Dear Referee,

Thank you for taking the time to read our manuscript and for giving good and useful comments. Taking them 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 as a direct 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, typed as plain text and numbered referring to the comment number.

Overview:

The Global Methane cycle is currently a topic of much interest following the near-zero growth in atmospheric methane concentrations in the early 2000's and its renewed growth since 2007. Various papers have suggested specific sources, including emissions from wetlands, and more recently changes in the atmospheric CH4 sinks, as possible explanations. Wetlands globally are the largest single source of methane, anthropogenic or natural. Boreal wetlands are significant and could become more important still with the faster warming of the Arctic and high northern latitudes. The Wetland Model Intercomparison (cited paper by Melton et al., 2013) highlighted the current performance of wetland models and the large range of wetland areas and methane fluxes simulated by the participating models. The recent paper by Saunois et al. (2016) on the Global Methane Budget removed some of this model uncertainty by prescribing the wetland extent/area.

(1) I was expecting the present paper to make this connection to the bigger picture. From the material presented, it is very unclear what the intended application of this new model is (local, regional, global??), how it would be used in practice (standalone or coupled) and indeed what advantage it offers over those mentioned in the paper (i.e., Peatland-VU, CLM) and those already in the literature (see cited paper by Xu et al.).

(1) Response:
We are sorry that we failed to describe the aim of this model clearly. The motivation for our work was to produce a methane model that can be used in different purposes, ranging from a component of a large-scale biosphere model to a platform for specific studies on methane processes. Therefore it is not clearly stated how the model should be used; the idea is that it can be used within different environments and scales. What is true is that most probably the parameter values we used are not always optimal but the model needs to be re-calibrated, especially if using it in large-scale modelling. This we do not mention in the manuscript, but it should be done.

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. HIMMELI was developed in order to have a CH_4 module that could be plugged into different peatland carbon models and that simulates transport of CH_4, O_2 and CO_2. Rather than taking, modifying and testing directly one of the existing model codes that are developed e.g. with some biosphere model, we decided to 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. We believe this is the advantage: HIMMELI is intended to be a CH_4 module that can be used with different input sources. On the other hand, we think that as the model has components similar to other methane models, results of the sensitivity analysis can be generally relevant. In many models, oxygen is simulated but is it known whether the fluxes and effects are realistic.

We also agree that we explained very vaguely how HIMMELI relates to the existing methane models. Xu et al. (2016) listed 40 terrestrial ecosystem models for CH_4 cycling. However, when considering only their CH_4 emission parts, this number seems to be slightly reduced. For instance, Ringeval et al. (2011) wrote that they included the Walter et al. CH_4 model in ORCHIDEE and Spahni et al. (2011) that they applied LPJ-WhyMe in LPI-Bern for biogeochemical modelling of CH_4 emissions.

Although HIMMELI does not include all processes that already exist in some models (e.g. alternative e^- acceptors, anaerobic CH_4 oxidation), it is among the most complete models considering the transport of compounds. According to Xu et al., there are only 5 models that simulate all vertically resolved biogeochemistry, O_2 availability to CH_4 oxidation, and three pathways of CH_4 transport. Of these, the Xu model (Xu et al. 2007), CLM-Microbe (Xu et al. 2014) and VISIT (Ito & Inatomi, 2012) do not explicitly 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. This is not a common feature in CH_4 models: to our knowledge, only the multi-substance version of TEM (Tang et al. 2010) and the Segers model (Segers and Leffelaar, 2001) included it.

(1) Suggested changes to the manuscript:
In the Introduction, we will clarify the aim of HIMMELI and describe how this model relates to earlier methane models by adding approximately the above text that refers to the review by Xu et al. (2016).

(2) The model considers the major CH4 release pathways to the atmosphere (diffusion, plant vascular transport and ebullition) and includes oxidation by O2. O2 is the only electron acceptor considered. What about others?

(2) Response:

We agree 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 not necessarily 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 model version 2.

(2) 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.

(3) I am little concerned at the realism of completely oxic layers sitting above the watersaturated anoxic layers. In reality, one might expect a continuous transition, as acknowledged by the authors.

(3) Response:
We agree with the Referee that the choice of using water table depth (WTD) as a strict divider of the peat to oxic and anoxic parts is a simplification and as mentioned in Section 2. 'Key factors for CH_4 transport and oxidation', water-filled, anoxic sites can occur above it. In our opinion, however, it is uncertain to what extent the model-based estimate of CH_4 emissions of a peatland site or larger area would be improved e.g., by assuming a certain volume of anoxic microsites in the peat above the WTD. Peatlands have microtopography, hollows and hummocks, and even the observation-based site-level WTD is only an approximate value for the peatland, not to speak of a modelled WTD. In addition, simulating partially anoxic peat layers would bring new uncertain parameters in the model. On these grounds, we think this strict division to anoxic and oxic parts is a robust and simple approach.

(3) Suggested changes to the manuscript:
We will add discussion about how realistic is the strict division to oxic/anoxic parts of peat, on page 7, Section 3.1.2.

(4) The model runs on a daily timestep. This may be appropriate for large-scale decadal or centennial runs but no justification is given. How was this timestep selected and what are the implications for the modelled methane fluxes?

(4) Response:
The reason for running the model on a daily time step was that the main plan for HIMMELI is to use it with models that provide daily input and so these test results are useful for that purpose. However, we agree that in this work that specifically aims at testing the transport model it would be reasonable to test the effect of time step length on e.g. daily CH_4 fluxes. So far we have not done it and thus do not know the effect on output CH_4 fluxes, but we can test this.

