Response to the Reviewer #1:

We thank the reviewer for the constructive comments and suggestions that have helped us rethink and improve the manuscript. We will revise the manuscript according to the reviewer's comments (see point-by-point responses below). For reference, our response to comment "n" by the reviewer is labeled "R1C[n]" where R1 represents Reviewer #1. Throughout this document, the reviewer's comments are reproduced in their entirety in black, and our responses are given directly afterward in blue.

This study considers cold-season CO2 and methane emissions from Arctic tundra, which has been recently identified as an important component of the tundra carbon budget. In this work, the authors modify an ESM land surface scheme (ELMv1-ECA) to better represent freeze/thaw processes in the soil, to better represent the impact of soil moisture and temperature on soil carbon decomposition, and to allow transport of methane out of the soil during the cold season. This addresses three factors that they identified as causing the model to produce too little carbon emission during the cold season. Namely, that winter soil temperature (and therefore also liquid water) were underestimated, that the CO2 production was too little even with the correct soil temperature, and that methane transport out of the land surface was not possible during winter. They find that the model is significantly improved with these modifications, although emissions during the ZCP are now slightly overestimated instead of underestimated. They also look at long term trends.

In general the paper is clearly written and logically ordered. It is clear that the model does improve between the initial and final model versions, and now can simulate cold season emissions better than previously. I liked the fact that they tested a large number of different functions for the decomposition response, since this is certainly a major source of uncertainty in modelling.

However, before considering this for publication I would ask for some substantial justification and clarity about the changes that have been made. I would also ask for some additions to the text to consider other (potentially) important factors.

We appreciate the reviewer's constructive comments. We provide detailed elaboration for the three items pointed out by the reviewer in the following responses:

General comments:
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1) Carbon/substrate. There is plenty of evidence that availability of carbon substrate is important for controlling methanogenesis (e.g. Strom et al 2012), and soil respiration in general (e.g. Brooks et al 2004). This is not discussed in this paper. It is not clear whether the model simulates the soil carbon dynamically, or whether (dynamic or not) the soil carbon takes appropriate values in the model. For one site (IVO) there is some discussion of this (Section 3.2), but it sounds like this soil carbon data is only used to set the soil thermal/hydraulic properties. Does it also form the substrate for soil respiration?

If the substrate is not correctly simulated, then you may compensate for this with incorrect choice of decomposition functions. I would ask for some more analysis of the soil carbon - for example compare it to observed values at the site and identify if this can be a source of bias (I would suggest add to a plot or table in the supplementary material and discuss in the main text).

R1C1: We agree with the reviewer about the importance of carbon substrate. We explicitly simulate carbon substrate and its impact on soil respiration as described by Koven et al. (2013) and

Zhu et al. (2019), and we will add descriptions about ELM carbon model into the revised manuscript (within the Appendix). We would also like to conduct more analysis on soil carbon, however, due to lack of observations, we cannot compare simulated soil carbon with observation for this study. We will explore available *in situ* soil carbon datasets and incorporate these analyses in our future studies.

2) Snow. The main problem with ESM's underestimating winter soil temperature is often related to representation of snow, so I was surprised that this was not discussed in more detail, and only the phase change was considered as leading to underestimated winter soil temperatures (although snow is mentioned once in the results). For example, Burke et al (2020) show the offset between air and soil temperature in CMIP5 and CMIP6 ESM's as a function of snow depth. In models that poorly represent snow, the offset can be up to 10 degree C biased - meaning at 10 degree C cold bias in winter soil temperature. In models that improved their snow scheme between CMIP5 and CMIP6, there is a huge improvement in this. Even in a model that does not represent latent heat *at all*, the winter soil temperature offset against air temperature is substantially smaller than in models with a poor snow insulation scheme. I would therefore strongly recommend that snow is considered in terms of the simulation of winter soil temperatures. I suggest that at the point where snow depth is discussed in the results (see specific comments, below), an assessment of how well the snow is simulated should be presented with supplementary figures.

R1C2: We agree with the reviewer on the importance of accurately simulated snow conditions (e.g., snow thermal insulation and snow coverage related impact). We have conducted additional experiments with another two reanalysis forcing datasets and also different precipitation-phase (rain or snow) partition methods (PPMs) at both the site scale and regional scale. We will analyze how biases related to snow (i.e., snow depth, snow water equivalent (SWE), and snow coverage) are propagated to biases in soil temperature and soil moisture, and then translated into biases in soil heterotrophic respiration and cold-season CO_2 and CH_4 emissions over pan-Arctic permafrost regions in our following paper (Tao et al., 2021). Please also see our response to Reviewer #2 **R2C1**.

Unfortunately, the snow depth measurements at the tested sites here are quite problematic, showing about 30 cm snow depth during summer times (see raw data by Oechel and Kalhori (2018)). For instance at ATQ, a site that shows the most reasonable snow depth observations, there are suspicious snow depth measurements in summer (upper right panel of Figure R1.1). We also checked Snow Telemetry (SNOTEL) sites but did not find one close enough to our sites for a better comparison. Indeed, continuous snow measurements, especially SWE, are extremely challenging to obtain (Pirazzini et al., 2018; McGrath et al., 2019). Still, as the reviewer suggested, we have added discussion on snow issues (provided below) into the revised manuscript.

Due to lack of reasonable snow depth measurements, we now discuss how snow impacts affected soil temperatures and carbon fluxes through sensitivity analysis (Figure R1.1). Different PPMs result in large discrepancies in the snowfall portion of total precipitation, leading to considerable differences in simulated SWE and snow depth (top figures in Figure R1.1). The sensitivity of soil temperatures to snow depth and SWE is affected by 1) saturation of snow thermal insulation capacity (i.e., the levels of snow thermal insulation will not increase with snow depth if it exceeds an effective snow depth) (Slater et al., 2017); 2) rainfall fraction of total precipitation, which will not only influence snow compaction and accumulation process (thus snow depth) and snow mass (thus SWE), but also severely impact soil water contents; 3) snow coverage, which impacts surface albedo and absorbed solar radiation, outgoing longwave radiation, and thus net radiation; 4) snow thermal conductivity schemes (Tao et al., 2019); and 5) active layer thickness and bottom boundary conditions of soil temperature, which resulted from lumped impacts of snow-or-rain partition over

the long-term period (1901 to 2017 here). Also, the maximum supercooled liquid water content in frozen soils does not dramatically decline with decreases in soil temperature if it drops to a certain level. These effects together cause impacts on zero-curtain periods, heterotrophic respiration (HR), and carbon fluxes. Over the Alaskan Arctic tundra, however, the integrated impact of snow depth on cold-season carbon fluxes is marginal (Figure R1.1). We see much larger snow impacts on carbon fluxes at sites in interior Alaska and other areas over the pan-Arctic permafrost domain, and we will discuss these impacts in our next paper.

