November 22, 2012

Biogeosciences Discussions

Manuscript: Present state of global wetland extent and wetland methane modelling: Conclusions from a model intercomparison project (WETCHIMP)

Author Reply to Referees #1 and #2:

Dear Editor,

This is our author reply to the two Anonymous Referees for our paper, 'Present state of global wetland extent and wetland methane modelling: Conclusions from a model intercomparison project (WETCHIMP)'. We wish to thank both referees for their time and care in providing comments on our manuscript. We will answer each in turn beginning with Referee #1.

Our comments are presented in blue font. Each Anonymous Referee's original comments are in black.

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Referee #1:

General.
The WETCHIMP project is a timely model intercomparison project for wetland methane emission models. The article describes the results of the WETCHIMP model experiments. It shows that large discrepancies between model-based wetland CH4 emission still exist, with a four-fold difference between the lowest and highest estimates.

A disadvantage of the article is that information on model structure of the participating models is lacking. Even a simple table comparing which processes are included in the various models is absent. Instead, much reference is made to an article of Wania et al (2012), which is not yet submitted according to the reference list. This reference should not have been included, as long as it is not accepted for publication.

The outcomes of the sensitivity tests to which the models were subjected (increased CO2, air temperature and precipitation) are interesting, although the design of the tests raises doubts (e.g. a stepwise increase in CO2); it should be explained in a better way why the experiments were set up in this way.

Not all models prove to model the same domain consistently. All models have a global domain including the tropics, but one model (LPJ-WhyMe) includes only northern wetlands. It is not clear why this model is restricted to a smaller domain, or why it is included if the domain cannot be extended.

The conclusions of the article are somewhat disappointing. They do not reach any further than the obvious statement that further work on better parameterization and evaluation of the models is necessary. A discussion of possible causes of the wide discrepancies between models is absent. The authors point out the lack of good observation data for model evaluation and the large uncertainty in observation datasets on wetland extent. But a discussion on which components of the model structure and parameterization may influence the large discrepancies between the models is equally important.

Furthermore, throughout the paper there are several inconsistencies, in particular in terminology on wetlands, their definition, vegetation, soil types. These should be corrected.

In any case this paper clearly shows that after some twenty years of research we still do not know much on the most important source of atmospheric methane. This is the true merit of this paper, which should be published with relatively minor revisions.

We are pleased to hear that the referee feels our project to be timely and of value. The referee raises five main criticisms in the General remarks.

First, the Wania et al. (2012) paper is cited in the article but was not at that time published or available. We apologize for this, however we had originally noted to the Editor during submission that we could make the MS available to reviewers if it should be required. The paper has now been submitted to Geophysical Model Development (October 30th).

Second, regarding the desired explanation for the design of the sensitivity tests, we were seeking as simple a comparison as possible, to avoid confounding issues in the simulations. As a result we chose to perform step-changes from an equilibrium state and then relaxation to a new equilibrium state. This was done to avoid transient effects as the models adjust dynamically to forcing changes. Given that this was the first wetland extent and wetland methane model intercomparison, we felt it appropriate to keep the tests simple to ease interpretation of the results. We have added more discussion on how the sensitivity tests were selected in the MS.

Third, it is correct that not all models were global. We had two models that were large-scale, but only regional (LPJ-WHyMe and UW-VIC). LPJ-WHyMe covers the Northern peatlands while UW-VIC is specific to the West Siberian Lowlands. As WETCHIMP was never intended to be a global model exclusive project, we have allowed participation by large-scale regional models. LPJ-WHyMe is a peatland only model, thus not applicable outside of peatlands, which explains why it was not extended to the entire global range. UW-VIC is also high specialized for its region, and thus not applicable outside of the West Siberian Lowlands. The results of UW-VIC will be presented in a follow-up manuscript focusing on the West Siberian lowlands and the influence of a region-specific model compared to global models for regional-scale modelling (T. Bohn, personal communication, 2012). These non-global models allow investigation of the benefits of a model tailored to a specific location or wetland type vs. the (usually) more general treatment afforded by global models.

