We thank the reviewer for the constructive comments and suggestions. We have revised the manuscript further according to the reviewer's suggestions (see point-by-point responses below). Again, throughout this document, the reviewer's comments are reproduced in their entirety in black, and our responses are given directly afterward in blue. Line numbers here are based on the marked-up version of the revised manuscript.

Second review of "Improved ELMv1-ECA Simulations of Zero-Curtain Periods and Cold-season CH4 and CO2 Emissions at Alaskan Arctic Tundra Sites" Tao et al

With thanks to the authors for their detailed response and apologies for being late with my reply. Most of the major issues have been sorted but there are still some unclear parts, please see my comments below. **Note, all line numbers here are based on the marked up version.**

General comments

R1C1: "R1C1: As we pointed out in the manuscript (lines X - Y)," - line numbers missing here...Having dug into it I think that lines 127-128 in the marked up version suggest the carbon is estimated from the soil properties. But then it turns out in Section 3.2 that this is only for one site, so this should be mentioned when this sentence first appears ("In addition, we used ABoVE soil moisture measurements to derive site-scale soil porosity and organic carbon content *at IVO* (see Section 3.2) "). I would also add here, for clarity "which is used to prescribe thermal and hydraulic soil properties. Note that carbon substrate for respiration is simulated dynamically in the model - see Appendix B."

Thank you. As suggested, we have modified the sentence as below:

> "In addition, we used ABoVE observed maximum soil moisture to infer site-scale soil porosity and then organic carbon content at IVO (see Section 3.2), which is used to prescribe thermal and hydraulic soil properties. Note that carbon substrate for respiration is simulated dynamically in the model (see Appendix B)." (lines 123 - 125)

Section 3.2 states that a global soil C dataset is mostly used to derive the soil properties, but for IVO the porosity is used to estimate soil C, and it is also stated that "The derived SOC content is also consistent with the soil survey data reported in Davidson and Zona (2018)", which suggests to me that some soil carbon data is in fact available, at least for this site, contrary to what the authors have said in their response?

The soil survey data in Davidson and Zona (2018) are not quantitative SOC along with soil depth but are organic layer thickness. To clarify, we modified the sentence as below:

> "The derived SOC content is also consistent with the organic layer thickness reported in Davidson and Zona (2018)." (lines 262 - 263)

Lastly I would still like to see an acknowledgement somewhere (Summary/Discussion would be best) that the optimised decomposition functions would be biased if there is a bias in simulated soil carbon / substrate, and therefore should not be taken directly to other models without further analysis.

Thank you for the suggestion. We have added two sentences into the "5. Summary and Discussion" section:

> "Note that the optimized parameterizations would be biased if there is a bias in simulated soil carbon, and therefore should not be taken directly to other models without further analysis. Instead, the optimization procedure described in this study provides a roadmap that can be directly adopted to calibrate other models at different sites." (lines 599 - 602)

R1C2: Thanks for the response, I appreciate that the analysis of snow depth was added to the supplementary. However I still think it needs to be highlighted more carefully in the main text as an important controlling variable, to give a more complete picture for any reader who is not already an expert.

For example, in the introduction, on line 74 (marked up version!) you could add something along the lines of "We note that representation of snow can also play a major role in underestimation of winter soil temperatures [reference], although we do not focus on this process here."

Thank you for the suggestion. We have added the sentence to the revised manuscript:

> "We note that snow representation can also play a major role in correctly simulating winter soil temperatures (Slater et al., 2017; Lawrence and Slater, 2010), although we do not focus on this process here." (lines 70 - 72)

In the discussion you added "Sensitivity analysis demonstrates large impacts of snow depth on simulated winter soil temperature, summer soil moisture, heterotrophic respiration, and CO2 fluxes (Figure S9)." – I would definitely recommend adding something here, like "therefore the simulation of snow should be the subject of future investigations"

Thank you for the suggestion. We have modified the sentence as below:

> "Sensitivity analysis demonstrates large impacts of snow depth on simulated winter soil temperature, summer soil moisture, heterotrophic respiration, and CO_2 fluxes (Figure S9); therefore, the simulation of snow should be the subject of future investigations." (lines 610 - 612)

R1C3: Phase change efficiency
Line 785 (marked up version), start with something like "To improve this scheme, we can incorporate..." so it's clear that you're not still describing the existing model.

