Note: in the following document, the original comments made by the reviewer are copied in black, while the authors' responses to these comments follow in blue.

Authors' response to comments submitted by Reviewer #2

General comments

The manuscript extensively documents a new process-based methane module for use with a global land surface scheme. The processes methane production, methane oxidation, methane transport from the soil to the air by ebullition, diffusion and plants are all included in the module. The model has been tested for the Samoylov research site in Siberia, which harbours polygonal tundra consisting of wet centers enclosed by relatively dry rims. Of course the model simulates higher methane emission in the wet centers than in the rims, because of the differences in soil moisture. Methane transport by plants seems to be underestimated, which might be related to uncertainty regarding the parameter values, e.g. root surface properties. Plants may also influence methane emission by root exudates; a source of labile organic carbon brought into the water-saturated layer by sedge roots, from which methane can be produced. I do not know how important this mechanism can be, but I would like to see some discussion about this role of plants.

The reviewer certainly raises a very important point regarding the influence of vegetation on the methane emissions. The reviewer is right that the CH_4 transported by plants is limited by the parameter values related to the plant characteristics. As requested by reviewer #1, in the revised paper we are incorporating results of a suite of sensitivity experiments, which also include the variation of the density of vascular plants. The effect of the range of parameter values is evaluated and discussed in the context of the total methane emissions.

With regard to the methane production and emission due to root exudates, we thank the reviewer for commenting on this process. The contribution of root exudates to methane production and emission has been highly neglected in models, and is also not considered in our model. This process has been also understudied in field experiments because it is a process that cannot be measured directly.

The indirect contribution to methane production from root exudates has been mainly analyzed in rice fields (Neue et al., 1996) and to less extent in peat soils (Koelbener et al., 2010). The later study suggests that the rate of root exudate is linked to the nutrient availability in soils, in a way that more root exudates are present in plants located in nutrient poor wetland soils. The wetland soils in Arctic tundra are known to be nitrogen limited (Melle et al., 2015; Gurevitch et al., 2002). In a recent work by Beermann et al. (2015), the authors evaluate the nitrogen content in different compartments of soil and vegetation in a polygonal lowland tundra located in Indigirka, Russia; the authors findings revealed that plant growth in these environments are co-limited by nitrogen and phosphorous with only about 5 % of the total nitrogen soil content to be an active part of the biological fraction. Knoblauch et al. (2015) investigated the indirect role of root exudates for methane production in polygonal ponds and water-saturated soils in Samoylov Island. The authors found almost 4 fold higher potential methane production rates in vegetated sites than in non-vegetated sites, both with same C and N concentrations in the soil, indicating the potential important contribution of root exudates to methane soil production. Perhaps the most direct attempt to quantify this process has been made by Ström et al. (2012), who studied the impact of plant exudates on methane emissions in an Arctic wetland in Greenland, and related them to different vascular plants species. They concluded that the presence of vascular plants in Arctic wetlands support the production of low molecular weight carbon compounds that are highly labile, which can promote methane emissions through their methanogenic decomposition.

Thus, with no doubt the contribution of root exudates to methane emissions can play an important role in Lena Delta, and needs to be taken into account in models. Of particular importance is to consider potential nutrient mobilization in Arctic soils related to permafrost

degradation under climate change (Kuhry et al., 2010). This effect may enrich wetland nutrient contents from mesotrophic to eutrophic state, implying a minor contribution to methane production due to root exudates.
Unfortunately, in the current configuration of the JSBACH model, the concentration of nutrients in the soil is not taken into account, thus the nutrients assimilation by plants roots is also not considered. These processes need to be parameterized in the model before the root exudate contribution to the total methane emissions can be accounted. We expect that this process is included in the future model iterations, and in that way the methane module can be completed with the inclusion of root exudates. We will discuss this point in the same way as above in the revised version of this manuscript.

