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Geoscientific Model Development Discussions

Interactive comment on "HIMMELI v1.0: HelsinkI Model of MEthane buiLd-up and emIssion for peatlands" *by* Maarit Raivonen et al.

Anonymous Referee #1

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In this paper, a methane submodel is proposed for use in a larger ecosystem C model. While this is a topic of interest to readers of the journal, this submodel has several key weaknesses that affect its acceptability for publication: (1) It is driven by inputs for anaerobic respiration calculated as a first order function of peat C and root exudation derived from assumed vertical distributions of root mass in the anoxic part of the soil profile (Eq. 6) . While I appreciate that anaerobic respiration is an input rather than an output of this model, it is nonetheless the key driver of CH4 production, as noted in p. 14 and Fig. 11. Anaerobic respiration therefore needs to be explicitly simulated as part of any CH4 model, rather than optimized for site conditions as done here, as it directly determines modelled CH4 emissions. The determination of Pmax. Rref and dWtol (a poorly constrained term) in eqs. B2 and B3 is necessarily site-specific and detracts from model robustness. This optimization overlooks the possibility that anaerobic

respiration can occur in wet soil above the water table. Model testing of anaerobic respiration could have been better constrained by including tests of modelled CO2 fluxes with modelled CH4 fluxes in Fig. 10. (2) It is unclear why total anaerobic respiration does not change with WTD on p. 12 l. 5. Simulating such changes is one of the key challenges in CH4 modelling, but is overlooked in this study. (3) The fixed fraction of respiration that generates CH4 (fm in eq. 7) should in theory be fixed at 0.5, rather than be reset to 0.25 for the field study. This fraction directly affects CH4 generation, but completely overlooks acetotrophic vs hydrogenotropic methanogenesis. (4) There was no clear distinction between gaseous and aqueous diffusive fluxes in eqs. 1 – 3, although they are very different above the water table. I presume these are aqueous fluxes below the water table, but what about gaseous transfer above the water table by which gases are exchanged with the atmosphere? Perhaps this can be easily clarified by the authors. (5) The daily time step of the model eliminates the simulation of diurnal variation in temperature, even though this can be an important driver of that in gas exchange. (6) It is very important to avoid arbitrary parameterizations, such as those associated with the assumed 2 m maximum rooting depth, as these can affect model results in unforeseen ways, and therefore limit the robustness of the model. (7) The only air-water interface that appears to be modelled is that at the surface of the water table, yet such interfaces exist throughout the soil above the water table. Gas exchange across these interfaces can cause localized anaerobic zones in which CH4 can be generated. (8) Are different root porosities considered in eq. 19? These are important in plant adaptation to wetlands, as well as in root gas transfer. (9) CH4 emissions appear to have limited sensitivity to temperature (p. 15), even though a T response of anaerobic respiration was considered in the model. However field studies indicate a large sensitivity of CH4 emission to T, as noted later on p. 16, which is likely important in climate warming studies. Has a key process been overlooked here? (10) Is it realistic that CH4 emissions should increase with WTD (p. 15), or is root-mediated O2 transport overestimated? Is root growth constrained by O2 below the WT? Or is this model result an artefact of assumptions regarding WTD and anaerobic respiration noted in (2) above?

In the Xu et al. (2016) paper cited in the manuscript, 40 existing CH4 models were reviewed. In many of these models, the issues raised above are explicitly addressed, but some key challenges to further development of these models were raised. The question to be addressed when considering this manuscript for publication is does the model proposed here build upon this earlier work by providing further insight into the key processes by which CH4 emissions are controlled and thereby addressing these challenges? Or is this just another empirical model of CH4 emissions, the parameterization of which is site- and model-specific without reference to earlier modelling work, and therefore of limited interest to the larger modelling community. Unless the authors can provide convincing responses to the points raised above, then I fear the latter.

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