Thank you for the suggestion. We have added this point to the revised manuscript:

> "To improve this scheme, we can incorporate soil-water freezing phase change into equation (Eq. A1) and rewrite the heat transfer equation as …" (lines 694 - 695)

I appreciate that some more equations were added. Equations A7 and A8 are totally clear. Then I would expect to see something that looks like a differential of equation A3 appearing in the updated version of T^(n+1) (so, there should be a factor of 1/B somewhere...). I guess maybe you just didn't include the equation for calculating T^(n+1). I think that would be helpful to add.

As Appendix A described, our model solves the heat transfer equation using the Crank-Nicholson method, which combines the explicit and the implicit method, and the numerical solution for T_i^{n+1} is documented in detail in Oleson et al. (2013). The updated solution basically follows that framework, with modifications to the phase change treatment, as described in the Appendix. We trust it should be fine to only include equations that are impacted by our modifications, e.g., the updated T_i^{n+1} (Eq. A11), instead of including many equations that are publicly available in the literature (Oleson et al., 2013).

I have several queries around equation A11. The freezing point depression temperature does not appear anywhere in this equation, it still has T_f, and it has the phase change efficiency which does not relate either to this temperature or to the original equations (A7 and A8) that you are trying to solve. This phase change efficiency slows down the freezing/melting when it takes a smaller value, and for freezing a smaller value corresponds to less liquid water, which makes sense (although done properly, the freezing point depression should demand a large energy to freeze liquid water when there is not much left, so this would somehow be a double factor?). But for melting, a smaller value of phase change efficiency corresponds to a small amount of ice, which suggests that melting will slow down as it approaches small amounts of ice left in the soil, which to me does not make sense. When there are small amounts of ice left in the soil they will be all surrounded by unfrozen water and it will be easier to transfer energy into them. It would make more sense if the phase change efficiency was always proportional to the liquid water, and then it would somehow represent the freezing curve is a curve and takes more energy for freeze/thaw when there is less liquid water. But again, I am still not sure it is necessary if you properly follow the freeze curve.

We thank the reviewer for these thoughts, and would like to make three related points. First, as Eq. A10 indicates, the freezing point depression temperature is expressed as a function of T_f, and therefore Eq. A11 does include T_f.

Second, we would like to clarify the distinction between soil water phase change and the associated latent heat. Since the soil water freezing process releases latent heat instead of demanding energy, it is not a double factor to employ the phase change efficiency.

Third, soil ice thawing requires energy. The phase change efficiency is applied to the initially estimated energy and mass change involved, i.e., H_i and thus H_m. During the soil ice thawing process, the decreasing phase change efficiency as ice fraction decreases means the process demands less and less energy for thawing further, as indicated by the reviewer. As a result, the new phase change scheme leads to better simulated soil temperatures than the baseline scheme during the thawing season, especially at IVO (red vs. blue in Figure 2). To indicate this improvement, we have added the following sentence to the revised manuscript:

> "Simulations with the new phase change scheme also show improved agreements between simulated and observed soil temperatures during the spring thawing season compared to the baseline results (red vs. blue in Figure 2)." (lines 407 - 408)

How do you calculate w_ice^(n+1) in equation A11, is this going to be different from the previous model version because the latent heat was included in the original temperature change equation (A7/A8) ? This is a key thing, right?

To better explain the updated mass of ice and liquid water, we have added the following sentence:

> " Here, $w_{ice,i}^{n+1}$ is calculated by (Eq. A5) as well, but with updated H_m (i.e., $-\varepsilon_i c_i \frac{\Delta z_i}{L_f} (Tv_i^{n+1} - T_i^{n+1})$)." (line 726)

I would request still more clarification of this phase change efficiency to make this paper clear.

