due to the loss of Palsa area (low fluxes), the increase in Eriophorum area (high fluxes), and the smaller increase in Sphagnum area (moderate fluxes). Due to the stronger radiative impact per kg CH_4, induced warming initially dominates (consistent with 100-year GWP values); but eventually methane's shorter residence time in the atmosphere limits its impact, while net CO_2 uptake continues to have an increasing cooling impact. After about 120 years of constant flux differences, the net impact due to vegetation changes switches to cooling (see the attached figure). We have added a sentence (Page 24 Lines 566-572) to describe the impact of vegetation changes on C flues at Stordalen if the fluxes from two scenarios with different vegetation cover areas were to persist for long time (Please see the response to the comment in Page 3979, Line 6). However, the assumption of constant fluxes and areas is not a solid one in this case, and the assumption that all area conversion happened in 1970 is also not well-founded. So we conclude that the approach of Frolking et al. (2006) would require more information than we have, and so focus on the more common, simple, and comparable GWP analysis.

References:

Frolking, S., Roulet, N., and Fuglestvedt, J.: How northern peatlands influence the Earth's radiative budget: Sustained methane emission versus sustained carbon sequestration, J. Geophys. Res., 111, G01008, doi:10.1029/2005JG000091, 2006.

Frolking, S. and Roulet, N. T.: Holocene radiative forcing impact of northern peatland carbon accumulation and methane emissions, Glob. Change Biol., 13, 1079-1088, 2007.

Johansson, T., Malmer, N., Crill, P. M., Friborg, T., Åkerman, J., Mastepanov, M., and Christensen, T. R.: Decadal vegetation changes in a northern peatland, greenhouse gas fluxes and net radiative forcing, Glob. Change Biol., 12, 2,352-2,369, doi: 10.1111/j.1365-2486.2006.01267.x, 2006.



Response Figure. Difference in radiative-forcing (RF, W m^{-2}) impacts of two scenarios (see response text above for the scenarios).

Page 3973, Line 17: “mesic” and “wet” are not parallel terms and their use here is unclear; please specify relevance of these modifiers.

Response: We have reworded "mesic" into "semi-wet" throughout the manuscript to clarify this description. In addition, soil water conditions at the study land cover types have been briefly introduced to specify the "semi-wet" and "wet" use in the manuscript.

Page 3973, Line 20: “gradient of soil moisture”; this should have been introduced at the page 3968 in describing the study design. How do these sites represent a gradient? This may be specified in other Stordalen literatures but is an essential component of this study that should be specifically described.

Response: We have added sentences to describe soil water conditions of the three land cover types in the section of "the study area and field observations" (Page 7 Lines 147-156). Generally, the Palsa sites are dry features; the Sphagnum sites are wetter than Palsa with water table levels fluctuate close to the ground surface; and the Eriophorum sites are generally wetter than Sphagnum with water table levels constantly near or above the ground surface. Therefore, these three land cover types represent a gradient of soil moisture.

Page 3973, Line 21: Please re-state in terms of the relationship between thermal conductivity and soil moisture.

Response: In the revised manuscript, we have described the relationship between thermal conductivity and soil moisture, and explained why rate of summer thaw accelerated along the gradient of soil moisture during simulations (Page 15 Lines 348-352).

Page 3973, Line 23: “few” should be “a few” for correct implication.

Response: The "few" has been reworded into "a few" in the revised manuscript.

Page 3974, Line 1: the downward dip is persistent in all simulated years. Model seems to chronically overestimate thaw rate and to produce the inverse time trend (increasing rather than decreasing thaw rate). Please modify description accordingly.

Response: This increasing thaw rate during the late periods of soil thaw was only observed in one year (2005) at the Sphagnum site; however, since thaw measurements could go to 90 cm, it may be that an abrupt thaw (to >90 cm) also happened in other years but was not recorded in the data. However, we have modified this description to state that "DNDC overestimates the thaw rate during the late periods of soil thaw in most years" (Page 15 Lines 353-356).

Page 3974, Lines 20-21: How does RMSE=13% indicate “success”? How good does the simulation need to be?