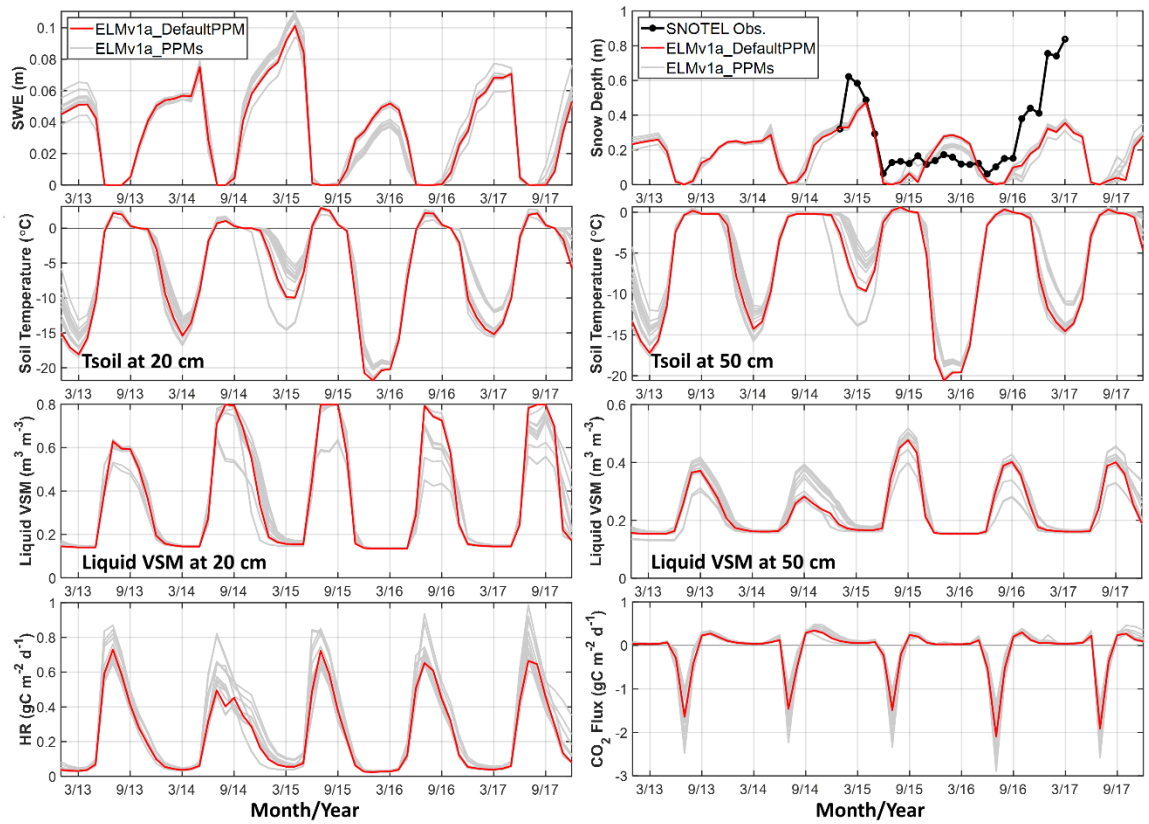

Figure R1.1: Sensitivity of ELM simulated SWE, snow depth, soil temperatures, liquid volumetric soil moisture (VSM), heterotrophic respiration (HR), and CO_2 flux at ATQ to climate forcing (i.e., CRUJRA, CRUNCEP which ends in 2016, and GSWP3 which ends in 2014) and precipitation-phase partition methods (PPMs). Red lines indicate the simulations with default PPM, and grey lines are simulations with different PPMs as tested by Jennings and Molotch (2019). Suspicious snow depth measurements appear during summertime (upper right panel).

3) Justification of the phase change modification. While I have no problem that the main modification to the phase change calculation (allowing temperature to fall below zero during the phase change) is physically sound, I am not so convinced by the phase change "efficiency" parameter that was introduced. This is referenced to some papers where such a parameter was included in a model previously, but those papers are extremely brief in the justification of this and there is no reference to some observation or physical theory. Additional justification is therefore required for this "efficiency" parameter (or removal of this parameter if it is not fully justified).

R1C3: Currently, we employed this phase-change efficiency to effectively solve ELM's problem of overestimating phase-change rates while maintaining the current modeling structure as much as possible. In such a manner, the same modification can be easily adapted by other studies. We agree

with the reviewer that the reference papers for the phase-change efficiency are extremely brief in their justification. Below we provide detailed justification for the phase-change efficiency and the virtual soil temperature; we have added a brief description of this justification to the revised manuscript.

The heat (conduction) transfer equation (Eq. R1.1) can be rewritten as (Eq. R1.2) with soil-water freezing phase change,

$$c\frac{\partial T}{\partial t} = \frac{\partial}{\partial z}\left(\lambda\frac{\partial T}{\partial z}\right), \qquad \text{(Eq. R1.1)}$$

$$c\frac{\partial T}{\partial t} = \frac{\partial}{\partial z}\left(\lambda\frac{\partial T}{\partial z}\right) - L_f\rho_{liq}\frac{\partial \theta_{liq}}{\partial t}, \qquad \text{(Eq. R1.2)}$$

where T is soil temperature (K), c is the volumetric soil heat capacity (J m^{-3} K^{-1}), λ is soil thermal conductivity (W m^{-1} K^{-1}), z is the soil depth (m) of the model soil layers, L_f is the latent heat of fusion (J kg^{-1}), θ_{liq} is soil liquid water content (m^3 m^{-3}), and ρ_{liq} is the density of liquid water (kg m^{-3}). Instead of $-L_f\rho_{liq}\frac{\partial \theta_{liq}}{\partial t}$, We can also use the increasing rate of ice content (θ_{ice}) with opposite sign ($+L_f\rho_{ice}\frac{\partial \theta_{ice}}{\partial t}$) as in the manuscript. Here, we keep $\frac{\partial \theta_{liq}}{\partial t}$ for the discussion, and rewrite (Eq. R1.2) as,

$$\left(c + L_f\rho_{liq}\frac{\partial \theta_{liq}}{\partial T}\right)\frac{\partial T}{\partial t} = \frac{\partial}{\partial z}\left(\lambda\frac{\partial T}{\partial z}\right), \qquad \text{(Eq. R1.3)}$$

By introducing an apparent heat capacity $c_{app} = c + L_f\rho_{liq}\frac{\partial \theta_{liq}}{\partial T}$, (Eq. R1.3) can be rewritten as,

$$c_{app}\frac{\partial T}{\partial t} = \frac{\partial}{\partial z}\left(\lambda\frac{\partial T}{\partial z}\right), \qquad \text{(Eq. R1.4)}$$

which is the same as the heat (conduction) transfer equation (Eq. R1.1) with the actual soil heat capacity c replaced with the apparent heat capacity c_{app}. That is, the apparent heat capacity incorporates the latent heat released by soil water freezing into the actual soil heat capacity.

To solve (Eq. R1.4), we need to compute the derivative of the soil freezing characteristic curve ($\theta_{liq}(T)$) with respect to temperature ($\frac{\partial \theta_{liq}}{\partial T}$). Here, we approximate the $\theta_{liq}(T)$ curve by combining the freezing point temperature-depression equation (Eq. R1.5) (Fuchs et al., 1978) and the soil water retention curve (Eq. R1.6) (Clapp and Hornberger, 1978). This leads to the supercooled water formulation (Eq. R1.7) (also the Eq. A3 in our manuscript) (Niu and Yang, 2006), shown below,

$$\psi(T) = \frac{10^3 L_f(T_f - T)}{gT}, \qquad \text{(Eq. R1.5)}$$

$$\psi(\theta_{liq}) = \psi_{sat}\left(\frac{\theta_{liq}}{\theta_{sat}}\right)^{-B}, \qquad \text{(Eq. R1.6)}$$

$$\theta_{liq}(T) = \theta_{sat}\left[\frac{10^3 L_f(T_f - T)}{gT\psi_{sat}}\right]^{-1/B}. \qquad \text{(Eq. R1.7)}$$

Computing $\frac{\partial \theta_{liq}}{\partial T}$ requests the soil freezing curves $\theta_{liq}(T)$ to be continuous and differentiable for a range of temperatures during the freezing process (Kurylyk and Watanabe, 2013; Hansson et al., 2004). Here, we follow the existing ELM framework to implement Eq. R1.7 in a much simpler way. Specifically, the baseline ELM first solves the heat transfer equation (Eq. R1.1) without consideration of soil water phase change; then it estimates the energy involved (H) for adjusting the initially solved soil temperature to the freezing point (T_f), and then estimates the mass change needed (H_m) based on H. The model then readjusts the soil liquid water, ice content, and soil

temperature according to the actual available liquid water for freezing given the maximum supercooled liquid water allowed under current soil temperature ($\theta_{liq}(T)$, see example in Figure R1.2). The numerical representation for readjusted soil temperature is given by (Eq. R1.8) (or Eq. A5 in the manuscript),

$$T_i^{n+1*}=T_f+\frac{\Delta t}{c_i\Delta z_i}H_{i^*}=T_f+\frac{L_f\left(w_{ice,i}^{n+1}-w_{ice,i}^{n}\right)}{c_i\Delta z_i}. \qquad \text{(Eq. R1.8)}$$

This uncoupled two-step implementation significantly overestimates soil water freezing rates. Two reasons are responsible for the overestimation. First, the freezing point (T_f = 0°C) is used to determine the occurrence of soil water phase change under all conditions. To further freeze supercooled soil liquid water, as Figure R1.2 shows, the soil temperature has to be colder than the virtual soil temperature (Tv), as we introduced by (Eq. A8) in the manuscript. Second, due to the steep slope (especially close to T_f = 0°C) as shown in Figure R1.2, the estimated ice mass increase (i.e., $w_{ice}^{n+1}-w_{ice}^{n}$ or $w_{liq}^{n}-w_{liq,max}^{n+1}$, see (Eq. A5) in the manuscript) most often exceeds the required mass change, i.e., $H_m=-c_i\frac{\Delta z_i}{L_f}\left(T_f-T_i^{n+1}\right)$, and thus soil liquid water freezes quickly in a large chunk. Soon, the liquid water available to be frozen becomes too small to release sufficient latent heat to compensate for the required energy deficit ($T_f-T_i^{n+1}$).