Fourth, the Referee finds the conclusions of the paper to be somewhat disappointing. We, ourselves, would have preferred to have found strong conclusions of the differences between models and against observational datasets. Unfortunately, at present, the best available observational datasets to evaluate the models are inadequate. Indeed we feel this is an important outcome of our project, that further improvements in our models are intricately linked to further improvements in the datasets we compare to. We hope that our study catches the attention of the observational community and sparks further collaboration with modellers to help develop observational datasets that can be used for model improvements and evaluation.

Given the complexity of the models, it is difficult to determine with greater precision what components of the model structures or parametrizations would yield the large differences we observe between models. Probing deeper into the model structures is outside of the ability of this iteration of the WETCHIMP given the chosen experimental design. For this iteration of WETCHIMP, we have accomplished our goal of determining the different model responses to identical forcings and the state of our ability to evaluate the models. This is a novel contribution to the community and allows a benchmark against further model developments. However in future WETCHIMPs, experiments could be designed to help us understand the actual model components and parameterizations responsible for the outputs created.

Lastly, the Referee found inconsistencies in terminology. This is likely due to the description of 'mineral wetlands' and the LPJ-Bern methane source, 'wet mineral soils'. They are not the same thing, and we have added text to make this distinction more clear.

Detailed remarks.

77-85: I miss here a reference to Petrescu et al (2008), who first demonstrated the large variability in CH4 model outcomes related to wetland area.

A reference to this paper has been added in the discussion body as it is more relevant there.

96: Wania et al (2012) is not yet published, not even submitted. This reference should not be used. Moreover, even if it was published, it would be very convenient for the readers if the essential differences in methodology and model structure were listed here, e.g. as a table.

Discussed above. As both papers are published in open access journals, we feel it makes sense to keep the papers separate and allow the Wania et al. (2012) paper to do a more thorough explanation of each model and how they differ. We have summarized the main differences in model wetland extent determination and methane production in Table 1.

115: 'Mineral wetlands are dominated by vascular plants that facilitate CH4 transport through their roots . . . . Unlikely. Aerenchymous wetland plants are not restricted to wet mineral soils but also occur abundantly on peat soils.

Mineral wetlands is a defined class of wetlands given by the National Wetlands Working Group (1998) as described on page 11583 line 11. This is not to be confused with 'wet mineral soils', which we classify as a separate, non-wetlands, methane source. It is unclear what the referee finds unlikely about our statement. We do not state the aerenchymous plants are limited to mineral wetlands. We simply state that mineral wetlands have large amounts of them.

123: The wet mineral soils simulated by LPJ-Bern are not mineral soils according to your wetland definition. However, the wetland definition in 110-113 includes 'mineral wetlands'. This is confusing, give a better explanation why the LPJ-Bern mineral soils are excluded from wetlands.

Mineral wetlands and wet mineral soils are separate classes of methane producing regions. While both use the word, 'mineral', this does not imply that they are one and the same. Mineral wetlands are a distinct wetland class (National Wetlands Working Group, 1988), while 'wet mineral soils' are a proposed $CH_4$ source that is modelled by LPJ-Bern. This source is distinct from wetlands in their model. The 'wet mineral soils' parameterization of LPJ-Bern allows $CH_4$ production if the soil is moist enough and has some organic matter to mineralize, *regardless of slope*. This lack of dependence on slope is a key difference. We have added in a sentence to make this more apparent.

130-134: Like in the previous comment, this paragraph illustrates again that the distinction between wetland and non-wetland is quite artificial. From a modellers perspective it would be quite logical to consider any soil that is water-saturated or flooded most of the time as a wetland.