Please see our responses above.

R1C4: Thanks for these changes, all looks good!
R1C5 - justification for transport of methane through frozen soil / aerenchyma. In general this is clearer, thanks for the efforts on this. Just a couple more comments:
In the Appendix it describes epsilon_(snowdiff) (line 913), which was added but it does not show the equation to show how this parameter was applied. This would be helpful to show. Also the justification for the parameter choice, since I understand that this parameter was not varied in the sensitivity study.

Thank you for the suggestion. We have added the equation for snow resistance (Eq. C18) and related descriptions to the revised manuscript (lines 808 to 815).

Line 247: "We also conducted sensitive tests on seven CH4 parameterizations, including six parameterizations resulting from fractional three key variables and one parameterization scheme using all the tested values for the three variables "
In this sentence:
sensitive -> sensitivity
"fractional three key variables" does not make sense.
"tested values for the three variables" - I think you mean parameters, not variables? But even then, this part of the sentence is unclear.

Thanks. Sorry for the typo. We had meant "factorial" here. To better describe the sensitivity experiments, we have simplified this sentence in the revised manuscript, and clearly listed the tested parameterizations in Table S3 (see the updated Table S3):

> "We also conducted sensitivity tests on three key parameters related to CH_4 oxidation and transport processes and tested seven parameterizations (Table S3)." (lines 227 - 230)

Line 252: again 'sensitive' -> 'sensitivity'

We have modified the word as suggested. Thanks.

R1C6 - adding IVO CO2 to analysis: thanks for doing this.
However in Section 4.2, the discussions of CO2 emissions between sites are mostly unchanged and do not include IVO, despite its being added to the plots, please check these and modify as necessary.
(For example, line 544-545 in marked up version: "Thus, the improved NSEs for CO2 and CH4 emissions at BES/CMDL and BEO were larger than those at ATQ" - and what about IVO?)

Thank you. We have added some discussion about CO_2 results at IVO:

> "At IVO, although generally showing low NSEs for CH_4, some new simulations have improved NSE_{CO2} that are larger than 0.5 (Figure 5), compared with -0.3 for baseline. Indeed, the best result at IVO (with a NSE_{CO2}=0.78) significantly improved the simulation of summer CO_2 sink compared to baseline result (Figure 6)." (lines 484 - 486)

Line 650-656. IVO is still missing from this part also.
(New) Figure 5, the IVO plot is covered over by the legend. We should be able to see some values for CO2, at least? Looking at Figure 6 it looks like CO2 is significantly improved at IVO, so this should be apparent in the NSE for CO2?

Thanks. We have moved the legend outside the plot for IVO to show the figure better. At IVO, the CO_2 results are significantly improved compared to baseline results, with some NSEs for CO_2 larger than 0.5. But the NSE for CH_4 is still low at this site. See the updated Figure 5.

R1C7 "we had checked the emissions vs. temperature and moisture (included in the authors' comments uploaded earlier)" Would this not be worth including in the manuscript / supplementary material?

Thanks. We now have included the figure in the supplementary as Figure S10.