Response: In this study, we calculated the discrepancies (as indicated by RMSE) between the simulated and observed cumulative C fluxes to check if the simulated rates of seasonal cumulative C fluxes were comparable with the corresponding measurements. The word "success" was used if discrepancies between simulations and observations were close to or less than the standard deviations of observations, i.e., if the discrepancies were comparable with or less than the uncertainty of observations. Palsa NEE discrepancies were less than the standard deviations of the observed cumulative NEE in each year. We have added these explanations not only in this case (Palsa NEE) but also in all other cases.

In addition, we have specifically pointed out the simulations in which the discrepancies between the simulations and observations were obviously larger than the standard deviations, indicating that the model overestimated or underestimated C fluxes in these cases.

Page 3974, Lines 22-23: Please state criteria and reasoning for “generally captured” vs. “discrepancies appeared in 2003”. There are discrepancies in every year.

Response: In this study, we applied the DNDC model to assess effects of permafrost thaw on C fluxes of a sub-arctic peatland at Stordalen. Our main focus was to check whether the model can capture the observed differences in seasonal soil thaw, NEE, and CH_4 fluxes across the Palsa, Sphagnum, and Eriophorum sites at Stordalen. In addition, we evaluated the model's ability to predict seasonal patterns of soil thaw, NEE, and CH_4 fluxes through visually comparing the daily simulations and observations (Figures 2, 3, and 4) as well as calculating the coefficient of correlation (R). We have added sentences to describe both the simulated and observed seasonal patterns (as shown in these figures) in the revised manuscript, in addition to calculating correlation coefficients. For this case (Sphagnum NEE), the simulated and observed seasonal patterns of daily NEE were similar across 2004 to 2009. We hope this similarity can suggest that the DNDC generally captured the seasonal fluctuations of daily NEE over these years. We fully realized that discrepancies existed in every year on a daily basis, which has been explicitly mentioned in the revised manuscript for all simulations (e.g., Page 17 Line 385). For the discrepancies in 2003, we described more details, since it seems these discrepancies were systematic biases.

Page 3974, Line 26: Shouldn’t soil temperature be a driver here rather than air temperature?

Response: In DNDC, plant growth is affected by several environmental factors, including radiation, air temperature, soil moisture, and nitrogen availability (as mentioned at Page 9 Lines 203-205). While low solar radiation and air temperature directly limited plant productivity, low soil temperature and shallow soil thaw depth also restricted water and nitrogen availability, and thereby limited plant growth. Therefore, low soil temperature and shallow soil thaw depth should also be the reasons for the predicted lower uptake rates of CO_2 during 25 May to 22 June in 2003 at the Sphagnum site. We have added these drivers in the revised manuscript (Page 17 Lines 388-390).

Page 3975, Line 1: Low R is 0.32

Response: Thanks. This error has been corrected.

Page 3975, Lines 6-8: “successfully predicted” and “good agreement”; again, what constitutes success? As with the Sphagnum sites, it seems R is not the best metric of fit since 2004 (3p) looks to have systematic offset despite R = 0.52. Is this a result of inaccurate simulation of soil temperatures early in some years as appears to happen in 3h?

Response: This sentence has been deleted in the revised manuscript. Instead, we have added sentences to describe both the simulated and observed seasonal variations in daily NEE, which showed similar seasonal patterns across the studied years excepting 2004 at the Eriophorum site. We hope these descriptions, in combination with R, could better guide readers. Thanks for pointing out the systematic offset in 2004 (Figure 3p). We explicitly described this offset in the revised manuscript. Due to the lack of necessary information, we can't determine the reasons for the inconsistencies of NEE in this case. But it seems discrepancies in soil temperature can't explain the inconsistencies, because we did not find an overestimation of soil thaw rate at this site, which may result in higher uptake rates of CO_2 in model simulations as shown in Figure 3p.

Page 3975, Lines 17-18: “reliably simulated” – same comment as above: what are the criteria? What is the goal of the simulation?

Response: We have revised the manuscript according to this comment. Please see the above response to the comment at Page 3974 Lines 20-21 for the explanation of " reliably simulated". For this case (Eriophorum NEE), the discrepancies between the simulations and observations were less than the standard deviations of the observed cumulative NEE in each year from 2003 to 2007. Therefore the model may reliably simulated the growing season cumulative NEE over these years. However, the discrepancies were higher than the standard deviations of the observed cumulative NEE in 2008 and 2009, suggesting that the model may overestimated the CO_2 uptake during growing season in these two years. We now have explicitly pointed out these discrepancies in the revised manuscript (Page 18 Lines 420-424) to remind readers.