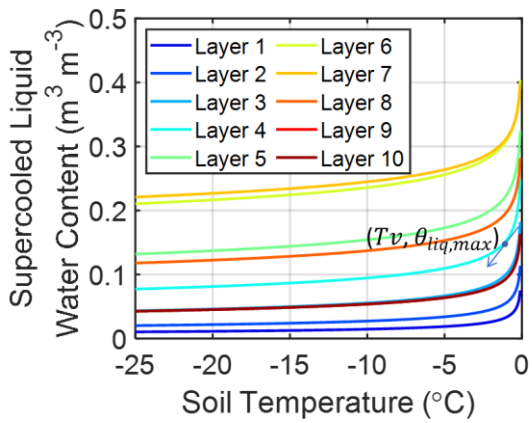

Figure R1.2: Maximum supercooled soil liquid water as a function of soil temperature at BEO.

Thus, through multiplying the initially estimated mass change (H_m) by the phase change efficiency (ε), we replace the freezing point in (Eq. R1.8) with an efficiency-weighted average of the initially solved soil temperature (T_i^{n+1}) and the freezing point,

$$\begin{aligned} T_i^{n+1*} &= T_f+\frac{\Delta t}{c_i\Delta z_i}\left(-c_i\frac{\Delta z_i}{\Delta t}T_{inc}\varepsilon_i+\frac{L_f\left(w_{ice,i}^{n+1}-w_{ice,i}^{n}\right)}{\Delta t}\right) \\ &= T_f-\left(T_f-T_i^{n+1}\right)\varepsilon_i+\frac{L_f\left(w_{ice,i}^{n+1}-w_{ice,i}^{n}\right)}{c_i\Delta z_i} \qquad \text{(Eq. R1.10)} \\ &= (1-\varepsilon_i)T_f+\varepsilon_i T_i^{n+1}+\frac{L_f\left(w_{ice,i}^{n+1}-w_{ice,i}^{n}\right)}{c_i\Delta z_i}. \end{aligned}$$

The two changes effectively improve the soil water freezing process simulations and prevent soil becoming irreversibly too cold quickly as simulated by the baseline phase change scheme. We will rework our Appendix and methodology section and better develop the elaboration for our modification. In the future, we will bypass this two-step implementation by solving (Eq. R1.4) in a coupled mode using the Apparent Heat Capacity Parameterization (AHCP) method (Kitover et al., 2016; Rawlins et al., 2013; Wang et al., 2010; Nicolsky et al., 2007; Hinzman et al., 1998;

Endrizzi et al., 2014; Guymon et al., 1980; Mottaghy and Rath, 2006), along with our incorporation of advective heat transfer.

4) Environmental modifiers. The soil moisture function is modified in two ways: 1 to decrease respiration at high water contents, which replaces the oxygen-availability modifier, and 2 to continue to have respiration at zero water contents. Firstly, why would you replace the process-based oxygen availability with an empirical function that would presumably represent the process less well? And secondly, setting non-zero respiration at zero water contents is dubious and the functions chosen (shown in Fig S1) are strange-looking. There is no evidence that respiration occurs at zero water contents, rather that it continues over winter because there is non-zero liquid water in frozen soil. It would make much more sense to change the function so that it reaches zero at zero water contents, i.e. shift the curves to the left. This would also look a lot more like existing literature, e.g. Yan et al 2018. So, I am not convinced by these modifications.

R1C4: Unfortunately, the process-based oxygen availability estimated by ELMv1 shows some instabilities, i.e., unrealistic abrupt jumps between 0 and 1. We will dig into this issue and further explore the reasons causing these instabilities. At this point, we believe using the empirical function is a good compromise solution to the instability problem.

Regarding the reviewer's comment: "setting non-zero respiration at zero water contents is dubious and the functions chosen (shown in Fig S1) are strange-looking", we feel the reviewer might misinterpret the figure. To be clear, we did not "set non-zero respiration at zero water contents". Indeed the model never reaches a zero liquid water content (although it can become close to zero) even under a very cold condition because of the supercooled liquid water coexisting with ice in frozen soils (Niu and Yang, 2006). Examples are shown in Figure R1.1. This result is consistent with studies reporting considerable microbial respiration even when the soil temperature is below -20°C (Natali et al., 2019; Zona et al., 2016).

We do agree with the reviewer about the strange-looking moisture functions, and we thank the reviewer's suggestions of shifting the curves to the left, but we plan to incorporate more sophisticated moisture functions proposed by our group (Tang and Riley, 2019). Also, soil liquid water contents cannot be smaller than the maximum supercooled liquid water content allowed in frozen soils, which does not dramatically decline with decreases in soil temperature when the soils become too cold, as discussed above **(R1C3**). Thus, the sensitivity of soil heterotrophic respiration to moisture functions for small soil liquid water contents (that usually occur in frozen soils) is small and would not impact our results and conclusion very much.

5) CH4 cold season transport. Again this is poorly justified and the equations are missing. In the appendix line 700-702 it states that "We integrate the emissions from ice cracks and remnants of aerenchyma tissues with (Eq. C14) by removing temperature limitation and applying a small Taere during winter time" Firstly, there was no mention of temperature limitation, so what does this part refer to? Secondly, what is "a small Taere"? (ie what is the value and why did you choose it?). It would really be useful to give the equation that you use in the model, instead of just this unclear description. In the methods it is justified by reference to a paper that there could be more conduction of methane through snow. However, the change made to the diffusion inside the soil is set as an arbitrary value and a sufficient justification is not given for changing it. Potentially, increasing Taere would increase the emissions enough (if you choose the right value) that this non-justified change to diffusion rate would not be needed.

R1C5: In the baseline model, methane emissions through aerenchyma when the soil temperature is below 0°C are set to zero. We have added detailed elaboration (including all relevant equations, parameters, and description) regarding the methane transport mechanism into the Appendix.

Also, the reviewer is correct about the changes to diffusion within frozen soils. We assigned the small value from our sensitivity analysis, which, however, happened before we modified "Taere". We now have reconducted the sensitivity analysis on this particular change to diffusion and found no significant impacts to our results. This is because, as the reviewer pointed out, emissions through ice cracks and remnants of aerenchyma tissues dominated the total emissions. Hence, we will remove this modification in the revised manuscript, which does not affect our results and conclusions.

6) Missing out of IVO. It is shown that IVO cannot simulate reasonable methane emissions even with the correct temperature and moisture, however CO2 emissions did not suffer from this problem. Therefore I don't fully understand why CO2 from IVO cannot be included in the analysis. The optimisation would have to be done only on CO2 but it could still be optimised, is that right?

R1C6: Thanks. We will add back the CO_2 results at IVO (shown by the figures below). We also have changed the line styles and colors as suggested by the reviewers, and we will make all the figures consistent in plotting styles (see Figures R1.3 and R1.4).

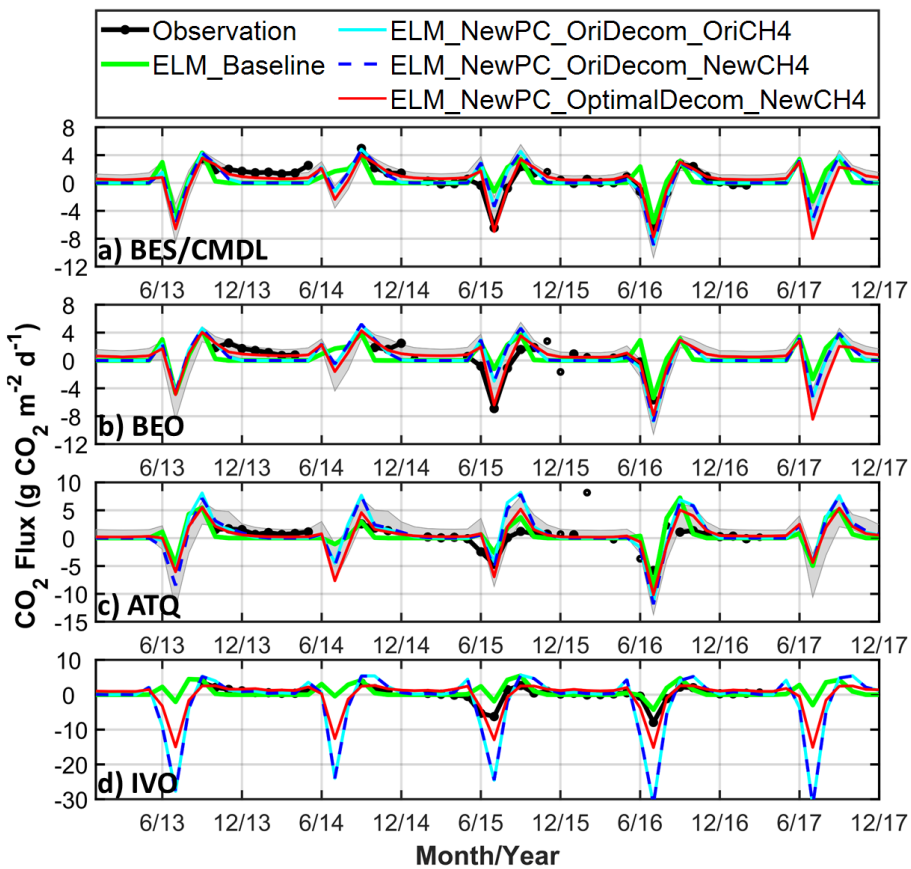

Figure R1.3: Same as the right panel of Figure 8 in the manuscript, which now includes CO_2 results at IVO. For a better illustration, we now have removed two simulations that we did not extensively discuss in the manuscript, i.e., 'ELM_NewPC_NewDecom_NewCH4_EnMean' and 'ELM_NewPC_GenericDecom_NewCH4'.

