Dear Referee,

Thank you for the good and useful comments regarding the manuscript. They helped us to see shortcomings in the work and taking your comments into account would improve the paper. Especially the aim of the model and this work in general should indeed be better described. Below we reply point-by-point to your comments. We hope you find our responses satisfactory and we can prepare and submit a revised version of this manuscript along the lines we suggest below.

A change that we suggest, not directly as a response to any Referee comment, is related to the model parameters. A set of parameter values that we use in the manuscript was taken from an optimization done using Markov chain Monte Carlo methods with observational CH_4 flux data from the Siikaneva peatland site, and we refer to Susiluoto et al. (2017, in prep.). The final results and a more exact description of the calibration work are now reported in Susiluoto et al. (2017, GMDD-2017-66). However, there were some major differences between the approaches used here and in the final version of Susiluoto et al., which led to some difference in the values. As the parameter values are not the main point of the present work and as they produce a good fit with observations, we suggest that we keep the current values for the revised manuscript. However, in the revised version we will not anymore refer to Susiluoto et al. but add in the Materials and Methods section a description of the optimization.

Referee comments are typed in italics. They are followed by our responses and suggestions of how we would revise the manuscript, as plain text and numbered referring to the comment number.

(1) This manuscript presents a sensitivity analysis of a methane module that could be included in peatland models. The authors argue that the novelty of this study is that the model has been developed independent of a full peatland carbon model and can then be tested for sensitivity allowing for dependencies within the methane models itself to be assessed separately from the entire C model. The fact that it is a module without the complete C cycling that feeds input to the methane module, makes it difficult to assess the ability of the model to estimate fluxes as the test that compares it to field-measured fluxes did not optimize the anoxic respiration input and this would actually be generated from the entire peatland C model. Also, sensitivities are difficult to assess this way as important drivers (e.g., temperature driving CH4 production) are not included as this would happen in the other part of the peatland C model that would drive anoxic respiration rates.

(1) Response:

We agree with the Referee that it is difficult to evaluate the model's ability to predict CH_4 fluxes and sensitivities to input when it does not include the whole carbon cycle. The reason for developing this kind of methane model was to produce a module that can be used in different purposes, as a platform for specific studies on methane processes, but principally as a component of large-scale biosphere models that provide the anoxic respiration input. We believe it can be useful for the community. For instance, the CH_4 model of Walter & Heimann (1996) has been utilized in several peatland ecosystem modelling frameworks to simulate methane (e.g. in Ringeval et al. 2011, van Huissteden et al. 2009).

We considered the test with Siikaneva data (Sections 3.2.3, 4.2 and 4.3) as a test of whether HIMMELI produces realistic output, as we aimed at using as realistic input respiration as possible in this test. On the other hand, as written in the manuscript, we especially think that as the anoxic respiration rate largely governs the CH_4 emissions, it is important to standardize it and find out what kind of dependencies there are inside the CH_4 model alone, given that it usually takes a relatively large portion of a complete peatland carbon model.

(1) Suggested changes to the manuscript:

In the Introduction, we will clarify the aim of HIMMELI and explain more clearly why simulation of anoxic respiration is not included in the model.

We will clarify the role of the Siikaneva test in the paper and, as suggested by another Referee, we will add a comparison of the model with data from another peatland site. This will be a test of how well the current parameterisation fits to other peatland sites.

(2) Aside from testing the sensitivity of a methane model outside of a full C model, the novelty of the model itself is not clear. The way in which methane production, oxidation and transport is considered in the model appears to be largely developed according to methods used in previous models and therefore it is not clear what improvement is expected here. The way in which ebullition is handled, for example, is quite simplistic and not consistent with literature that clearly illustrates trapping of free-phase gas over time as opposed to release as soon as a bubble is formed (e.g., Comas et al., 2014; Ramirez et al., 2015). I think a clear justification of why another peatland CH4 model is needed must be included to illustrate the utility of this model.

(2) Response:

About ebullition: please see our responses to your comments number (11) and (14).