Page 3975, Line 20: water tables – use of observed vs. simulated seems like methods rather than results. Also, did WTDs account for the rise and fall of the floating mat? How was the “ground surface” height assessed?

Response: This sentence has been deleted in the revised manuscript. There was no evidence of floating mats anywhere that fluxes were measured (or anywhere in the Mire). For the field measurements, the ground surface was taken to be the apparent wrack on the root surface, sometimes with Sphagnum – i.e., when the broad edge of a ruler would stop when gently pushed. DNDC does not simulate floating vegetation, so WT in the model is always relative to the soil/peat surface (i.e., where above-ground litter is deposited, and below which roots are active). Ultimately though, what is important for DNDC is the relative amount of unsaturated (and saturated) peat – which is well represented by the depth of the water table from the apparent or vegetated surface.

Page 3975, Line 26: “generally matched”; please quantify.

Response: We have added sentences to describe both the simulated and observed seasonal variations in daily CH_4 fluxes, which showed similar seasonal patterns across the studied years at the Sphagnum site. As mentioned at Pages 18-19 Lines 432-437 in the revised manuscript, both the simulations and field measurements showed the highest peak in August or September. In addition, DNDC simulated small spikes of CH_4 emission a few days after snowmelt and during post-growing season, which agreed with the observations. The R values also indicate the simulated seasonal variation of daily CH_4 fluxes was significantly correlated with the observed seasonal variation in each year ($P < 0.0001$). The similar patterns and significant correlations between the simulations and observations may suggest that DNDC generally captured the observed seasonal characteristics of CH_4 fluxes, despite a few remaining inconsistencies.

Page 3975, Line 28: grammatically unclear.

Response: We have changed this to "despite a few remaining inconsistencies".

Page 3976, Lines 1-2: Appears circular – isn't this what the model does rather than a result of the model? A figure indicating these relationships in the model might help.

Response: We have added a Figure (Figure 5) into the revised manuscript to demonstrate the relationships between the simulated CH_4 emissions and soil temperatures as well as water tables by following this suggestion. The relevant descriptions have also been added (Page 19 Lines 451-455).

Page 3976, Lines 1-8: The simulation appears to capture some early and late season spikes consistent with literatures – how does this happen in the model? Seems important to mention as the shoulder seasons can be hard to capture with observations.

Response: We have added sentences to explain the simulated early and post-season spikes of the CH_4 fluxes. The simulated early CH_4 flux spikes were induced by snowmelt and thaw of surface soil layer, which created water saturation in surface peat and thereby supported CH_4 production and emission. The high fluxes predicted during post-growing season occurred during occasional thaw of the surface soil layer during the early freezing stage, which provided pathways of releasing for both newly produced methane and methane accumulated in the soil profile (Page 19 Lines 437-443).

Page 3976, Line 11: grammar - "few biases"

Response: corrected.

Page 3976, Lines 11-13: Please provide some explanation of why this offset is mentioned when others aren't, as well as insight about where the model indicates relationships other than what is hypothesized to go on.

Response: We realize that discrepancies in daily CH_4 fluxes existed in each year, which has now been explicitly mentioned in the revised manuscript (Page 20 Line 472). For the offset in 2008, we described more details, since it looks like this offset was a systematic discrepancy. Also, we have added a Figure (Figure 5) into the revised manuscript to demonstrate the relationships between the simulated CH_4 emissions and soil temperatures.

Page 3976, Line14: What does a P value indicating "significant" mean in this situation given the number of observations? Capturing seasonal trends but not detail?

Response: In this study, we checked the model's ability to predict seasonal patterns of C fluxes through visually comparing the daily simulations and observations (Figures 3 and 4) as well as calculating the correlation between simulated and observed values. The significant P values ($P < 0.0001$ for most cases in this study) indicate the modeled C fluxes were linearly correlated with the measurements. We think the significant correlations, together with the similarity between the simulated and observed seasonal variations, indicate that the model captured seasonal trends of daily fluxes, although discrepancies existed in each year.

Page 3976, Lines 16-18: How did the modeled results demonstrate a relationship to soil temperature? Seems reasonable but these results are not presented and this explanation has not been posited previously in this manuscript.