Across the whole spectrum of wetland research, the distinctions of a 'wetland' vs. 'non-wetland' are going to be artificial and somewhat fuzzy. A wetland is not an easily delineated land form. We assume the referee is discussing pg. 11584 lines 1-9. We do not include lakes, rivers, rice agriculture, saline-estuaries, salt marshes, and reservoirs in our modelling and definition of a 'wetland'. From a modelling perspective this is because of the distinct hydrologic (rice agriculture, lakes, rivers and reservoirs) and geochemical (saline estuaries and salt marshes) regimes of these systems. An attempt to model them all as 'generic-wetlands' will result in inaccurate and misleading simulations. As a result, we have excluded these systems.

180: See my remark with line 96: the references to Wania et al (2012) should be replaced by an adequate summary of the main structural differences between the models. The summary in line 163- 180 is very general and hardly gives information on individual models. Table 1 only refers to the parameterization of wetland area.

Discussed above. The main elements of each model's methane parametrization have also been added to Table 1.

194-195 Although the CO2 increase experiment proved to be quite useful, the instantaneous increase of CO2 prom present-day values to 857 ppm is an unrealistic approach. Explain here why this approach was chosen instead of a more gradual increase.

As discussed in General remarks, to allow for a more simple interpretation we chose to compare model equilibrium states. A transient response to changing $CO_2$ would be more complicated to interpret, but is likely a useful simulation for future iterations of WETCHIMP.

204-208 Here is admitted that the step changes are unrealistic. However, although it is stated that this is ‘suitable for the purpose of the sensitivity test’ no really good arguments are given. The last part in this long sentence, on the use of the R statistical package, seems misplaced here. Line 194- 208 should be rewritten, providing a better argumentation for the stepwise changes in the sensitivity tests.

The purpose of the sensitivity-tests was to examine model responses to these changes amongst the participating models. As a result, the realism of the step change is relatively unimportant, the importance is on the relative model response, not the absolute value. The sentence has been reworded to make that more apparent and to integrate the details about the data analysis better.

Table 2: According to this table the LPJ-WhyMe only considers northern peatlands. What is the use of including this model, when the other models do global simulations? Could the model domain not be extended? Give good arguments why the model is included despite its restricted domain.

LPJ-WHyMe is included because it fulfills the criteria of being a large-scale model as discussed above in General remarks. More appropriately, it offers an important counterpoint for the global models. While LPJ-WHyMe is designed to simulate peatlands only, many global models do not explicitly treat peatlands as different from tropical wetlands. Comparing the two kinds of models allows us to evaluate the differences between explicit peatland (with specific PFTs and treatment of lawns and hummocks, etc.) and more general ‘wetland’ simulations.

445 why does this affect primarily the tropical wetlands?

Tropical wetlands are primarily affected due to the large systems of floodplains in the tropics. However, this statement is weak and would require more information to prove definitively - so we have removed it.

479-480 This is awkward – determining peatland extent based on CH4 emissions. Here also the terms ‘peatland’ and ‘wetland’ are probably confused. Please clarify.

Agreed. We have tightened up the language here. For the simulations by LPJ-Bern, the peatland extent is unchanging, as you would expect. However, since most wetland areas are prescribed and/or constant in their simulations, the LPJ-Bern approach has been to use the modelled methane emissions from each grid cell to determine ‘active’ wetland extent for the analysis of the results. What this means is that amongst all of their modelled ‘methane producing areas’, which include peatlands (constant), inundated areas (from GIEMS), and wet mineral soils, the ‘wetland’ extent is taken as those areas that are actively producing methane in that year. This is a slightly different approach than most models. The reason for this approach is LPJ-Bern’s inclusion of their ‘wet mineral soils’ methane source, whose areal extent is determined solely by their methane emissions. By then also taking this same approach (determining ‘wetland’ extent by methane emissions) across all of their methane producing regions, they can approximate a ‘wetland extent’ for comparison with other models.

690-692: Normalizing with respect to what? Please clarify.

