

Interactive comment on “Year-round simulated methane emissions from a permafrost ecosystem in Northeast Siberia” by Karel Castro-Morales et al.

Anonymous Referee #2

Received and published: 20 November 2017

Overall:

The ms has its focus on regional scale methane dynamic and the modelling of year round dynamics, which is certainly relevant and highly needed. I general there are quite few year round measurements of methane dynamics in the arctic region, which also explains why the modelling studies are even fewer and regional budgets are poorly constrained. Further, the understanding of drivers and exact transport mechanisms in the top soil and soil – snow- atmosphere still in most (not all) cases relies on an interpretation of a net emission, rather that independent quantification of the individual components adding up the net CH₄ emission. For that reason the focus of the current

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ms is important and timely. Despite that the ms is well written and in general well references, I'm a bit reluctant about the qualities of the ms, because I basically find that it tries to accomplish too much and not in a fully convincing way. As pointed out by reviewer 1, also I have a serious problem with the differences in scaling which are used in the different components of the study. In my perspective, the very coarse spatial scale of the model does not compare well with the highly advanced model approach of partitioning the production and transport of CH₄ in the soil and snow. The ms simultaneously tries to solve the issues of the spatial /temporal methane dynamics of the large Siberian wetlands, the process pathways and comparing all the modelling output to relatively few and very local measurements near Chersky. I basically don't think that the available measurements are well suited to verify the model output of the processes leading to the net CH₄ emission at the surface, and the differentiation of pathways of CH₄ during different periods of the year. In my opinion the ms could benefit from being divided into two; one with focus on the annual budget for Siberian and one focused on the process modelling of the different pathways for methane through the soil/snow pack. The later one could benefit from some kind of lab or micro cosmos comparison, where processes could be studied more precisely than what is mostly the case in the field. Regardless of the approach, the issues of differences in scales should be discussed much more detailed and qualified than it is done in the present version of the ms. From my perspective the output of the model and the assessment of the advances in the new "improved" version is not credible as it appears now, despite that the output is in the same ballpark as the measured data, and a number of other studies.

Specific:

L48 -> 66: Maybe a matter of taste, but I'm in general against using these "horror scenarios" which draw lines between the carbon pool of the Arctic soils and potential increase of GHGs. I think we now know that no indications are found that something very dramatic is happening in foreseeable future, and it doesn't add to the understanding of the ms. Consider rephrasing.

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L187: Despite that you are obviously aware of the complications of the comparison between scale I'll encourage you to address specifically how the scaling issue between 0,5° modelling grid and EC footprint or chambers is dealt with.

L218: Again please justify, why 11 soil layers are needed, when the horizontal scale is this coarse.

L323: Spun up for 10.000 years? Please justify further, climate (or C – pools) can not be assumed to have remained constant for this period of time.

L403: I understand that the numbers can be compared, but please argue why the field site measurements can be assumed to be averaging the full 0,5x0,5° modelling pixel.

L445: Differences seems to be substantial please comment.

L470: I basically don't understand how a threshold can be set for proportion of flooded area in a pixel – what is the rational ? Theoretically the whole pixel could be inundated –I assume?

L532: What effect of the snow would you have expected in this context?

L630: there seems to be significant difference in measured and modelled soil temperatures, please comment.

L665: probably why also both absolute values and seasonal pattern seems distinctly different

L710 -723: that differentiation between ebullition and diffusion seems unfounded, and it is hard to see how you verify the different pathways, please elaborate.

Para 3.4.3: could this be merged with the sensitivity study in 3.2? seems to be fundamentally alike.

Fig. S5b: legend does not seem to match.

L916-920: the conclusions here seem somewhat unfounded due to the previously men-

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tioned scaling issues.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-310>, 2017.

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