Response: We have added a Figure (Figure 5) into the revised manuscript to demonstrate the relationship between the simulated CH_4 emissions and soil temperature (and WTD). The relevant descriptions have also been added (Page 20 Lines 479-482).

Page 3976, Line 18: "because of inundated conditions"; Please specify how inundated conditions relate to soil temperature etc., and show how the model "demonstrated" these relationships.

Response: We have reworded the text to more clearly express that the inundated conditions at the Eriophorum site generated constantly wet anaerobic conditions suitable for CH_4 production, therefore the temporal patterns of CH_4 fluxes at this site were mainly related to changes in soil temperature, instead of changes in water table (Page 20 Lines 475-479). A figure illustrating the relationships between CH_4 fluxes and soil temperature as well as water table has been added in the revised manuscript (Figure 5). Soil temperatures are related to water conditions – summer thaw rates were rapid at the Eriophorum site due to inundated conditions (as mentioned at Page 15 Lines 338-339). We have clarified how water conditions affected summer thaw rates in the DNDC model (Page 15 Lines 348-352).

Page 3976, Line 21: "well captured" – grammar issue

Response: We have revised this grammar issue in the revised manuscript (Page 21 Line 485).

Page 3977, Lines 9-10: How did differences in these environmental conditions influence simulated NEE? The land cover type differences have not been described.

Response: We have explained how differences in environmental conditions resulted in different predictions of annual total NEE across the Palsa, Sphagnum, and Eriophorum sites in the manuscript. Please see Pages 21-22 Lines 508-518 for details. The differences in vegetation characteristics were represented by different values of the physiological parameters used for simulating plant growth (Table 2), which has been clarified in the revised manuscript (Page 21 Lines 507-508).

Page 3977, Lines 10-12, 15: high CO_2 uptake "due to" high productivity seems circular; low CO_2 uptake "because of" low productivity also circular.

Response: Measured CO_2 uptake is the net of productivity and respiration, so it does not necessarily follow that high productivity leads to high net uptake, though that will usually be the case. In this study, high productivity results from vegetation parameter values - field data show that the Eriophorum site vegetation has greater NPP than the palsa sites. This difference is one of the factors explaining why the model simulated the highest and lowest net CO_2 uptake at the Eriophorum and Palsa sites, respectively. We are just documenting the simulations and don't consider these results to be surprising (or completely circular).

Page 3978, Line 10: "permafrost thaw gradient" – The relationship among these land cover types was described as a "gradient of soil moisture" and the Eriophorum and Sphagnum types were described as two possible outcomes of Palsa conversion – implying either could happen, not a sequence from one to the other. Please describe clearly what the site types represent in the methods section so that the study design is clear. This may be represented in other papers but is critical to describe here.

Response: In the section of "The study area and field observations", we have described soil environments (i.e., permafrost conditions, soil thaw characteristics, soil water conditions) of these three land cover types. Generally, the study sites have different permafrost regimes and soil thaw rates, and therefore represent a gradient of permafrost thaw – essentially "intact", "diminishing", and "gone". (Please see the earlier response to the comment at Page 3968 Lines 3-4 for details). We have added these explanations into the manuscript.

Page 3978, Lines 15-18: “stronger warming potential”: Again, consider the approach of Frolking et al., 2006 with respect to warming potential in peatlands, or explain why this has not been done. Also, is this difference significant? What is “stronger warming potential”?

Response: Please see the response above (to the comment at the Page 3792, Lines 21-24) regarding why we did not use the radiative forcing approach of Frolking et al. (2006).

Based on the simulated annual total C fluxes, the Palsa site was a larger net sink of CO_2-equivalents than the Eriophorum site and this difference was significant ($P < 0.05$). This sentence has been revised into "Therefore, the modeled results demonstrated that for the wetter Eriophorum site, higher CH_4 emissions offset its larger net C sink, and the Palsa site was a larger net sink of CO_2-equivalents than the Eriophorum site."

Page 3978, Lines 21-24: What about conversion among types? Was sphagnum ever converted to Eriophorum or vice versa? If not then the “permafrost thaw gradient” statement above should be modified.

