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Geoscientific Model Development Discussions

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.

R. Wania et al.

rita@wania.net

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We would like to thank referee #1 for a very constructive review and for the helpful comments and suggestions for changes. We will respond in detail to each of the comments below.

The model evaluation and application sections of the manuscript are much weaker than the description of the model itself.

We agree that there is an imbalance, but this arises partially from the nature of the paper, which is a model description paper. There is therefore more emphasis and more detail in this part of the paper than in the evaluation and application sections.

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Interactive Discussion

Discussion Paper

We will try to improve the balance between the three parts of the paper by adding statistics to the evaluation section, elucidating the evaluation process in more detail and discussing the regional budgets some more.

It appears as though the evaluation work was done hastily, and without the goal of actually improving the model. [...] 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 evaluation work was used purely to decide on a set of model parameters, rather than as a route to changing or adding to the model structure. The model is a prototype of a new methane emissions model that can be used for further studies. There are many aspects of the model that can and will be improved through further work (for example, Paul Miller from the University of Lund has added dwarf shrubs as additional plant functional types to the model), but we needed to draw a line under the development of the prototype model at some point so as to be able to publish the model so that other people can build on it.

We will emphasize this viewpoint more clearly in a revised version of our manuscript.

Comparison between model results and observations are unconvincing, and the authors gloss over examples of major model-data mismatch.

Again, we made the mistake of not stating the purpose of this model explicitly enough; the results of the model-observation comparison should be seen in the light of this purpose. We wanted to develop a circumpolar methane emissions model that can be driven by a minimal amount of input data. LPJ-WHyMe uses only climate data (temperature, precipitation, number of wet days and percentage of cloud cover), atmospheric CO_2 concentration, a soil type map (8 classes) and a peatland fraction per grid cell map as input data. Based on these, the model simulates everything starting from soil temperature, water table positions to methane emissions. What we wanted to show with the model-observation results is how well LPJ-WHyMe can do **despite** the fact that it uses only a minimum amount of data and runs on a 1 x 1 degree resolution.

We will add some more discussion on the examples of major model-data mismatches.

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.

We understand the referee's point of view here, but are unsure how to proceed. Although the circumpolar study deserves its own paper, this application was the primary reason for the development of LPJ-WHyMe, and serves as a good test of the performance of the model under the conditions for which it was developed. We could include more comparisons to site-based observations, but these are of less relevance to the questions of the large-scale performance of the model in terms of calculation of hemispheric methane emissions.

We would appreciate guidance from the responsible editor on this point. Would it be preferable to remove the circumpolar emissions application and defer presentation of these results to a later publication? If so, it is not clear to us how we might effect these changes without obscuring the primary intent of the development of LPJ-WHyMe.

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?*

The time step is 15 mins for the gas diffusion calculation, 0.5 day for the heat diffusion calculation and one day or more for all other processes. We will add this information to the manuscript. We tested running LPJ-WHyMe in its current configuration using daily meteorological data, but encountered numerical instabilities due to rapidly changing air temperatures in the driving data. We therefore use only monthly input data.

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 LPJWHy- 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.

This is a very good point and we agree that we should make some comments on the practicality of modeling bogs and fens to the revised manuscript (or, more correctly, the difficulties of doing so). We are aware of the importance of distinguishing between fens and bogs for the purposes of methane emissions calculations, but from a modelling perspective, it is a very tricky thing to do on a circumpolar scale. If we wanted to simulate fens and bogs, we would need to simulate groundwater, which is both inherently difficult and well beyond the scope of a dynamic vegetation model such as LPJ-WHyMe. A first step would be to prescribe the distribution of fens and bogs and to run LPJ-WHyMe separately for those two ecosystems via a built-in switch. However, that requires a circumpolar map giving the percentage of fens and bogs in each grid cell. For some areas, these maps are available already, but for others they are not.

Peatland scientists have yet to agree on a best map of current peatland distribution, let alone bogs *versus* fens — this last point was made clear by discussions at the last PEATNET meeting on Peatlands in the Global Carbon Cycle (Prague, September 2009). We are very tightly constrained by the availability of hemispheric datasets in this (and other) areas: while detailed information of the kind required to perform these more discriminating simulations is available for individual study sites, there is no consistent source of information for the whole of the region in which we are interested.

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?

This is a key observation, and is one we should have addressed more clearly. It is very useful to get the perspective of a reader who hasn't been immersed in this work: there were some unwritten assumptions lying behind our study that should have been written! Vegetation growth was modelled by LPJ. As mentioned above, our aim with this study was to have a model that can be used on a regional scale. This means that, for the majority of grid cells, no information on vegetation composition was available. We therefore did not want to prescribe vegetation composition even for the seven test sites that we used where detailed vegetation information would have been available. We believe that, for the intended purposes of our model, it was imperative to show how well the model performs *without* site-specific data. It is clear that we did not make this rationale clear enough in the manuscript, and we will correct this deficiency.