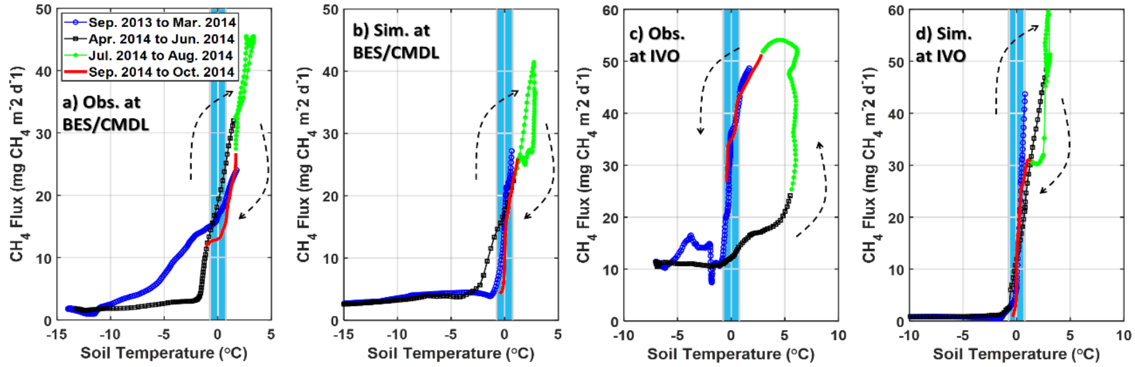

Figure S1 - (A similar figure as Fig.3 in Zona et al. (2016)). Daily CH_4 emissions vs. soil temperatures at 12 cm at two sites. Similarly, as in Zona et al. (2016), we applied a 30-day averaging window to smooth the daily data to produce clear seasonal progressions. Shaded blue areas indicate zero-curtain 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.

"In the future, we will apply a Macromolecular Rate Theory (MMRT)-based temperature sensitivity approach, which uses a quadratic relationship to approximate the CH4 - temperature dependencies and thus can address the CH4 hysteresis effect (Chang et al. 2020, 2021) "
This implies simply changing the temperature function to a quadratic? Chang et al 2020 shows that the microbial dynamics are important for the seasonal hysteresis effect. Chadburn et al 2020 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GB006678) also showed that the hysteresis effect can be captured by modelling methanogen seasonal dynamics, without MMRT. Therefore I am not sure that this is the key, but rather the fact that methanogens are slow-growing and slow responding organisms so they introduce a lag time on methane emissions. Thus, future work should consider simulating microbial population/activity levels.

Thanks. We now have modified the sentence as below:

> "In the future, we will incorporate a representation of methanogen seasonal dynamics and simulate microbial population and activity levels to address the hysteresis of CH_4 emissions with temperature." (Lines 437 - 439)

Line 488-489 "this mechanism and wetland inundation dynamics together would cause hysteretic effects on CH4 emission response to soil temperatures". The use of 'this mechanism' implies that advective heat transport is the cause of hysteresis. If anything advective heat transport would cause thaw to happen more quickly in the early season. In fact the hysteresis is likely more related to the microbial activity level, or potentially the substrate distribution in the soil. Please clarify this.

Thanks. We have modified the sentences as below:

"Also, methanogen seasonal dynamics would cause hysteretic effects on CH_4 emission response to soil temperatures (Chang et al., 2020; 2021; Chadburn et al., 2020)." (lines 435 - 437)

Specific Comments

Introduction

Line 73 of marked up version: "CO_2 emissions" -> "emissions of CO_2" to link up with the "and CH_4" that follows.

We have modified the sentence (Line 70) as suggested. Thanks.

Data

"The CARVE CO_2 measurements were not available at the data archive we used here"
Does this mean CARVE was only used for CH_4? Then you should say "and >CH_4 from< Carbon in Arctic Reservoirs Vulnerability Experiment (CARVE) flight campaign " in the previous sentence, that would make it a lot clearer.
"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"
This still does not make sense, if you already have CO_2 observations from 2013 to 2014 which the Natali et al observation are complementing... where are they from? Do you mean "to *complete* the CO_2 observations" ? Or "to complement CO_2 observations from 2015 to 2017" ?

Thanks. We have modified the sentences as below to clarify the data availability better.

"We assembled daily observations of CO_2 and CH_4 fluxes from 2013 to 2017 at five eddy-covariance flux tower sites in Alaska's North Slope tundra (Figure 1) from the Arctic-Boreal Vulnerability Experiment (ABoVE) project (2015 - 2017) (Oechel and Kalhori, 2018) and CH_4 fluxes from the Carbon in Arctic Reservoirs Vulnerability Experiment (CARVE) flight campaign (2013 - 2014) (Zona et al., 2016). The CARVE CO_2 measurements were not available at the data archive we used here; therefore, monthly winter-time CO_2 flux data from 2013 to 2014 at the same towers assembled by Natali et al. (2019) are included to complete CO_2 observations." (Lines 92 - 97)

Line 123 "evaluating" -> "evaluation of"

We have modified this (line 120) as suggested. Thanks.