Response: We have described differences in vegetation cover in the manuscript (Page 23 Lines 552-555). Based on changes in aerial photos analyzed by Johansson et al., (2006) and Malmer et al., (2005), it seems very likely that Palsa degradation, i.e., the transition from Palsa to Sphagnum or Eriophorum land cover types, has occurred. The reverse process, Palsa aggradation, can also occur (and probably did during the Little Ice Age), but this has not been clearly documented at Stordalen. However, DNDC does not simulate dynamic vegetation, and so cannot simulate these transitions, but only characterize the greenhouse gas fluxes of the various states. Please see the response to the comment at Page 3968, Lines 3-4 for why we stated "permafrost thaw gradient" in the manuscript.

References:

Johansson, T., Malmer, N., Crill, P. M., Friborg, T., Åkerman, J., Mastepanov, M., and Christensen, T. R.: Decadal vegetation changes in a northern peatland, greenhouse gas fluxes and net radiative forcing, Glob. Change Biol., 12, 2,352-2,369, doi: 10.1111/j.1365-2486.2006.01267.x, 2006.

Malmer, N., Johansson, T., Olsrud, M., and Christensen, T. R.: Vegetation, climatic changes and net carbon sequestration in a North-Scandinavian subarctic mire over 30 years, Glob. Change Biol., 11, 1895-1909, 2005.

Page 3978, Line 27: “wetter trend”: please reword for clarity

Response: We have replaced "This wetter trend" with "This trend toward a wetter ecosystem".

Page 3979, Line 2: “areas changes”; please correct grammar for clarity

Response: We have replaced "these areas changes" with "these changes in vegetation cover areas".

Page 3979, Line 6: Are these values really significantly different than zero given large range? Is there really net warming under a peatland scenario of ongoing emissions based on the method by Frolking et al. (2006)? The lack of winter observations is mentioned subsequently, but this is an additional uncertainty that bears mentioning here.

Response: We have re-calculated the net impact due to vegetation changes by considering the inter-annual variability of C fluxes and the results show that the estimation of a net CO_2 equivalent emission from 1970 to 2000 is not significantly higher than zero ($P = 0.07$). We have added a sentence about this point (Pages 28-29 Lines 683-685).

In addition, we did a quick analysis (see the response to the comment at Page 3972 Lines 21-24) using the method of Frolking et al. (2006) to investigate the net impact of vegetation changes under ongoing emissions. The conclusion is " If these fluxes from vegetation cover areas (1970 vs. 2000) were to persist for one to two centuries, an analysis with a simple model of atmospheric perturbation radiative forcing (Frolking et al. 2006) shows that the different atmospheric lifetimes of CO_2 and CH_4 are such that the CO_2 sink would overcome the CH_4 emissions in terms of instantaneous radiative forcing and the climate impact of this vegetation change would eventually switch to a net cooling after about 120 years". This sentence has been added into the revised manuscript (Page 24 Lines 566-572). We also added a note to remind readers that the simulated C fluxes over winter are not well-constrained by field data at this time.

Page 3979, Lines 12-13: "stages of permafrost thaw"; please clarify in methods and throughout as mentioned above

Response: Revised according to this comment. Please see the response to the comment at Page 3968, Lines 3-4 (above) for details.

Page 3979, Lines 16-20: Seems too broad. Please clarify whether the model captures transitions among site types rather than just seasonal to inter-annual variation in fluxes for a site type with changing atmospheric conditions. Are transformations of hydrology and vegetation modeled? Does the model predict when Palsa is transformed to Sphagnum? Please reword this section accordingly.

Response: We have clarified that the model captured the differences (not transitions) in C fluxes among these land cover types but cannot yet independently simulate subsequent changes in soil hydrology and vegetation (Page 24 Line 584 and Page 25 Lines 587-590). In this study, different WTD and vegetation characteristics were used as inputs for different land cover types, therefore we did not simulate changes of water conditions and vegetations. We have clarified that in both introduction and discussion (Page 5 Lines 112-115 and Page 30 Lines 724-729).

Page 3979, Lines 22-23: "on some days" - many or a few? Tended to overestimate? Please clarify and specify. Overestimated CO_2 uptake resulting from over-prediction of photosynthesis again seems redundant or circular, not causal.