An interesting finding is the simulated spring burst of methane that has accumulated over winter, although it is not clear if this happens in reality.
The authors agree with the reviewer that more tests on this effect need to be performed to properly evaluate how realistic are the spring burst as seen in the model output. Several published studies have reported the presence of methane spring thaw burst in wetlands and lakes at high latitudes using different measurement techniques (e.g. eddy covariance, chambers). These events are sporadic, and few studies have been exclusively dedicated to investigate spring methane emissions in the transition from winter to the growing season (e.g. eddy covariance technique in a lake and wetland area in north Sweden, Jammet et al., 2015; using chamber measurements and eddy techniques in north Sweden, Friborg et al., 1997; or in Finland, Hargreaves et al., 2001; chamber measurements in a peatland in northern Japan, Tokida et al., 2007; chamber measurements in a wetland area in Northeast China, Song et al., 2012). These few studies suggest that the methane spring thaw emissions occur over short periods, and the magnitude of the short-term emissions can exceed the mean summer fluxes by a factor of 2 to 3. These emission events may therefore account for a large share of the total annual emissions. However, both the duration and magnitude of the spring thaw events are to date highly uncertain. Not only a further evaluation in the model is needed to evaluate the representativeness of these events in the model output, but also more observational data measured during the spring thaw season are needed to adequately characterize these events. For the model side, these steps will be considered in a follow-up manuscript in which we will also apply the methane module to a larger spatial scale. We will include this discussion in the revised version of the manuscript.

Overall, a promising methane module, but improvements regarding the hydrological scheme, which currently does not allow for standing water, and fine-tuning of parameter values are required in follow-up studies, before it can be applied on a global scale.
The authors appreciate the support of the reviewer to our manuscript. As is correctly pointed out, the status of the configuration presented in the current manuscript still requires improvements toward the hydrological scheme. This will be particularly important for any large-scale application in future. Practically, we see a possibility to apply a TOPMODEL approach for estimating the inundated fraction of the grid cell. These steps are currently been taken into account and will be part of a follow up study. Please see also our discussion with reviewer #1 in this respect. In addition, before the methane module can be applied on a global scale, current tests at a regional scale will be implemented. However, we would like to stress that the intention of the current manuscript is the presentation and description of the newly developed process-based methane module, and its application at a site level scale. This should be always the first step before going global. Therefore including regional applications in this work goes beyond the scope of this manuscript.

Specific comments

L.1 Remove the word consistent; not clear what is meant and it implies that other methane modules were not consistent.

In this context, what we wanted to say was that the new model is even more process-based than previously existing model implementations, e.g. regarding the use of physical state variables (water or ice content, free pore space) in modules such as the computation of thermal conduction, or gas transport. We will avoid a misleading use of the word 'consistent', as suggested by the reviewer. The manuscript text will be revised accordingly.

L.4-6 Is the simulation of oxygen processes new compared to other methane models? If yes, please let the reader know, for example: In this model, oxygen has been explicitly

Yes, one of the novel developments of the methane model presented in this manuscript is the explicit addition of oxygen content in the soil as a state variable, and of two methane oxidation processes. As discussed in the manuscript, the two oxidation processes are:

1) bulk soil oxidation of methane: here, above the water table at most 40 % of the available oxygen is used for heterotrophic respiration, and 10 % is reduced by other physical processes such as N or P oxidation. Below the water table, 50 % of the oxygen content is available for methane oxidation. Via Michaelis-Menten kinetics for methane and oxygen as well as methane and oxygen availability, the model then calculated how much methane is oxidized;
2) oxidation of the methane that is available to be transported by roots (plant oxidation): the model allows oxygen also to enter through the aerenchyma of the plants, and this oxygen is then allowed to oxidize the available methane to be transported by roots. Again, the same procedure like for bulk soil oxidation calculates how much methane is actually oxidized.

In the revised manuscript, we will add explicit sentences on the addition of the oxygen processes to emphasize the novelty of this configuration. This explicit consideration of oxygen is an important point for making the model more process-based where previously these processes have been implicitly taken into account by effective parameters.

L.14 Please add a concluding sentence about potential applications of the model.

As suggested by the reviewer, we will add in the revised manuscript a concluding sentence about the potential applications for the methane model. To mention some of them: application in studies at regional to global scale with the main focus of evaluating the contribution of methane emissions to the global carbon budget due to permafrost thaw in high latitudes, the projection of future methane emissions (total and from the different emission processes) at different spatial scales (from regional to global), and ultimately a quantification of future methane emission contributions on future climate and corresponding feedback mechanisms.

