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Interactive comment on "Implementation and evaluation of a new methane model within a dynamic global vegetation model: LPJ-WHyMe v1.3" *by* R. Wania et al.

Anonymous Referee #1

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Review of:

Implementation and evaluation of a new methane model within a dynamic global vegetation model: LPJ-WHyMe v1.3

By R. Wania, et al.

This manuscript describes a new state-of-the-art model of methane emissions from boreal and high-altitude wetlands. The manuscript may be divided into three major sections 1) a detailed description of the model providing rationale for all representations, 2) evaluation of the model performance at several sites where peatland methane emissions have been measured, combined with a sensitivity analysis of the free pa-

rameters used in the model, and 3) an application of the model to the circumpolar domain providing an estimate of hemispheric methane emissions from sub-boreal to arctic wetlands.

The authors have done an admirable job developing a model that contains detailed representations of most of the processes that are responsible for high-latitude and high-altitude peatland methane emissions. The model description is thorough and detailed and forms an excellent reference work for other researchers, who should easily be able to reproduce the authors' methods. As such, this manuscript forms an exemplary contribution to Geoscientific Model Development (GMD) and should definitely be published. Unfortunately, the study and manuscript have significant shortcomings that need to be addressed before publication. The model evaluation and application sections of the manuscript are much weaker than the description of the model itself. It appears as though the evaluation work was done hastily, and without the goal of actually improving the model. Comparison between model results and observations are unconvincing, and the authors gloss over examples of major model-data mismatch. The entire study would be improved by showing how the model was iteratively improved in light of observations, and then presented in a final version that agrees better with observed methane fluxes. The hemispheric application of the model provides little new information and begs a more thorough analysis. These sections require major revision, or possibly deletion, before the current manuscript is acceptable for publication. For expedient publication of this manuscript n GMD, I would recommend strengthening the model evaluation section and some further work to demonstrate how the evaluation is used to improve the model, and deleting the hemispheric application completely. I would be pleased to recommend publication of this manuscript in GMD, after substantial revision.

Specific comments on the model and its description

What is the time step of the model? While the time step for calculating gas diffusion is provided (15 mins), the overall model time step is not clearly stated. I have the impression it may be daily, based on interpolated monthly meteorology as in standard LPJ, but then daily results are presented for comparison to observations. This issue of time step should be made clear. Further to this point, is it possible to drive LPJ-WHyMe with meteorology at any temporal resolution, or would only certain time steps work?

Why have the authors not attempted to distinguish between bog and fen ecosystems? Numerous previous studies have emphasized the importance of peatland age, depth of peat, geometry, and hydrologic setting for net methane emissions, e.g., recent papers by S. Frolking (Frolking et al., 2006, Frolking & Roulet 2007). It seems as though this factor would be essential in any modeling study that attempts to improve our ability to model peatland methane emissions. The observation that net CH4 emissions are reduced as boreal wetlands age from fen to bog is not discussed in the context of LPJ-WHy-Me. On the other hand, implementation of some distinction between bog and fen could be an extremely valuable feature of any new boreal wetland methane model. At least the authors should discuss the implications and practicality of modeling such a distinction.

Specific comments on the model evaluation

How was vegetation modeled at each site? Prescribed based on observations, or allowed to evolve freely by LPJ? If the vegetation was allowed to evolve directly from the model simulation, why was this done? The authors need to provide some rationale at this point. Given the importance of plant-mediated transport (shown at several sites), wouldn't it have been more logical to simply prescribe the extant vegetation at each site based on the detailed field observations?

Why have the authors not made any comparison with measurements at large boreal wetlands that display little or no methane emissions, e.g., Mer Bleue in Quebec, Canada. This very well studied site is often cited as a typical boreal peatland representative of large areas of peatland across the boreal zone. Yet it has been shown that the same wetland has little methane flux across most of its area. It would be very helpful to

comment on this in light of the LPJ-WHy-ME results and to demonstrate that the model is capable of reproducing low-emissions wetlands.