Yes, thanks. We have now clarified in the text. The data was normalized to the peak value across the 1993-2004 period. This sets that value to one thus each model then ranges between 0 and 1.

779-783: Here the lack of information on model structure becomes crucial. I miss an explanation why the wetland extent increases. Is this an effect of increased water use efficiency at higher CO2 levels that decreases evaporation, or other causes? How do the models differ in this respect? Some models show a quite strong response in figure 9!

Sorry, the discussion on wetland extent increases was written in such a way as to appear unique to UVic-ESCM. These processes are not unique to that model so it has been rewritten to be a more general explanation for all models. The basic mechanism is elevated [$CO_2$] levels allow plants to increase stomatal closure, decreasing plant water loss and water demand from the soil. The decreased water demand from the soil increases soil moisture and increases wetland areas. The response differs between models due to model parameterizations. Models that estimate wetland area directly from soil moisture could see a larger effect than models that estimate wetland area from water table position. The complexity of the model soil hydrology and soil physics will have an influence as well as the particulars of the grid cell including type of vegetation and freeze-thaw cycles.

803-807: Again, a table comparing model structure and methods would have been very useful here.

Discussed previously, model structures and methods are available in an open-access article by Wania et al. (2012). Note that a table on model structure does not mean that interpretation of the models' behaviour becomes simple. We had full access to all teams' knowledge of their models, and it was still very difficult for us to narrow the exact causes of discrepancies between the models.

977-980: An interesting conclusion that the models behave better for the northern latitudes. A bit speculative: could it be that most models have been specifically designed for northern latitudes – and have been tested on higher latitude data?

It is doubtful that the models have been specifically designed for high latitudes. Perhaps more likely is that they have been better tested for high latitudes. There is a large amount of observations for high-latitude sites and a relative paucity of sites in the tropics.

1018-1027: I agree that more testing should be done and better data should be available, but I would have expected something more substantial in this conclusion section. The important question is, how good is the structure of these models? What elements may be missing? In line 1015, this is touched upon only briefly, by mentioning nutrient limitations, but there could be more. For instance how spatial spatial heterogeneity of natural wetlands is included in the models – if included at all. Exploring model structure and method effects could make the conclusion section – and the article – more interesting to read.

We are limited by what we can suggest given our ability to properly evaluate our models. This paper shows that, presently, we are not able to adequately evaluate our models. To provide more concrete information on improvements to model structure, etc. we would require a more robust ability to evaluate the models. As it stands now, we have provided suggestions based upon what we have found in our study, while being careful to not overstep the limitations of our present ability to evaluate our models.

Minor remarks, grammar, style, typo's

In line 96 please delete 'please'
403: Put 'however' at the start of the sentence.
527: 'niether' = neither
576: after 'reason' add 'that'
Figure 6: the grey dashed lines are hardly visible

All are now fixed.

Figure 8: The legend appears incomplete. What do the colours represent exactly?

There is a legend in the upper right panel. We have added to the caption to make this more explicit.

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**Referee #2**

General Comments

The paper of Melton et al. aspires to describe the present state of modelling wetland extent and its associated methane emissions. It does so by describing the results of a model intercomparison project, WETCHIMP. In my opinion the paper is extremely comprehensive, perhaps overly so - see below - though its full impact will only be realised with the publication of the companion paper by Wania et al. in which the contributing models are described in more detail. This will allow readers to evaluate the extent of the "parameter and structural uncertainty" touched upon here, but never adequately discussed.

The findings are worrying in the sense that state-of-the-art models still show such wide disagreement in both simulated wetland area and $CH_4$ emissions. There is range of +/- 40% in the latter, whereas Wetland area, the authors conclude, is at present very difficult to evaluate. The response to the sensitivity experiment in which the temperature was increased varied between models. However, the models do show a clear and consistent response to a (large, stepwise) $CO_2$ increase.