We acknowledge the fact that HIMMELI does not bring any new processes into CH_4 modelling and the process descriptions are based on earlier models. This is mentioned on P3, lines 11-12. We wanted to produce a model that simulates the transport of all CH_4, O_2 and CO_2 that is not a common feature among CH_4 models. We decided that rather than taking directly one of the existing model codes that are developed with and thus closely connected to some biosphere model, we would systematically start from fundamental elements and combine the process descriptions in a format that can be flexibly applied for different uses as, for instance, the peat column structure is not fixed.

Although HIMMELI does not include all processes that already exist in some models (e.g. alternative e^- acceptors, anaerobic CH4 oxidation), it is among the most complete models considering the transport of compounds. According to Xu et al. (2016), who reviewed 40 existing terrestrial ecosystem models for CH_4 cycling, there are only 5 models that simulate all these: vertically resolved biogeochemistry, O_2 availability to CH_4 oxidation, and three pathways of CH_4 transport. Of these 5, the Xu model (Xu et al. 2007), CLM-Microbe (Xu et al. 2014) and VISIT (Ito & Inatomi, 2012) do not explicitely simulate O_2 transport between the atmosphere and peat. On the other hand, LPJ-WhyMe (Wania et al. 2010), a revised multi-substance version of TEM (Tang et al. 2010) and a recent model by Kaiser et al. (2017) - that were not included in the list by Xu et al. -- do simulate all these. HIMMELI also simulates CO_2 transport via all three transport pathways. To our knowledge, only the multi-substance version of TEM (Tang et al. 2010) and the Segers model (Segers and Leffelaar, 2001) included it.

We think that as the anoxic respiration rate largely governs the CH_4 emissions, it is important to standardize it and find out what kind of dependencies there are inside the CH_4 model alone, given that it takes a relatively large portion of a complete peatland carbon model. Here the fact that HIMMELI contains similar components as other methane transport models means that the results reveal and clarify inherent assumptions and process dynamics in the other models.

(2) Suggested changes to the manuscript:

We will justify the necessity of the new model by adding the contents of the above text (reference to Xu et al. 2016) in the Introduction.

(3) Page 2, Line 5: Maybe the 2nd largest anthropogenic radiative forcing after CO2? Water vapour causes the greatest radiative forcing in the atmosphere, followed by CO2 and then CH4

(3) Response:
Agreed.

(3) Suggested changes to the manuscript:
We will correct this sentence: "…inducing the second largest radiative forcing among well-mixed greenhouse gases."

(4) Page 2, Line 13: Saying no other alternative electron acceptors exist is a bit extreme. Many freshwater wetlands will have cycling of NO3, Fe, SO4, etc., in addition to CH4 production. I suggest rewording this sentence.

(4) Response:
Yes, we agree.

(4) Suggested changes to the manuscript:
We will remove this sentence.

(5) Page 4, Line 28: I guess 45-60% is meant (as opposed to : : :). This happens throughout the manuscript in my version.

(5) Response:
The manuscript preparation guidelines of this journal say: "A range of numbers should be specified as "a to b" or "a…b". The expression "a-b" is only acceptable in cases when no confusion with "a minus b" is possible." We thought it would be clearest to use the same convention consistently throughout the manuscript and therefore the expression "a…b" everywhere but we can change this.

(5) Suggested changes to the manuscript:
We leave the "a…b" expression only in tables but within the text change it to "a to b".

(6) Page 5, Lines 1-3: And also, peat properties and pore sizes are likely to vary within and between peatlands based on composition of the peat (i.e., sedge vs. wood vs. moss) as well as decomposition status.

(6) Response:
Yes, true, that is relevant information here. Thank you for pointing out these.

(6) Suggested changes to the manuscript:
We will add this information on Page 5.

(7) Page 5, line 7: the effect of tortuosity on the diffusion coefficient indicates that it is not only the porosity that is important, but the interconnectivity and shape of that porosity and probably the pore size distribution

(7) Response:
Yes, this is a good point. We are sorry for the inadequate piece of text.

(7) Suggested changes to the manuscript:
We will modify this text so that it also describes the significance of tortuosity.

(8) Page 6, Line 7: In reality WT is the not the divide between water-filled and partially water-filled pore space. Above the WT there is always some fully saturated layer as the capillary fringe. In practice in the model it doesn't make a difference as the boundary would instead be the capillary fringe, but the way it is written here is technically incorrect.

(8) Response:
We agree, this is an incorrect statement as it is, this should refer only to the model.