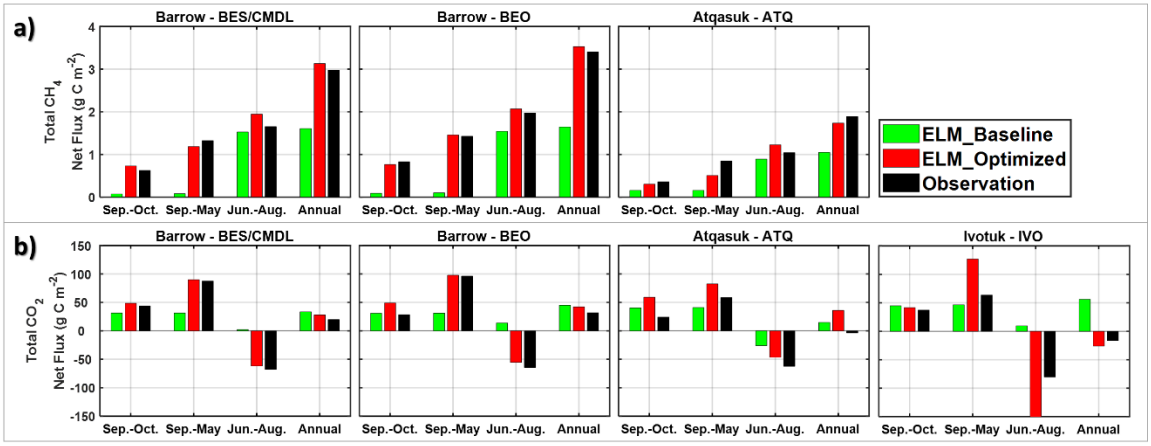

Figure R1.4: (New Figure 10 in the manuscript.) Multi-year (2013-2017) averaged total CH_4 emissions and CO_2 net fluxes during the early cold season (Sep. and Oct.), cold-season period (Sep. to May), warm-season period (Jun. to Aug.), and the annual cycle (Sep. to Aug.) at our study sites.

7) Additional analysis: For further investigation of the temperature/moisture functions in frozen conditions, you could plot the emissions against temperature and moisture, instead of over time. Then you can see if the models and observations are producing similar functions. (This would show up, for example, if there is hysteresis in the observations which would make it difficult for any single function to capture the dynamics, and would be worthwhile to know.)

R1C7: We had checked the emissions vs. temperature and moisture and found hysteretic dynamics at IVO, as indicated by the reviewer. Examples are shown below (Figure R1.5) and in the revised supplementary material (Figure S.X). In the future, we will apply a Macromolecular Rate Theory (MMRT)-based temperature sensitivity approach developed by our colleagues (Chang et al. 2020, 2021) to address the hysteresis effect. As mentioned above, we will also employ more sophisticated moisture functions proposed by our group (Tang and Riley, 2019).

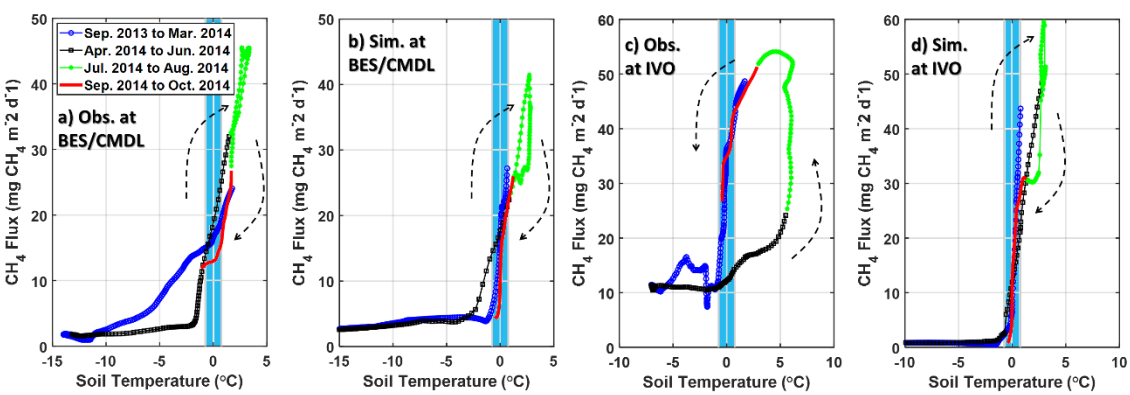

Figure R1.5: (A similar figure as Fig.3 in Zona et al. (2016)). Daily CH_4 emissions vs. soil temperatures at 12 cm at two sites. Similarly, as in Zona et al. (2016), we applied a 30-day averaging window to smooth the daily data to produce clear seasonal progressions. Shaded blue areas indicate zero-curtain periods, i.e., [−0.75 °C, 0.75 °C]. At BES/CMDL and IVO, observed seasonal progressions proceed in opposite directions (e.g., from black to green and then to red), while modeled seasonal progressions follow the same clockwise direction.

Specific comments
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Abstract

Line 16-17 "simulated cold-season emissions at three tundra sites were improved by 84% and 81%" - it is not clear what metric the 84% and 81% refer to, is this the mean absolute error? Please specify.

R1C8: Thanks for pointing out this confusion. We will revise this sentence as below. (We now have mentioned "MAE" earlier in the abstract.)

"Furthermore, the MAEs of simulated cold-season emissions at three tundra sites were improved by 84% and 81% on average for CH_4 and CO_2, respectively."

Line 17-19 "...zero-curtain period in Arctic tundra, accounted for more than 50% of the total emissions" This statement is slightly misleading. This is the case in the model, but the study showed that this part was overestimated compared to the observations. I would therefore add something like "in the model, compared with around 45% (30-60%) in the observations"

R1C9: Thanks for the suggestion. The simulated contribution of the early cold season (Sep. and Oct.) CH_4 emissions to the cold-season total were 62%, 52%, and 60% for the three sites, in comparison with the observed 47%, 58%, and 43%, showing slight model overestimations. The released CO_2 over the early cold season accounted for 54%, 50%, and 72% of the total emissions throughout the cold season for the three sites, but we do not have continuous CO_2 observations to calculate the counterpart contribution.

We thus will revise this sentence as below.

"Overall, CH_4 and CO_2 emitted during the early cold season (Sep. and Oct.), which often includes most of the zero-curtain period in Arctic tundra, accounted for more than 50% of the total emissions throughout the entire cold season (Sep. to May) in the model, compared with around 49.3% (43-58%) in CH_4 observations."

Introduction

Lines 60-62 " However, current land models tend to significantly underestimate soil temperature during the cold season over permafrost regions (Dankers et al., 2011; Tao et al., 2017; Nicolsky et al., 2007; Yang et al., 2018b). One possible reason is that many land models fail to appropriately account for the latent heat released during soil water freezing" It is true that many land surface models did underestimate soil temperatures but, more recently, improved snow schemes have removed a lot of this problem. For example, your first reference Dankers et al (2011) has a followup study Burke et al (2013) which includes a multilayered snow scheme and removes the majority of the winter cold bias - although a small cold bias remains. I highly recommend adding some discussion of snow here to make it clear that the latent heat is not the only (or even the biggest) factor. Most recent LSM's (e.g. in CMIP6) do represent latent heat, if not particularly well, I suggest clarifying that to "One possible reason is that while many land models account for latent heat released during soil water freezing, they do not treat and distribute this heat appropriately"

R1C10: Thank you very much for the suggestion. We will change the sentences as the reviewer suggested.

"One possible reason is that while many land models account for latent heat released during soil water freezing, they do not treat and distribute this heat appropriately or/and do not simulate soil moisture correctly."

Lines 69 "many land models cannot accurately capture the ZCP length due to their underestimation of soil temperatures" This is not really an accurate statement. Many land models cannot accurately capture the ZCP length (true), but this is because they don't have enough soil moisture or an adequate representation of latent heat, not "due" to underestimated temperatures. Rather, underestimated temperatures can arise as a *result* of not simulating the ZCP.