L.135 It is not clear to me why water content at field capacity is used and not water content at saturation. What soil type is used in these simulations, peat or mineral soil? At least in peaty soils there can be quite some difference between water content at field capacity and saturation. What value has been used for field capacity?

This is a good point and from a site-level perspective this point is also very relevant. Looking at methane emissions from polygon rim or centre, pedon-scale hydrology plays an important role, and in particular lateral water fluxes. As the ultimate goal is to arrive at a methane module embedded into a global land surface scheme, from the beginning this methane module has been incorporated into the existing land surface model, which uses a simplified hydrological scheme, e.g. assuming lateral flows can be neglected at 0.5 degree grid cell size. Still, we believe that running the model using site-level climate data and soil information is very useful for a first-order guess about its reliability. Therefore, we mimic as much as possible the different hydrological regimes at rim and centre with a different simulation protocol, and also we apply a semi-empirical relationship of relative soil moisture to water table depth. The reason is that the hydrology scheme of the land-surface model does not work with saturation of soils but water above field capacity is basically removed by runoff. This representation of

water table height will be obsolete in future when coupled to hydrology schemes such as e.g. TOPMODEL, which will estimate the inundated fraction of each grid cell. We will revise the manuscript in order to clarify this point.
Regarding the question on soil type, the current model implementation just considers mineral soil, no peat layers in this version. Field capacity was set to 0.435, porosity to 0.448.

L.398-402 Here I need some more information about soil carbon decomposition and particularly about the depth distribution. Do soil temperature and soil moisture influence the decomposition rate (which would lower the decomposition at depth)? Is there one soil organic carbon pool, or is it divided into labile and more stable carbon pools?
The processes the reviewer refers to are explicitly resolved along the vertical soil profile, with detailed information on the implementation provided in the appendices of the submitted manuscript. The carbon decomposition is temperature dependent, but moisture dependency has been implemented only indirectly: The soil profile is divided into three sections, and each of them has its own temperature used for the decomposition. The vertical extent of each section depends on the positions of actual and the minimal water table, respectively, which is where the moisture influence comes in.

Details on the depth distribution of the existing soil carbon are provided in Appendix 3 of the manuscript. Because for the carbon decomposition the soil profile is only split up into three sections, the decomposition module itself does not use this information at the original, high resolution. But the calculated decomposed carbon amounts of the three pools are then distributed by the methane module over the layers according to the distribution of the soil carbon as described in Appendix 3. Further details on these processes are also shown in Appendix 2.

L.393-394 This is confusing: OK, diffusion through the root exodermis is very slow, but this layer is very thin compared to the water layer. Is there still a lower rate of transport through plants than through water?
In the methane module, the diffusion of gas through root exodermis is represented as only 80 % of the diffusion of gas in water. Although the exodermis is a very thin layer, it is an efficient barrier against gas exchange that allows the gases to remain within the plant (e.g. in case of oxygen) to use it for metabolic processes. Thus, the diffusion of methane from the soil pores through this very thin layer cannot be as fast as in water. This is explained in section 2.2.7, L.311-330 of the discussion manuscript.

L.523-524 This was also the conclusion of the previous section. Please combine sections 3.3. and 3.4 as there is quite some overlap.
These sections will be combined in the revised manuscript as suggested by the reviewer.

L.572-574 Isn't this already taken into account by defining saturation as … % of field capacity?
The 90 % field capacity in the soil corresponds only to the soil layer, and in this current model configuration no standing water above the soil surface is taken into account. Despite the soil remaining moist, there will be no methane emissions to the atmosphere. By considering this in the context of comparing chamber measurements to model results, we wrote the statement that the model might have "less moist hydrological conditions" than the field sites where the chamber measurements were performed. In the chamber measurements sites, potential accumulation of water above the soil can lead to higher methane emissions, thus contributing to a larger discrepancy when comparing field measurements to model results. We will clarify this sentence in the revised version of the manuscript.

Although I am not a native English speaker, the manuscript needs some editing with regard to the English language.
As suggested also to reviewer #1, we plan to have the revised version of the manuscript reviewed by a native speaker before resubmission.

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