(8) Suggested changes to the manuscript:
We will correct this sentence to: "In the model, WTD is taken as a strict divider of the peat into water-filled and air-filled parts."

(9) Page 6, Line 11: When WT is above the surface it can become oxygenated by windmixing. Is this considered?

(9) Response:

Yes, windmixing can affect the O_2 concentrations but this is not considered in the model yet. The model naturally is a rough simplification of reality: so far it assumes a pure water layer on top of the peat surface, although there often is vegetation growing in the peat. Vegetation would hinder the windmixing via affecting wind speed and generally modifying the physical conditions affecting thin boundary layers regulating gas transfer across the water-air interface. These processes are not fully understood even for open water surfaces of inland water bodies (we are also working with these issues) and in our opinion the inclusion of partly unknown processes is out of the scope of the present manuscript.

(9) Suggested changes to the manuscript:

We will discuss this point in the Section 3.1.2 in which the possible water layer on top of the peat surface is mentioned.

(10) Equation 7: What about inhibition by other electron acceptors? I know you are not following them in the model, but they could be important in some fen systems. Is CH4 production from the peat matrix accounted for – anaerobic respiration is driven by rooting depth, but CH4 could be produced from other substrates.

(10) Response:

We think that other electron acceptors are an important issue. We did not include them in the model because we thought their concentrations depend on site characteristics, such as the water source, and it would be difficult to estimate them. Therefore, these estimates would necessarily not improve the accuracy of the model. However, given that our results (and also earlier works) indicate that methane production rate largely drives the simulated emissions and the oxygen inhibition thus plays a significant role, including other e- acceptors could possibly be a way to take into account site differences, for instance, bog vs. fen. This could be done in future model versions.

In the current model version, anoxic respiration is one bulk input stream and HIMMELI does not take a stand on what organic compounds are decomposed, whether they are root exudates or other substrates. For simplicity, everything is distributed with the root mass except in the case that peat depth exceeds 2 m when some respiration also is allocated in the rootless peat layers. This choice (as opposed to distributing the input e.g. evenly across the peat column) was motivated by the fact that recently fixed carbon seems to be the main source of methane. For instance, according to Oikawa et al. (2017), less than 5% of CO_2 and CH_4 emissions originate from soils below 50 cm in flooded peatlands. However, in case that

HIMMELI is used in a context where it is essential to simulate the different carbon sources, it is not a big task to modify the code so that this becomes possible.

(10) Suggested changes to the manuscript:

We will add text/discussion about the possible other electron acceptors and distribution of input carbon in the Section 3.1.3 on CH_4 production.

(11) Equation 11: Is this really realistic? This would allow a bubble to form, but that doesn't mean that ebullition occurs. Also, once bubbles form, they are often trapped and this affects the concentration gradients and also the ebullition fluxes. A very large bubble release is likely to provide such a high concentration when released that even if the WT is below the surface, not all the CH4 will be oxidized (see page 14, line 15 in the manuscript).

(11) Response:

We agree with the Referee that after a bubble has been formed there are still several processes that take place before the bubble reaches the surface and contributes to the CH_4 flux to the atmosphere. For instance, the bubbles still need to traverse through the peat column up to the atmosphere. Also, like the Referee mentions, during the time that the bubbles travel upwards towards the atmosphere they constantly interact with the surrounding pore water and hence alter e.g. the CH_4 concentration gradients.

Such processes are still missing from most of the peatland CH_4 models (Xu et al., 2016), including HIMMELI. This is most likely because relatively little is known about bubble movement in peat and how to describe it accurately in models, although there are some attempts to model this process (Ramirez et al., 2015). In general, bubble movement in porous media is a highly complex problem, which depends on the fine-scale structure of the media in which the bubbles are moving in. How to incorporate such complex phenomenon in a simple, yet accurate way in peatland CH_4 cycling models is still unsolved. However, as it happens, we are at the moment preparing a manuscript in which we are comparing different ebullition modelling approaches and one of them incorporated a simple scheme to take into account the bubble movement. Nevertheless, as mentioned this is a topic for the other study.

