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The Cryosphere Discussions

Interactive comment on "Improved ELMv1-ECA Simulations of Zero-Curtain Periods and Cold-season CH_4 and CO_2 Emissions at Alaskan Arctic Tundra Sites" *by* Jing Tao et al.

Anonymous Referee #1

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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.

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).

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.

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).

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.

5) CH_4 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.

6) Missing out of IVO. It is shown that IVO cannot simulate reasonable methane emissions even with the correct temperature and moisture, however CO_2 emissions did not suffer from this problem. Therefore I don't fully understand why CO_2 from IVO cannot be included in the analysis. The optimisation would have to be done only on CO_2 but it could still be optimised, is that right?

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 obser-

vations which would make it difficult for any single function to capture the dynamics, and would be worthwhile to know.)

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.

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"

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

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many land models account for latent heat released during soil water freezing, they do not treat and distribute this heat appropriately"

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.

Study sites and data

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

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.

Line 153-154 "We applied the phase change efficiency to the initially estimated energy and mass change involved, i.e., ðİŘżàŕIJ and ðİŘżàŕă (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).

Table S2: I am missing where the moisture functions ModifiedELMâĂŘS1 ModifiedELMâĂŘS2, etc are documented?

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.

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 *non-inundated 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.

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.

Line 346 Reference to Figure 3 should be Figure 4.

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 freeze-up 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!

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)".

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.

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.

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".

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

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.

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.

Line 471. vary -> varies

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.

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).

Line 496 slightly -> slight

Line 511-512 "We find that the simulated cold-season CO_2 emissions were larger than the warm-season CO_2 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)

Summary

I suggest that you additionally mention the potential issues of using the heterotrophic respiration to estimate CH_4 production. For example, this means that CH_4 emissions may drop as the soil becomes more saturated (once soil moisture passes the optimum), whereas in fact the highest CH_4 emissions should be in saturated conditions.

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.

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

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

Line 571-573 "The increasing rate of cold-season heterotrophic respiration (releasing CO_2) may become larger than the trend of warm-season vegetation CO_2 uptake under future climate" In fact in your simulations, the cold season respiration already became larger than the warm season CO_2 uptake by 2017, is that right? This point could be made stronger with that information.

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. 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? 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? Line 700: Apologies if this is common knowledge but I don't know what "amount of carbon per tiller" means. Is this correct? Line 700-702: This needs more explanation/equations, see comment (5) in general comments, above. Line 710: "Table 2" should be Table 1, I think 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) Line 711: presents -> is present.

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?

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.

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.

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

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