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.*

We considered using the Mer Bleue site in our evaluation. However, one of LPJ-WHyMe's current limitations is that the water table position is allowed to fluctuate only between 10cm above and 30cm below the peat surface. This limitations stems from the use of the hydrological scheme from Granberg et al. 1999. The water table at Mer Bleue is often below the lower limit of -30cm and we therefore could not include it in our analyses.

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.

We chose not to run the model with observed meteorological data from specific sites because, in this first instance of evaluation, we were interested in how well the model performs in the configuration that will be used for circumpolar simulations. For other projects, we have used site-specific meteorological data to run LPJ-WHyMe, but this was not the focus of this study. As mentioned already above (although not clearly enough in the manuscript), our goal was to develop a regional model that does not require a lot of input data and to show how well the model performs given those constraints.

At the time this study was conducted, gridded climate data from the Climate Research Unit at the University of East Anglia were available only until the year 2002. We therefore chose observations that were collected before January 2003. We are not aware of any eddy covariance (EC) data for methane emissions that are available before this date. We chose to include the Abisko site nevertheless to function as a representative for EC measurements.

A study as suggested by the reviewer, namely running LPJ-WHyMe for EC sites and comparing all of the available data merits a publication of its own, one which we hope to write at some point in the near future.

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.

Agreed. We will rephrase the comparison with Mastepanov's work.

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.

The model as presented in our manuscript is the end-point of four years of work. At some point in the development of a model, it becomes necessary to pause development and publish, even if the model is not "perfect" (no model ever is anyway). Although we are aware of a number of deficiencies in LPJ-WHyMe and a large number of avenues for future development of the model, pressure from potential collaborators obliges us to publish something about the model in its current state.

One of the primary reasons for submitting our manuscript to GMD is the possibility of maintaining a connected "portfolio" of publications collating future development of our model. That portfolio has to start somewhere. In our opinion, the model has reached a state where publication of its description is of interest to the community, even though we are aware of deficiencies and potential improvements, some of which are mentioned in the manuscript.

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.

We appreciate the concerns of the reviewer here, and as mentioned above, we have some difficulty in deciding what the correct approach to take is. However, from the point of view of presenting a coherent case for the approach we have taken in the development of LPJ-WHyMe, we would prefer to keep the model application section in a revised manuscript. The main purpose of this model development is the regional application; we want to point the reader towards the possible application of the model, but also give a circumpolar methane emissions estimate to indicate how LPJ-WHyMe compares to other models. We do agree though that there are many further aspects of the circumpolar emissions question that could be addressed and would like to leave detailed analysis of the circumpolar region to a separate manuscript (one aspect of this is already in progress, mentioned below in the context of inverse modelling).

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 Doell, 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.

It is not clear to us that there is a unified opinion amongst peatland scientists of which peatland map is the best. We are not aware of any studies that estimated wetland methane emissions based on the Lehner and Doell, 2004 wetland map, so there would have been no added benefit (at least not at the moment) in that respect. The Bergamaschi et al. (2007) map would have been beneficial to us if it had been available earlier, but by the time of its publication, we were already using the IGBP-DIS map. Since we had published results on the land surface processes and vegetation dynamics in LPJ-WHyMe using the IGBP-DIS map, we decided to continue to use this data for the methane emissions study, in order to maintain consistency and comparability of results with that earlier work. We completely agree though on the usefulness of different studies using the same peatland/wetland distribution to estimate methane emissions in order to compare different models.

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

First, if the peatland map we used includes rivers, forests and other landscape features, we cannot expect the model to account for those. We rely on the input data we feed into the model and if the peatland map tells the model that 50% of the grid cell is peatland, then the model will simulate the carbon dynamics for those 50% as if they were peatlands. We will attempt to take the large-scale regional estimates into account in our revised manuscript but, as is clear from many of the issues the reviewer has addressed, we are strongly constrained by the absence and difficulty of generation of consistent regional datasets for these important model input parameters.

Second, we have been involved in an extensive inverse model study where our peatland methane emissions were scrutinized. This work will be published in a separate manuscript (R. Spahni et al. in prep), and we will add a reference to this work in the current manuscript.

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

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plant-mediated transport is so important and depends largely on extant vegetation.

We will add some more information on how the simulated vegetation compares to observations at least at the seven test sites. We conducted a detailed evaluation of the vegetation and carbon dynamics in peatlands in Wania et al. 2009 and therefore did not put much emphasise on it in this manuscript. We will make the references to this earlier work more explicit.

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. Interactive comment on Geosci. Model Dev. Discuss., 3, 1, 2010.

We will correct this. Being tedious to read is the last thing we want!

Interactive comment on Geosci. Model Dev. Discuss., 3, 1, 2010.

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Interactive Comment