Considering the reviewer's comment about the page 14, line 15: in this manuscript direct ebullition to the surface takes place only when WTD is above the surface. If WTD is below surface, then the CH_4 in the bubbles is released to the lowest air layer from where it is transported via diffusion in the air-peat column to the atmosphere. Hence even large bubbles are first released to the bottom air layer below the peat surface, before reaching the atmosphere. We argue that this is how it happens also in reality and hence is the correct way to describe this process in a model.

(11) Suggested changes to the manuscript

We will discuss the points mentioned above (how the bubble movement would happen in reality, compared with the model) in Section 3.1.7. In addition, we will clarify the sentence on page 14. It now is: "…the direct ebullition fluxes to the atmosphere were zero when WTD was below the peat surface" but we will rephrase it: "ebullition to the atmosphere occurred only when WTD was at or above the peat surface". We hope this slightly modified version is clearer.

(12) Page 12, Lines 20-25: Was the model parameterized with the data from Siikaneva? If so, how appropriate was the test?

(12) Response:

Yes, the parameter set used in this study was a combination of literature values and values set by calibrating HIMMELI with Siikaneva data. In this sense the test was not appropriate for evaluating the model fit. However, the purpose of this test was to demonstrate that combined with realistic input, HIMMELI does output realistic CH_4 fluxes, which is not so evident if looking only at the mechanistic sensitivity tests. We admit this is not said very clearly in the manuscript. In addition, we think that comparing the explanatory power of input respiration only with the input + HIMMELI combination is continuation to the sensitivity tests, as it addresses the question of how necessary the transport+oxidation model is.

(12) Suggested changes to the manuscript:

As already mentioned (point 1), we will clarify the role of the Siikaneva test in the paper and, as suggested by another Referee, we will add a comparison of the model with data from another peatland site. This will be a test of how well the current parameterisation fits to other peatland sites.

(13) Page 16, lines 26-27: Does this illustrate that evaluating sensitivities in the methane only module, especially when production rates are not appropriately driven by changing conditions, is problematic? Temperature is a very important driving factor for CH4 production, but it is not included in the way the module is constructed making it very difficult to interpret the actual sensitivities of the model.

(13) Response:

We do not think it is problematic. The purpose of the mechanical sensitivity tests was precisely to find out what kind of physical mechanisms govern the behavior of this CH_4-only-module, which facilitates its evaluation. For instance, we find it relevant knowledge that the impact of temperature on the processes included in HIMMELI is principally mediated via gas solubilities by affecting the concentrations of dissolved O_2.

Perhaps we misunderstand the Referee's comment but temperature is included in the way that the module is constructed. We agree that simulating anoxic respiration is highly important in CH_4 modelling, however, the idea here is that another model (e.g. the soil carbon model of a land surface scheme) has already taken care of it. Most probably the total anoxic respiration rate provided by this other model depends on temperature, but we did not want to set any dependency here since it would have meant, in practice, that the test results are valid only when the dependency is as we described it.

(13) Suggested changes to the manuscript:

We will emphasize on p. 16 that finding different temperature sensitivities when the carbon input is independent of temperature is not a downside but new relevant information for CH_4 model development.

(14) Page 17, line 10: In this model, the peat column and layering is not important, but what about if the gas is being trapped prior to ebullition or even once mobilized from one layer and then trapped in another (e.g. Comas et al., 2015). We know this happens in reality, but it is not included in this model. If it was how would the results of the study change?

(14) Response:
Like the Reviewer mentions, bubble movement in peat is not included in HIMMELI. However, as mentioned before, we are preparing a manuscript about comparing different ebullition modelling approaches and one of the approaches that are compared in that study included a simple bubble movement scheme. We will shortly describe our findings in that study now here. As expected, if bubble movement (attach, detach) are included, then smaller amount of bubbles reach the surface, i.e. ebullition flux to the atmosphere is smaller. On the other hand, the bubbles that are attached during their ascent release CH_4 to the pore water if the pore water CH_4 concentration is low enough (Henry's law). Hence, the vertical CH_4 concentration gradient is smoother when compared to the case without bubble movement. However, we did not test different layerings with the other ebullition modelling approaches and thus, unfortunately, cannot say whether the model would be more sensitive to the layering if the ebullition model took trapping of gas into account.

(14) Suggested changes to the manuscript:
None, except what was suggested for the comment 11.

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