R1C11: We agree with the reviewer on this point and have modified the sentence as below.

"Nevertheless, many land models cannot accurately capture the ZCP length due to inaccurately simulated soil moisture and/or inadequate representation of latent heat, thus underestimating soil temperature and cold-season CO_2 emissions (Commane et al., 2017) and CH_4 (Zona et al., 2016)."

Study sites and dataa

Line 93. " CARVE CO2 measurements were not available;" should this be "...were not available from 2015-2017;" ? Currently this part is unclear.

R1C12: To accurately clarify this, we will revise the sentence as below.

"The CARVE CO_2 measurements were not available at the data archive we used here; therefore, monthly winter-time CO_2 flux data at the same towers assembled by Natali et al. (2019) are included to complement CO_2 observations from 2013 to 2014."

Line 116-117 "Due to the discontinuity of observed soil moisture over time and along with the vertical depth, evaluating ELMv1-ECA simulated liquid water content at layer node-depth was limited." This sentence does not make sense to me, please clarify.

R1C13: We will modify the sentence as below.

"The observed soil moisture is only available at two or three depths that are quite different from model layer node-depths, and also show discontinuities in time. Thus, evaluating ELMv1-ECA simulated liquid water content was limited."

Methodology

Line 152-153 "The underlying assumption here is that the liquid water of soil resists freezing as the freezing process proceeds and Sfnliq,i decreases, analogous to how dry soils resist getting drier due to capillary force." This is the explanation given for the efficiency factor (see comment 3, above): However, this capillary force in freezing soils is represented by the non-zero liquid water contents at sub-freezing temperatures, and it is not clear to me that it needs an additional factor. The efficiency factor, I guess (although it is not clear what it actually does - see next comment) corresponds to a 'loss' of some of the energy produced by latent heat. It does not make sense that energy would just disappear. Please explain/justify.

R1C14: Thanks. Please also see our response **R1C3**. We will remove or rephrase this sentence as we incorporate our description in **R1C3** to the manuscript.

Line 153-154 "We applied the phase change efficiency to the initially estimated energy and mass change involved, i.e., H_i and H_m (see (Eq. A4) in the Appendix)". It is not clear what 'applied' means here, did you multiply some part of these equations by the efficiency factor? The easiest thing to do would be to include the equations in the Appendix that you actually used (i.e. rewrite those equations with the efficiency factor included, instead of leaving it to our imagination).

R1C15: We multiply the initially estimated energy and mass change involved (see (Eq. A4)) by the phase change efficiency (see (Eq. A7)). We will modify our Appendix by incorporating all the equations related to the changes. Please also see our response to **R1C3**.

Table S2: I am missing where the moisture functions ModifiedELMâˇARˇ S1 ModifiedELMâˇARˇ S2, etc are documented?

R1C16: We thank the reviewer for pointing this out. We mentioned in the manuscript that these moisture functions differ from each other in the values of parameters b, Sf_{op}, and f_{W_min} (Eq. B10). We will add another table in the supplementary to list these values.

Line 264-266 "We confirmed that ELMv1-ECA's PFT dataset was a good compromise between representing the site-scale ecosystem and other global parameters and surface datasets within ELM. " Firstly, what is ELMv1-ECA's PFT dataset? This is not mentioned. Secondly, how did you assess whether it was 'good'? I recommend adding more information here.

R1C17: The ELMv1-ECA's PFT dataset was derived from satellite-based data (i.e., MODIS - the Moderate Resolution Imaging Spectroradiometer) data by Lawrence et al. (2007). We will add this information to the revised manuscript. Also, as mentioned in the manuscript, we tested different PFT datasets derived from a detailed vegetation survey at ABoVE flux tower footprints obtained in 2014 (Davidson and Zona, 2018). We found that these PFT datasets generally are not superior to the original PFT dataset (**Figure R1.6**). We thus decided to retain the original PFT dataset.

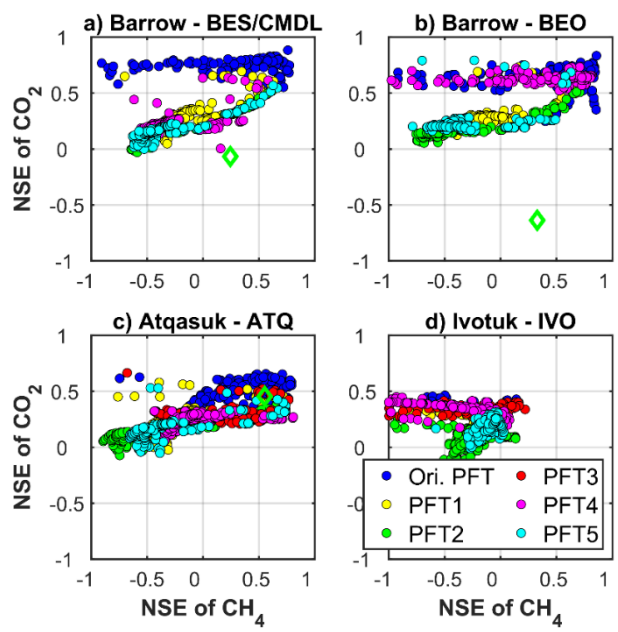

Figure R1.6: Scatter plot between the Nash–Sutcliffe Efficiency (NSE) of simulated monthly CH_4 and CO_2 fluxes. Colors indicate different PFTs tested here. Diamonds represent baseline simulation.

In addition to vegetation (PFTs) and related parameters, other surface datasets, including soil colors, soil organic matter content, soil sand and clay percent, topography, slope, and inundation-related parameters, etc., all need to customize for an ideal site-level simulation. In the future, we plan to embed an advanced calibration tool, i.e., the Shuffled Complex Evolution method at the University of Arizona (SCE-UA), into the ELM framework. This coupled ELM-SCE framework will allow flexible calibration for both site-scale and spatial simulations and have the capacity of multi-objective optimizations, i.e., against observations of soil temperature, soil moisture, and CH_4 and CO_2 fluxes. That is, wrapping all the procedures conducted in this work into a program package that can automatically repeat our work here with limited manual interventions and thus can efficiently optimize surface datasets at desired resolutions from site-scale to the global scale.

Line 266-267 "The simulated saturated and unsaturated CH4 emissions were weighted with the estimated inundation fractions at the footprint of ABoVE eddy-covariance flux towers" Surely the *unsaturated CH4 emissions should be weighted with the *noninundated fraction in the footprint? I guess this is probably what you did, it's just not written very clearly, it currently sounds like both saturated and unsaturated CH4 emissions were multiplied by the inundated fraction.

Readers will not know that the model simulates methane separately from saturated and unsaturated grid cell fractions, therefore I suggest making that point here.

R1C18: Thanks for pointing this out. We will modify the sentence as below.

"The surface CH_4 emission is a weighted average of simulated saturated and unsaturated components using predicted inundation and non-inundation fractions. To compare simulated CH_4 emissions with ABoVE measurements at the site scale, we use the estimated inundation fractions at the footprint of ABoVE eddy-covariance flux towers (see details in (Xu et al., 2016))."

Results

Line 326-343 Here you talk about the improvement to the ZCP. Looking at the plots, there is a great improvement in deeper soil layers but not so much in the surface (for 3/4 sites). I suggest that the text should recognise this fact about the surface being less well simulated.

R1C19: We will add here an additional sentence at the end of this paragraph.

"In general, the improvements in ZCP are larger in deeper layers than topsoils with the top layer showing only marginal improvement."

Line 346 Reference to Figure 3 should be Figure 4.

R1C20: Thanks. We have changed this.

Line 357-358 "The deeper active layer simulated by NewPC implies more soil water storage capacity, resulting in lower soil moisture in shallow soil layers and higher soil water in deep layers" This pattern is not really seen with most of the sites, either the new simulation seems to have lower soil moisture in general, or in the case of IVO it is greater or similar in almost every layer in the new simulation. There is also the claim of soil moisture being improved - this is true because the timing of thaw and freezeup is better, but actually the level of saturation in general seems to now be lower and in several cases the old scheme was better in that regard. This is just a suggestion, but I am aware of more than one land surface scheme that has found their scheme of dealing with saturation of soil moisture leads to water being forced out of the top of the soil during the freeze-

up period. I was just wondering if simulating a longer ZCP might lead to more water being lost in this way, and would therefore explain why the new model is drier. There are several possibilities, of course!

R1C21: The "claim of soil moisture being improved" was made based on the RMSE of simulated soil liquid water content (Table S3). The pattern (i.e., lower soil moisture in shallow soil layers and higher soil water in deep layers) is shown in Figure 3 by the magenta vs. green lines during summertime when the active layers reach the deepest thaw depths.