(4) Suggested changes to the manuscript:
We will test running HIMMELI with realistic input data at frequency shorter than one day, with diurnal variation of soil temperature (as Referee 1 asked about diurnal temperature variation). Results of this model run will be compared with simulation done on daily time step, in which input data are daily averages of the previous test. The outcome will be added to results.

(5) Many of the model parameters are optimised using the measurements made at a site in southern Finland (see Table 1, p. 30). These results are included in a second paper (Susiluoto et al., 2017), which is in preparation. This makes it hard to assess their significance, especially in the light of the statement The uncertainty of some of these parameters is rather high, and a more complete analysis can be found in Susiluoto et al. (2017, in prep.).

(5) Response:
This is true. Originally we planned to include a detailed description of the MCMC parameter optimization in this manuscript but since it was already being done for the other paper, Susiluoto et al., we decided to just refer to it. However, we agree this is now left too vague and

as mentioned above, it is necessary to describe the optimisation in this manuscript also because there were some major differences between the approaches used here and in the final version of Susiluoto et al. We can add a new section in Materials and Methods that describes the parameter optimisation.

(5) Suggested changes to the manuscript:

We will add a new Section 3.2 (changing current 3.2 ‘Model testing’ to 3.3) that describes the parameter optimization process done for this manuscript and remove references to Susiluoto et al. 2017 in, e.g. Table 1.

(6) The cited paper by Rinne et al. (2007) shows an exponential dependence of the measured flux on the peat temperature to day 200 (Figure 6 in paper). The lack of a temperature dependence presumably indicates that the temperature dependence is effectively determined by that of the input 'anaerobic carbon decomposition rate'. The temperature-dependence revealed in Fig. 6 is presumably associated with the modelled transport and loss processes.

(6) Response:
Yes, this is correct. The temperature dependence in Fig 6 in our manuscript results from the impact of temperature to the processes simulated by HIMMELI, when its input respiration did not depend on temperature. Presumably the exponential dependence of CH_4 emissions on temperature would be observed if the anoxic respiration rate depended exponentially on temperature, which often is the case with soil respiration.

(6) Suggested changes to the manuscript:
We will emphasize this when discussing the Fig 6. (current p. 15).

(7) Many of the key driving variables (soil temperature, leaf area index, water table depth) could be taken either from observations or modelled. It is not clear that this is the case for the anaerobic carbon decomposition rate. If it could be measured, this would improve the utility of HIMMELI.

(7) Response:
This is true; direct measurement of the anoxic respiration rate is complex or impossible, it can be only estimated/simulated. Apparently the closest possible direct measurement would be on the CO_2 flux, which would require that the model includes simulation of photosynthesis, probably driven with solar irradiation. This would of course be possible, as we already now simulated photosynthesis (Appendix B) but our modelling work aimed at creating a module that takes anoxic respiration rate as input and thus is dependent on another model.

(7) Suggested changes:
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(8) The model is setup and the modelled CH4 fluxes are compared to eddy covariance flux measurements of CH4 made at Siikaneva, a peatland site in Southern Finland. The intake for the CH4 flux measurements is given as 2.75 m above the peat surface (p. 13). Presumably the surface is fairly homogeneous as no information is given about the footprint nor the prevailing wind direction.

(8) Response:
Siikaneva is a well-established site following the common standards and requirements for eddy-covariance measurements and its characteristics and representativeness of the data has been analyzed in several papers (Aurela et al. 2007, Rinne et al. 2007). The site is under

ICOS (Integrated Carbon Observation System) labelling process to get accepted as an ICOS Class 2 site.

(8) Suggested changes to the manuscript:
We can add the information given above into the manuscript.

(9) A good fit of the observed and measured fluxes is seen over several annual cycles. This site is effectively used for both model calibration/optimisation and evaluation. This begs the question of how general the derived parameter values are or whether are they specific to this site. There is an obvious need for comparison against measurements from other sites.

(9) Response:
The purpose of running the test with data from the Siikaneva site was principally to demonstrate that combined with realistic input, HIMMELI does output realistic CH_4 fluxes, which is not so evident if looking at the mechanistic sensitivity tests only. The parameter values are chosen to be physically sound and so they should, in principle, fit also other peatland sites but they are not given as general values for large-scale modelling. They were used here since they were optimized for the Siikaneva site. When moving to other peatlands and especially for large-scale modelling, the model needs to be recalibrated.

We agree that all this was left quite vague in the manuscript and that it would be interesting to see how well the current parameterisation fits to other peatland sites.

(9) Suggested changes to the manuscript:
We will define the scope of this part of the work and the validity of these parameter values better. In addition, we can add a comparison against 5 years of CH_4 flux measurements from another peatland site, Lompolojänkkä, a subarctic fen site in Northern Finland (Aurela et al. 2009). This would be a test on how well the current parameterisation fits to another peatland site.

(10) It would have been interesting to see upscaled fluxes to the regional/boreal scale and hence an estimate of methane emissions from boreal peatlands.

(10) Response:
This is certainly true, however, this is not within the scope of this paper. This will be done in future works when HIMMELI is combined with a large-scale land surface model.

(10) Suggested changes to the manuscript:
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Technical comments:
(11) The ellipsis (...) is used throughout the paper for 'to', e.g., page 12, line 14: '10...50 cycles' instead of '10 to 50 cycles'

(11) Response:
The manuscript preparation guidelines of this journal say: "A range of numbers should be specified as "a to b" or "a…b". We chose to use "a…b" everywhere, however, we can change this.

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

(12) Intercomparison is used in several places when 'comparison' is sufficient (a) Page 1, line 30; (b) Page 12, lines 20 and 22; (c) Page 17, line 18.

(12) Response:
Agreed.

(12) Suggested changes to the manuscript:
We will change ‘intercomparison’ to ‘comparison’.

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