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."

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?

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.

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."

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"

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.

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.

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.

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?

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

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.

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

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?)

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?

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?

"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.

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.

Specific Comments

Introduction

Line 73 of marked up version: "CO2 emissions" -> "emissions of CO2" to link up with the "and CH4" that follows.

Data

"The CARVE CO2 measurements were not available at the data archive we used here"
Does this mean CARVE was only used for CH4? Then you should say "and >CH4 from< Carbon in Arctic Reservoirs Vulnerability Experiment (CARVE) flight campaign " in the previous sentence, that would make it a lot clearer.

"monthly winter-time CO2 flux data at the same towers assembled by Natali et al. (2019) are included to complement CO2 observations from 2013 to 2014"
This still does not make sense, if you already have CO2 observations from 2013 to 2014 which the Natali et al observation are complementing... where are they from? Do you mean "to *complete* the CO2 observations" ? Or "to complement CO2 observations from 2015 to 2017" ?

Line 123 "evaluating" -> "evaluation of"

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.

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?

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.

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.

Summary

Line 704-705 "The underestimated emissions during post-ZCP months (Oct. to Nov.) are mainly caused by the lack of sudden bursts of CO2 and CH4 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".

Appendix

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

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

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."

Figures

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