Our model should be able to simulate the mechanism of "water being forced out of the top of the soil during the freeze-up period", given the differences in porosity and ice volume. But at the tested sites, soils usually either are unsaturated or get drained before the freeze-up period, and thus we did not find that mechanism to be the case here.

Line 383-384. As I discussed above, the snow is important and I suggest that this is the place to present some additional analysis rather than simply referring to "underestimated snow depth (not shown)".

R1C22: Thanks. We will add the figure included in our response **R1C2** into the supplementary file and add the relevant discussion. Please also see our response to Reviewer #2 regarding this point, i.e., **R2C1.**

Line 416, having checked that using observed soil moisture and temperature does not improve the CH4 simulation, saying that including advective heat transport would likely improve the simulations is surely incorrect, since this would just improve the soil temperature, which you found did not help. I would also be surprised if a better wetland simulation would help if using observed soil moisture did not improve the simulation. Geological seepage is certainly a possibility though.

R1C23: Due to discontinuities in soil moisture observations, as we have discussed, imposing soil moisture to the model is quite tricky. Unless through a well-designed data assimilation method, the control experiments still cannot correctly simulate surface standing water and wetland inundation dynamics. We agree with the reviewer that the advective heat transport might play a lesser role here, and thus we will remove this factor from the statement.

Line 420-423. This is missing the information that the performance of CH4 is degraded at the BES/CMDL and BEO sites. It's somewhat misleading to only mention the improvements.

R1C24: We do not think the degraded CH4 performance here matters very much since we only changed the phase change scheme but kept the carbon decomposition and methane modules the same as the baseline which we know are problematic. Improved ZCP durations and soil temperature and moisture do not necessarily guarantee improvements in CO_2 and CH_4 given problematic carbon decomposition and methane modules. We feel our discussion here is not misleading. The original sentences are copied below,

"The improved phase-change scheme, and thus improved simulations of ZCP durations and soil temperature and moisture, resulted in greatly improved performance for CO_2 emissions at BES/CMDL and BEO, and slightly better performance for CH_4 emissions at ATQ, compared to the baseline (cyan for "NewPC_OriDecom_OriCH4" vs. green for baseline; Figure 7), even though the carbon decomposition and methane modules remained the same. Incorporating the revised CH_4 model (discussed in section 3.1.3) improved simulated CH_4 emissions at BES/CMDL, BEO, and ATQ (blue for "NewPC_OriDecom_NewCH4" vs. cyan for "NewPC_OriDecom_OriCH4"), especially during the cold season (Figure 8)."

Line 442-444. This part is unclear. When you say "soil properties", do you mean soil thermal/hydraulic properties? And is the improvement of the ELMv1-ECA's moisture scalars due to the function being based on the suction rather than the volumetric soil moisture content? Please clarify this. Also, please give evidence that ELMv1-ECA "reasonably explained the varying influence along the vertical soil profile".

R1C25: Soil properties here mean the soil texture-dependent porosity and saturated soil water potential; and yes, the reviewer is correct that ELMv1-ECA uses soil suction rather than soil moisture content for moisture scalars. Over high-latitude peatlands, soil porosity varies with depth due to vertically stratified organic matter contents, which leads to differences in soil pore spaces and substrate affinity. This is a key strength of ELMv1-ECA's moisture scalars that differs from other models' moisture scalars. The Fig. 1 in Niu and Yang (2006) is good evidence supporting that ELMv1-ECA "reasonably explained the varying influence along the vertical soil profile". We will revise the sentence as below.

"For the Sierra et al. (2015) empirical moisture functions, the influence of liquid moisture content on heterotrophic respiration is uniformly applied to all active soil layers, even though the soil properties (e.g., porosity and saturated soil water potential) are quite different vertically. ELMv1-ECA's moisture scalars (including the original scheme) that use soil water potential, in contrast, reasonably explained the varying influence along the vertical soil profile (Niu and Yang, 2006). "

Line 444 "Thus, the simulations..." I suggest removing 'thus' because overestimation isn't implied from the previous sentence.

R1C26: Thanks. We will remove "thus" here.

Line 450 "assigns small thresholds for the moisture scalar" this is unclear. Did you mean "assigns small minimum values for the moisture scalar"?
Line 451: Same problem as 450.

R1C27: Yes, we will change "small thresholds" to "small minimum values (f_{W_min})" as the reviewer suggested.

Line 457-459 ", at ATQ, where cold-season temperatures are relatively warmer than at BES/CMDL and BEO, simulations with the original ELMv1-ECA environmental modifier (i.e., "NewPC_OriDecom_NewCH4"; discussed in Section 3.1.2), already released much more CO2 and CH4 throughout the cold season than in the baseline simulations, " Can you add a reference to a Table or Figure that shows this happens more at ATQ than the other sites? It isn't very clear to me on Figure 8. And in fact on Figure 6 (c3 and c4), it looks like the cold season production of CO2 and CH4 still goes to zero at ATQ with NewPC_OriDecom_NewCH4.

R1C28: We have modified Figure 8 as the reviewer suggested (see **Figure R1.3**), and with the revised figure, it should be clearer. In Figure 6 (c3 and c4), the NewPC_OriDecom_NewCH4 simulated CO2 and CH4 still goes to zero after the ZCP ends, but the cold-season emissions are still larger than the baseline result (Figure 6 b3 and b4) which has a much shorter ZCP.

Line 471. vary -> varies

R1C29: We have changed it as suggested.

Line 471-474 " For cold sites (i.e., BES/CMDL and BEO), the sensitivity of simulated CH4 to Q10 values is larger than the sensitivity of CO2 net flux to Q10 because cold temperature suppresses

vegetation growth (i.e., CO2 uptake); while for the warm site (i.e., ATQ), both CH4 and CO2 net flux are very sensitive to the Q10 values." Can you refer here to some numbers/figures that show this? I see that in Table S4 this is apparent if you compare the lines with the same soil moisture function but different Q10's.

R1C30: We will include the figure below in the supplementary file to support this statement.

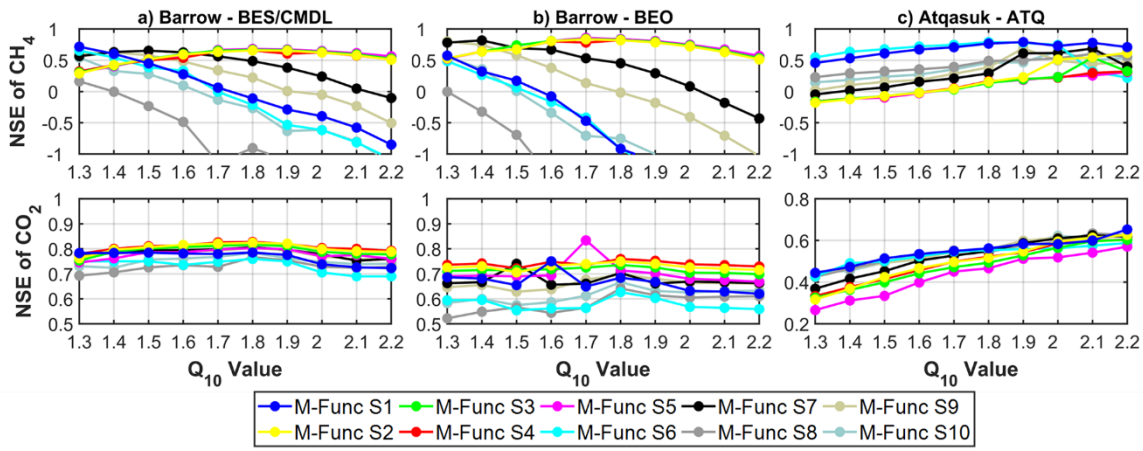

Figure R1.7: Performance of simulated CH_4 and CO_2 varies with soil moisture functions and Q10 values. Note the different y-axis scales of NSE of CO_2 at ATQ from the other two sites.

Line 485/Section 4.3. I suggest you start this section with a clarification that throughout this section you are analysing the results with optimal decomposition scheme for each site (and therefore different parameters are used for each site).

R1C31: Thanks for the suggestion. We will add a sentence as below.

"Throughout this section, we only retain and discuss the identified optimal simulation results (i.e., NewPC_OptimalDecom_NewCH4) for each site."

Line 496 slightly -> slight

R1C32: Thanks. We have revised this.

Line 511-512 "We find that the simulated cold-season CO2 emissions were larger than the warm-season CO2 net uptake at all three sites" Please specify during which time period. (Presumably they are in balance during the spinup, but will become out of balance later in the simulation due to changing climate, so it makes sense to note the time period here)

R1C33: Thanks. We will modify the sentence as below.

"We find that the simulated cold-season CO_2 emissions were larger than the warm-season CO_2 net uptake during the analyzing period (2013-2017) at all three sites (Figure 10, Table 5)."