Furthermore, why have the authors decided not to run LPJ-WHy-ME with observed meteorology at more eddy covariance sites where detailed meteorological, hydrological, botanical and other measurements are available for the model evaluation? Eddy covariance stations besides Abisko have been established in many boreal and tundra wetlands in recent years. These include the Lena Delta, Zackenberg, Mer Bleue, and west Siberia. There are probably more sites I am not aware of. Some of these sites also measure methane fluxes. Instead of the crude approach the authors used, choosing the nearest point in gridded meteorology and comparing to modeled to observed methane emissions, the evaluation of the process representation of methane emissions would have been much more convincing if it had been done using in-situ meteorology, vegetation composition, and measured fluxes. The approach the authors use at the Abisko site is promising, but needs to be expanded.

Section 32, paragraph 11. I do not believe that the Mastepanov et al., "methane freeze-out" phenomenon has been observed at Abisko, even though measurements there have covered several periods of freeze up at the beginning of the winter.

Finally, though the authors comment on possible improvements to LPJ-WHy-ME in the context of the model-data mismatch, it appears that they haven't actually done any of these things. The entire paper would be greatly strengthened if the authors actually made some changes to improve the model in this version and described how they did this in the paper. Instead of simply stopping with the parameter sensitivity test and an acknowledgement of model limitations, e.g., where observations were outside the range of any possible parameter combination, the authors could have made improvements to the model. For example, it will be useful to see how LPJ-WHy-ME can be modified to improve model's ability to simulate peak emissions closer to the observed magnitude at southern sites, and then showing how those changes affect the model results at different sites or on larger spatial scales, e.g., in a cross-validation technique.

Specific comments on the model application

As I described above, I believe that this manuscript would benefit from removing the application section. There is enough very good content in the model description and evaluation sections of this manuscript to warrant publication. The application section could be saved for its own paper, where it could benefit from a much more thorough analysis, e.g., evaluation of simulated peatland vegetation in light of observations, a breakdown of emissions at regional scale, more placement in the context of other recent forward and inverse attempts to estimate large-scale wetland methane emissions.

If the authors insist on including this section in the current manuscript, then the following issues need to be addressed:

In applying a map of prescribed wetland distribution, why have the authors decided to take a completely new approach to mapping peatland distributions instead of using the an established dataset such as the GLWD (Lehner & Döll, 2004) or composite approach such as that described in Bergamaschi et al. (2007). Using one or the other of these maps would have the added benefit of being able to make a direct comparison of flux estimates presented here with earlier studies.

In any large-scale model analysis of the wetland methane emissions, an essential parameter must be the scaling factor for wetland productivity. Here, the authors choose 75% based only on small-scale field-based estimates of wetland microtopography. However, the spatial resolution used in the current study models wetland methane emissions on a 1x1 degree grid (roughly 100x200 km at northern latitudes). At these scales, forests, rivers, and other landscape heterogeneity exists even in areas identified largely as wetland, e.g., see Roulet et al. (1994) detailed maps of the Hudson Bay Lowlands (HBL). At very least, the authors should have compared their modeled fluxes to the few large-scale regional estimates of wetland methane emissions that are available, including those for the HBL, e.g., Roulet et al. (1994), Worthy et al. (2000). Furthermore, the authors could have made extensive comparison of hemispheric emis-

sions to the forward and inverse estimates of wetland methane emissions provided by Bergamaschi et al. (2007).

Finally, at large spatial scale it would be essential to investigate how simulated peatland vegetation compare to observations, e.g., from Canadian peatlands database, or other floristic information from wetlands (there must be enormous amounts of these data available). Also it is not clear from this paper how forested peatlands are handled or if they occur at all. This evaluation of simulated wetland vegetation may be the subject of another paper, but it deserves discussion in the current manuscript because the plant-mediated transport is so important and depends largely on extant vegetation.

Style comments

While the writing style and English usage in this manuscript are excellent, the authors' frequent use of the passive voice makes the manuscript a bit tedious to read.

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