Response: A few days, which has been clarified in the revised manuscript. These overestimations may have resulted from over-prediction of photosynthesis due to lacking site-specific data. Analysis of RMSE, following methods of Willmott (please see responses to Reviewer #1) also showed that for NEE the majority of discrepancy was unsystematic (systematic error ~ 20% of mean squared error), implying that the discrepancies between the modeled and measured NEE could be primarily attributed to random components, including absence of site-specific data. We have clarified this in the revised manuscript. In DNDC, NEE is calculated as the difference between net primary production (NPP) and soil microbial heterotrophic respiration (HR). Over-predictions of photosynthesis result in higher NPP would thereby make DNDC overestimate CO_2 uptake rates (i.e., lower NEE).

Page 3980, Line 5: What about soil temperature, perhaps due to prior air temperature conditions or snow cover? What about biogeochemical processes influencing belowground temperature in ways not captured by the model?

Response: Yes. Low soil temperature is a reason for the predicted lower uptake rates of CO_2 during 25 May to 22 June in 2003 at the Sphagnum site. Please see the response above (to the comment at the Page 3974, Line 26) for details. We can't figure out the reasons causing the inconsistencies of NEE in this case based on available information. But it seems potential discrepancies in soil temperature cannot explain these inconsistencies because we did not find an underestimation of soil thaw rate at this site, which may result in lower uptake rates of CO_2 in model simulations as shown in Figure 3h.

Page 3980, Line 9: “few inconsistencies” should be “a few inconsistencies”.

Response: Corrected.

Page 3981, Lines 25-26: good point; please evaluate the degree to which classes and replication of them as well as measurement points within them are adequate at this site.

Response: While this was not the focus of our study, we may get a few hints from the model results. As shown in this study, NEE and CH_4 fluxes are strongly controlled by soil thaw regime, soil water conditions, and vegetation characteristics; which implies adequate measurements for quantifying C fluxes may need to include all combinations of these environmental conditions. Since soil thaw dynamic usually closely relates to soil water conditions and vegetation characteristics at Stordalen, we think adequate measurements should include combinations of soil water conditions and vegetation characteristics. In this study, we simulated Palsa, Sphagnum, and Eriophorum because there was a large dataset of fluxes from these sites. Whether they sufficiently capture the range of land cover types can only be evaluated by scaling up to the mire and comparing with eddy flux tower data. There are now several flux towers operating on the mire, and so after a few years of data collection, this should be possible. We can't conclude how many replications are adequate for this site based on this modeling work. We have added this sentence to the end of the paragraph: This can be evaluated in future analyses by comparison of up-scaling flux by aerial fractions of land cover with multi-year eddy covariance tower fluxes. Flux towers are now operating at Stordalen under the European Integrated Carbon Observation System (ICOS) program (Paris et al., 2012).

Reference:

Paris, J. D., Ciais, P., Rivier, L., Chevallier, F., Dolman, H., Flaud, J. M., Garrec, C., Gerbig, C., Grace, J., Huertas, E., Johannessen, T., Jordan, A., Levin, I., Papale, D., Valentini, R., Watson, A., Vesala, T., and ICOS-PP consortium: Integrated Carbon

Observation System, Geophysical Research Abstracts, 14, EGU2012-12397, 2012.

Page 3982, Lines 4-6: Are there observations of soil temperature that could be compared to the simulations, both for soil temperature and gas flux? Seems very important to show these and reference the literature relating fluxes to soil temperature.

Response: We did not find observations of soil temperature profiles that could be compared to the simulations. We investigated relationships between the simulated CH_4 fluxes and soil temperature. The results (see Figure 5) showed that the temporal patterns of CH_4 fluxes were largely related to the changes in soil temperature. This conclusion is consistent with the field studies. We have described these results and cited relevant literature in the revised manuscript.

Page 3983, Line 18: “soil thaw” should be “seasonal soil thaw” to clarify that longer term thaw trajectories are not modeled.

Response: We have changed "soil thaw" into "seasonal soil thaw".

Table 1 Please specify that rates are unit-less (m/m per day) if this interpretation is correct

Response: Thanks, we have specified the units of surface inflow, surface outflow, and ground outflow rates in the revised manuscript.

Figures 2-4 please specify years for clarity

Response: We have specified years in these figures.

We appreciate your comprehensive reviews. Please also note the attached revised manuscript.

Sincerely yours,

Changsheng Li and co-authors