Methods

Line 321 in marked up version: "Results vary with soil depths", does this mean "Results for ZCP duration vary with soil depth at which the ZCP is taken" ? Please replace if so, or clarify if not.

Thanks. We have modified the sentence (line 295) as suggested.

Results

R1C21 Response: "The pattern (i.e., lower soil moisture in shallow soil layers and higher soil water in deep layers) is shown in Figure 3 (Figure 2 in the revised manuscript) by the magenta vs. green lines during summertime when the active layers reach the deepest thaw depths." If you look at Figure 2a (BES/CMDL), when the active layer is deepest the water is lower in every layer except the bottom one, but this bottom one I believe is partially frozen so it's not possible to tell how much water is actually in there? I guess it's just not totally clear without showing the unfrozen water as well, could you add the line for 'total water contents' as well as unfrozen, maybe in same colours but a different line style?

Indeed, the moisture saturation Sf ($\theta_{liq}/\theta_{sat}$) shown in Figure 2 means unfrozen (liquid) water content. We have stated this in the revised manuscript, and now we also clarified this in the figure caption by adding, "Here, the moisture saturation means soil unfrozen (liquid) water content." (Line 1134)

Line 519. "Figure 6 illustrates the uncertainty associated with the model representations of environmental influences on heterotrophic respiration and methane parameters"
Are you sure it's Figure 6? I think you might mean Figure S8, based on the discussion that follows.

Thanks. We have changed Figure 6 to Figure S8.

Line 530 R1C25 "reasonably explained the varying influence along with the vertical soil profile (Niu and Yang, 2006)" This wording isn't clear, the varying influence of what on what? It would be great if you can rephrase this part.
Could you also say "(Niu and Yang 2006, Figure 1)" just to make that part clear, as you mentioned in the author response? Thanks! I am also struggling to see where there is a vertical soil profile in Niu and Yang Fig 1.

Thanks. Fig. 1 in Niu and Yang (2006) shows that the relationship between unfrozen soil moisture and soil temperature varies with clay fraction in soils, which reflects the vertical distribution of soil properties along with soil depth. We have modified this sentence as below:

> "…, reasonably explained the varying influence along with the vertical soil profile (i.e., relationships between soil liquid water content and soil temperature varies with soil clay fraction as demonstrated by Fig.1 in Niu and Yang, 2006)." (Lines 468 to 469)

Summary

Line 704-705 "The underestimated emissions during post-ZCP months (Oct. to Nov.) are mainly caused by the lack of sudden bursts of CO_2 and CH_4 during the freeze-up period" I don't think there was anything in the paper that showed this definitively (please correct me if I'm wrong). I suggest you tone this down to "may be caused by" instead of "are mainly caused by".

Thanks. We have changed "are mainly caused by" to "may be caused by" (Line 614).

Appendix

Line 869 and 895: "on default" -> "by default"

Modified as suggested. Thanks.

R1C39 - thanks. I think there might still be an extra * in equation A10 (new version).

Removed the extra *. Thanks.

R1C42: Thanks for adding the reference. I have looked up "tiller" in Wania et al (2010) where they provide a footnote as to what it is, which indicates to me that perhaps it is not widely known. It might be helpful to include something similar to their footnote which I have copied here for convenience:
"Tillers are segmented stems produced at the base of many plants in the family Poaceae, with each stem possessing its own two-part leaf. The usage of the word “tiller” has been expanded to the order of Poales, which includes both groups, grasses (Poaceae) and sedges (Cyperaceae), and is here used in its wider meaning."

Thank you very much. We now have included a sentence here to better explain “tiller”:

> “Here, tillers mean segmented stems of plants in the Order of Poales, including grasses (Poaceae) and sedges (Cyperaceae) (Wania et al., 2010).” (Lines 790 to 791)

Figures
Thanks for the improvements to the Figures, they are definitely easier to interpret.

Thanks.

Reference

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