Summary

I suggest that you additionally mention the potential issues of using the heterotrophic respiration to estimate CH4 production. For example, this means that CH4 emissions may drop as the soil

becomes more saturated (once soil moisture passes the optimum), whereas in fact the highest CH4 emissions should be in saturated conditions.

R1C34: Thanks. We will add the discussion in the summary section as suggested.

Line 529 "by updating upper boundary resistance" Was this the only change? What about the change you made to the diffusion through the soil? I don't think that is related to the upper boundary? Please check this to make sure it's summarizing accurately.

R1C35: We apologize for missing information here. We will revise the sentence as below.

"We further refined the cold-season methane processes by mimicking emission pathways through ice cracks and remnants of aerenchyma tissues and reducing upper boundary (snow) resistance that allows CH_4 to be emitted from frozen soils through the snow to the atmosphere."

Line 546 "the identified an" -> "the identified"

R1C36: Thanks. We will revise this as suggested.

Line 562 add "due to microbial dynamics" or similar, for clarity

R1C37: Thanks. We will revise the sentence as below.

"In addition, the single static multiplicative function used to parameterize the impact of environmental conditions on respiration might not be appropriate, because the environmental impact also depends on maximum respiration rate, soil texture, soil carbon content, and microbial biomass (Tang and Riley, 2019)."

Line 571-573 "The increasing rate of cold-season heterotrophic respiration (releasing CO2) may become larger than the trend of warm-season vegetation CO2 uptake under future climate" In fact in your simulations, the cold season respiration already became larger than the warm season CO2 uptake by 2017, is that right? This point could be made stronger with that information.

R1C38: Thanks. We are discussing the increasing rates here, not the absolute magnitudes. In terms of net annual CO_2 budgets, the observations at ATQ and IVO still indicate net uptake (see **Figure R1.4**). In our following work (Tao et. al., 2020), we indeed found that the modeled increasing trend of Alaskan Arctic tundra warm-season net CO_2 uptake still exceeds the cold-season net CO_2 emissions increasing trend during 1950 to 2017 (without consideration of wildfire, abrupt permafrost thaw, insects, etc.), despite cold-season soil temperatures warming three times as fast as warm-season soil temperatures.

Appendix Eq. A3. What does * mean in this equation? Eq. A7. as already discussed in my comments on the Methodology, you need to show where/how these factors are applied in the model - via equations would be easiest.

R1C39: The * in Eq. A3 (and A5) is a typo and we will remove it. The variables with * mean the ultimately adjusted variables (e.g., T_i^{n+1*}). We will also rework our Appendix to make everything consistent and include all the relevant equations. Please also see our response in **R1C3**.

Line 642. This equation isn't entirely consistent with equation A3, the 10^3 is on the bottom and g is missing. Can you check both of these?

R1C40: That is because of different units used for soil water potential (mm or mb or MPa), but both equations are correct. We will make the equations consistent in the revised manuscript.

Line 692. It is not entirely clear what A(z) represents, is this the total methane emission to the atmosphere or just the part from aerenchyma?

R1C41: We will revise the sentence as below.

"Vascular plant aerenchyma tissues serve as diffusive pathways to transport CH_4 from soil to the atmosphere. The CH_4 transport via aerenchyma from soil layer z ($A(z)$, mol m^{-2} s^{-1}) is calculated as"

Line 700: Apologies if this is common knowledge but I don't know what "amount of carbon per tiller" means. Is this correct?

R1C42: This is described in Wania et al. (2010) and we will add this reference here. For clarification here, we will revise the sentence as below.

"and the factor 0.22 represents average observed tiller biomass (gC per tiller) (Wania et al., 2010; Schimel, 1995)."

Line 700-702: This needs more explanation/equations, see comment (5) in general comments, above.

R1C43: We will revise this as suggested. Please also see our response **R1C5**.

Line 710: "Table 2" should be Table 1, I think

R1C44: Yes, thanks. We will change it.

Line 710: Please specify which parameter in the equations you are changing. The table refers to it as "scale_factor_gasdiff_snow" and it's not clear where this fits in Eq. C13 (if it all)

R1C45: This modification was made to the upper boundary layer resistance, and we will also add the relevant equations in the Appendix.

Line 711: presents -> is present.

R1C46: Thanks. We will revise this as suggested.

Figures

General: Firstly, it is common to plot the observations in black and the model versions in colours (or at least use a different style of line), which I would recommend here since it would add clarity to the plots. Secondly, there appears to be a slight difference in the CO2 when the methane transport modifications are introduced, particularly for ATQ. I did not see any way that the methane transport would influence the CO2 simulation - could you explain this difference?

R1C47: We have replotted our figures according to reviewers' suggestions, i.e., using black for observations and other colors for simulations with different line styles. An example is shown in **Figure R1.3**.

Regarding the second point, changes in methane transport do not influence CO_2 predictions (see blue and cyan lines in **Figure R1.3)**.

Figure 1 is not super clear which labels are refering to which sites, since there are more labels than red dots. Could you add lines or arrows to indicate for certain which site is in which location.

R1C48: We have modified this figure as suggested and will replace the original Figure 1 with the new one.

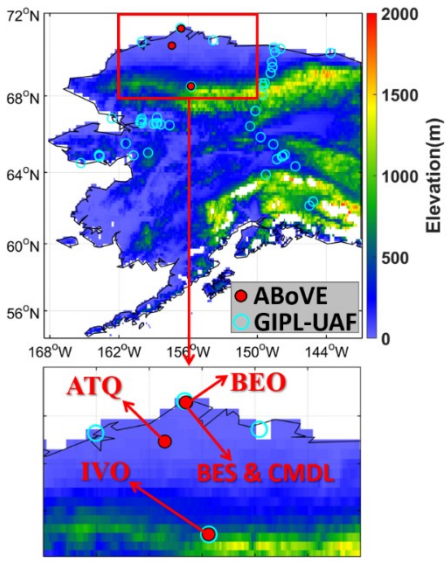

Figure R1.8: New Figure 1 with lines and arrows.

Figure 2 is a bit of a mess and it does not seem to be logically organised. For example, the split between green circles appears to be between methane and "every other form of carbon", perhaps it would make sense to separate vegetation and soil (non root) carbon? Most of the arrows are brown and seem to represent "some kind of influence". To me it is important to show the flows of carbon between the different spheres, some of which is shown in black (e.g. CO2 emission), some in brown (e.g. heterotrophic respiration producing CH4), and some not shown at all, such as the flow of carbon between plants and soil. This diagram needs to be revisiting to get a complete and coherent presentation.

R1C49: After careful consideration, we decided to remove Figure 2.

Papers mentioned

Burke et al 2013 https://link.springer.com/article/10.1007%252Fs00382-012-1648-x
Burke et al 2020 https://tc.copernicus.org/articles/14/3155/2020/
Brooks et al 2004 https://onlinelibrary.wiley.com/doi/full/10.1111/j.1365-2486.2004.00877.x
Strom et al 2012 https://www.sciencedirect.com/science/article/abs/pii/S0038071711003385
Yan et al 2018 https://www.nature.com/articles/s41467-018-04971-6

References

Chang, K. Y., Riley, W. J., Crill, P. M., Grant, R. F., and Saleska, S. R.: Hysteretic temperature sensitivity of wetland CH4 fluxes explained by substrate availability and microbial activity, Biogeosciences, 17, 5849-5860, 2020.

Chang, K. Y., Riley, W. J., Knox, S. H., Jackson, R. B., and al., e.: Substantial hysteresis in emergent temperature sensitivity of global wetland CH4 emissions, (Under Review), 2021.

Clapp, R. B., and Hornberger, G. M.: Empirical equations for some soil hydraulic properties, Water Resources Research, 14, 601-604, 1978.

Commane, R., Lindaas, J., Benmergui, J., Luus, K. A., Chang, R. Y. W., Daube, B. C., Euskirchen, E. S., Henderson, J. M., Karion, A., Miller, J. B., Miller, S. M., Parazoo, N. C., Randerson, J. T., Sweeney, C., Tans, P., Thoning, K., Veraverbeke, S., Miller, C. E., and Wofsy, S. C.: Carbon dioxide sources from Alaska driven by increasing early winter respiration from Arctic tundra, P Natl Acad Sci USA, 114, 5361-5366, 2017.

Davidson, S. J., and Zona, D.: Arctic Vegetation Plots in Flux Tower Footprints, North Slope, Alaska, 2014, ORNL DAAC, Oak Ridge, Tennessee, USA. https://doi.org/10.3334/ORNLDAAC/1546, 2018.

Endrizzi, S., Gruber, S., Dall'Amico, M., and Rigon, R.: GEOtop 2.0: simulating the combined energy and water balance at and below the land surface accounting for soil freezing, snow cover and terrain effects, Geosci Model Dev, 7, 2831-2857, 2014.

Fuchs, M., Campbell, G., and Papendick, R.: An analysis of sensible and latent heat flow in a partially frozen unsaturated soil, Soil Sci Soc Am J, 42, 379-385, 1978.

Guymon, G. L., Hromadka, T. V., and Berg, R. L.: A One Dimensional Frost Heave Model Based Upon Simulation of Simultaneous Heat and Water Flux, Cold Reg Sci Technol, 3, 253-262, 1980.

Hansson, K., Simunek, J., Mizoguchi, M., Lundin, L. C., and van Genuchten, M. T.: Water flow and heat transport in frozen soil: Numerical solution and freeze-thaw applications, Vadose Zone J, 3, 693-704, 2004.

Hinzman, L. D., Goering, D. J., and Kane, D. L.: A distributed thermal model for calculating soil temperature profiles and depth of thaw in permafrost regions, Journal of Geophysical Research-Atmospheres, 103, 28975-28991, 1998.

Hugelius, G., Tarnocai, C., Broll, G., Canadell, J. G., Kuhry, P., and Swanson, D. K.: The Northern Circumpolar Soil Carbon Database: spatially distributed datasets of soil coverage and soil carbon storage in the northern permafrost regions, Earth Syst Sci Data, 5, 3-13, 10.5194/essd-5-3-2013, 2013.

Jennings, K. S., and Molotch, N. P.: The sensitivity of modeled snow accumulation and melt to precipitation phase methods across a climatic gradient, Hydrol Earth Syst Sc, 23, 3765-3786, 2019.

Kitover, D. C., van Balen, R. T., Vandenberghe, J., Roche, D. M., and Renssen, H.: LGM Permafrost Thickness and Extent in the Northern Hemisphere derived from the Earth System Model iLOVECLIM, Permafrost Periglac, 27, 31-42, 2016.

Koven, C. D., Riley, W. J., Subin, Z. M., Tang, J. Y., Torn, M. S., Collins, W. D., Bonan, G. B., Lawrence, D. M., and Swenson, S. C.: The effect of vertically resolved soil biogeochemistry and alternate soil C and N models on C dynamics of CLM4, Biogeosciences, 10, 7109-7131, 2013.

Kurylyk, B. L., and Watanabe, K.: The mathematical representation of freezing and thawing processes in variably-saturated, non-deformable soils, Adv. Water Resour., 60, 160-177, 10.1016/j.advwatres.2013.07.016, 2013.

Lawrence, D. M., Thornton, P. E., Oleson, K. W., and Bonan, G. B.: The partitioning of evapotranspiration into transpiration, soil evaporation, and canopy evaporation in a GCM: Impacts on land-atmosphere interaction, J. Hydrometeorol., 8, 862-880, 2007.

Lawrence, D. M., and Slater, A. G.: Incorporating organic soil into a global climate model, Clim Dynam, 30, 145-160, 10.1007/s00382-007-0278-1, 2008.

McGrath, D., Webb, R., Shean, D., Bonnell, R., Marshall, H. P., Painter, T. H., Molotch, N. P., Elder, K., Hiemstra, C., and Brucker, L.: Spatially Extensive Ground-Penetrating Radar Snow Depth Observations During NASA's 2017 SnowEx Campaign: Comparison With In Situ, Airborne, and Satellite Observations, Water Resources Research, 55, 10026-10036, 10.1029/2019WR024907, 2019.

Mottaghy, D., and Rath, V.: Latent heat effects in subsurface heat transport modelling and their impact on palaeotemperature reconstructions, Geophys J Int, 164, 236-245, 2006.

Natali, S. M., Watts, J. D., Rogers, B. M., Potter, S., Ludwig, S. M., Selbmann, A.-K., Sullivan, P. F., Abbott, B. W., Arndt, K. A., Birch, L., Björkman, M. P., Bloom, A. A., Celis, G., Christensen, T. R., Christiansen, C. T., Commane, R., Cooper, E. J., Crill, P., Czimczik, C., Davydov, S., Du, J., Egan, J. E., Elberling, B., Euskirchen, E. S., Friborg, T., Genet, H., Göckede, M., Goodrich, J. P., Grogan, P., Helbig, M., Jafarov, E. E., Jastrow, J. D., Kalhori, A. A. M., Kim, Y., Kimball, J. S., Kutzbach, L., Lara, M. J., Larsen, K. S., Lee, B.-Y., Liu, Z., Loranty, M. M., Lund, M., Lupascu, M., Madani, N., Malhotra, A., Matamala, R., McFarland, J., McGuire, A. D., Michelsen, A., Minions, C., Oechel, W. C., Olefeldt, D., Parmentier, F.-J. W., Pirk, N., Poulter, B., Quinton, W., Rezanezhad, F., Risk, D., Sachs, T., Schaefer, K., Schmidt, N. M., Schuur, E. A. G., Semenchuk, P. R., Shaver, G., Sonnentag, O., Starr, G., Treat, C. C., Waldrop, M. P., Wang, Y., Welker, J., Wille, C., Xu, X., Zhang, Z., Zhuang, Q., and Zona, D.: Large loss of CO2 in winter observed across the northern permafrost region, Nat Clim Change, 10.1038/s41558-019-0592-8, 2019.

Nicolsky, D. J., Romanovsky, V. E., Alexeev, V. A., and Lawrence, D. M.: Improved modeling of permafrost dynamics in a GCM land-surface scheme, Geophys Res Lett, 34, 2007.

Niu, G. Y., and Yang, Z. L.: Effects of frozen soil on snowmelt runoff and soil water storage at a continental scale, J. Hydrometeorol., 7, 937-952, 2006.

Oechel, W. C., and Kalhori, A.: ABoVE: CO2 and CH4 Fluxes and Meteorology at Flux Tower Sites, Alaska, 2015-2017, https://doi.org/10.3334/ornldaac/1562, 2018.

Pirazzini, R., Leppanen, L., Picard, G., Lopez-Moreno, J. I., Marty, C., Macelloni, G., Kontu, A., von Lerber, A., Tanis, C. M., Schneebeli, M., de Rosnay, P., and Arslan, A. N.: European In-Situ Snow Measurements: Practices and Purposes, Sensors-Basel, 18, Artn 2016

10.3390/S18072016, 2018.

Rawlins, M. A., Nicolsky, D. J., McDonald, K. C., and Romanovsky, V. E.: Simulating soil freeze/thaw dynamics with an improved pan-Arctic water balance model, J Adv Model Earth Sy, 5, 659-675, 2013.

Schimel, J. P.: Plant-Transport and Methane Production as Controls on Methane Flux from Arctic Wet Meadow Tundra, Biogeochemistry, 28, 183-200, 1995.

Slater, A. G., Lawrence, D. M., and Koven, C. D.: Process-level model evaluation: a snow and heat transfer metric, Cryosphere, 11, 989-996, 2017.

Tang, J. Y., and Riley, W. J.: A Theory of Effective Microbial Substrate Affinity Parameters in Variably Saturated Soils and an Example Application to Aerobic Soil Heterotrophic Respiration, J Geophys Res-Biogeo, 124, 918-940, 2019.

Tao, J., Musselman, K. N., Clark, M., Koster, R., Reichle, R. H., and Forman, B. A.: Towards a General Snow Thermal Conductivity Scheme in Land Models, 99th American Meteorological Society Annual Meeting, 2019,

Tao, J., Zhu, Q., Riley, W. J., and Neumann, R. B.: Snow-to-Rain Shifts Regulate Cold-Season Carbon Emissions From pan-Arctic Permafrost, TBD. (In Preparation), 2021.

Wang, L., Koike, T., Yang, K., Jin, R., and Li, H.: Frozen soil parameterization in a distributed biosphere hydrological model, Hydrol Earth Syst Sc, 14, 557-571, 2010.

Wania, R., Ross, I., and Prentice, I. C.: Implementation and evaluation of a new methane model within a dynamic global vegetation model: LPJ-WHyMe v1.3.1, Geosci Model Dev, 3, 565-584, 2010.

Zhu, Q., Riley, W. J., Tang, J. Y., Collier, N., Hoffman, F. M., Yang, X. J., and Bisht, G.: Representing Nitrogen, Phosphorus, and Carbon Interactions in the E3SM Land Model: Development and Global Benchmarking, J Adv Model Earth Sy, 11, 2238-2258, 2019.

Zona, D., Gioli, B., Commane, R., Lindaas, J., Wofsy, S. C., Miller, C. E., Dinardo, S. J., Dengel, S., Sweeney, C., and Karion, A.: Cold season emissions dominate the Arctic tundra methane budget, Proceedings of the National Academy of Sciences, 113, 40-45, 2016.