In places the paper's readability suffers because the authors describe the output of every model, with overly long sections as a result. Sections 3.1.2 and 3.2.2. in particular are very long, and their detail makes it hard to see the wood for the trees. I believe these sections could either be shortened considerably, or summarised in the main text with the existing detail moved to appendices.

As mentioned above, the details of the models is lacking, though I understand the idea behind a separate paper with the details. However, I don't think it would be to preempt or spoil the Wania et al (2012) paper if a summary table was included with information relating to the models and the processes they include. E.g. transport pathways, pH, nutrients etc. Indeed, I believe it would whet the appetite, especially if the uncertainties revealed in this paper were discussed, in the final section, with reference to the models' unique features. (This is not to say this is never done in the paper, of course, e.g. in the differences due to the inclusion of permafrost in Section 3.1.1)

Finally, how can we "narrow the uncertainty" (p. 11609) revealed here? The Conclusions mention CH4 observations, and clearly tropical observations along the lines of the HBL data shown would be very useful indeed. But where should this data be gathered? The WETCHIMP modellers could use this opportunity to influence the process! And do the authors have recommendations as to how uncertainty in wetland area could be reduced? What fraction of the CH4 flux uncertainty could be reduced if area uncertainty was reduced?

I recommend that this paper be published with minor revisions, adequately addressing the issues listed here and below.

We thank the Referee for the comments. Many of the comments show a very thoughtful reflection on our paper. The three main points of criticism will now be addressed.

First, the paper is too comprehensive. This journal does not have page limits and we thus felt it reasonable to use the space to fully explore our data and provide a full analysis.

Second, Wania et al. (2012) is not available. As mentioned in our response to Referee #1, we apologize for the Wania et al. (2012) paper not being available. We did offer to make it available if need be when we submitted this MS. The paper has been submitted to GMD (October 30th). A summary table, as suggested, would be very difficult to produce for all of the different aspects of each model's parameterization, indeed Wania et al. (2012) has several lengthy tables for that express purpose. We do acknowledge the Referee's point that more information about each model would be helpful so we have added to Table 1 of this MS a brief overview of the main variables involved in each model's wetland methane flux parameterization. Detailed information is left to Wania et al. (2012) where the model parameterizations can be treated at the appropriate depth, along with detailed information about the intercomparison protocols.

Lastly, the Referee would like to see expanded recommendations on model development and narrowing of uncertainty. We have taken this advice and expanded this section of the MS. We have included a discussion of which areas (spatially) would be of most benefit and what recent initiatives appear to hold promise. We have also added discussion on what specific variables should be looked at. Also we discussed how site measurements could be made more useful for modelling on a global scale.

Specific Comments (addressing individual scientific questions/issues)

Section 2.2.1 - Could we have a new table listing features of the participating models of relevance to this paper? Or extend Table 1 with this information? Do all the models simulate a water table depth? Can it be plotted?

We have extended Table 1. Not all models simulate a water table depth. This is addressed in detail in Wania et al. (2012), now available on-line.

Section 2.2.2 - Please explain the reasons for choosing the sudden, uniform step change approach adopted in Experiments 4-6. Why not, say, a longer transient experiment continuing beyond 2009 to 2100?

We have added more discussion in the MS explaining the use of step-changes, as we have already outlined above for Referee #1's comments. A longer transient experiment for 2009 onward would be hampered by a lack of inundation dataset to drive any models that require it. Also the transient response of the models would be even more difficult to interpret given the already difficult interpretation of the models with a simple step-change.

- Why not an experiment with simulataneous T + CO2 + precipitation changes? I think a sentence justifying its omission is needed here.

In retrospect, this would have been a good experiment to replace experiment 3. However, for some modelling groups experiment 3 was desirable as it allowed them to run their model in its native configuration and compare that to our standard configuration. Since we already had a large amount of data, we have not presented any of experiment 3 in this paper. There are many different possible experiments we could have run, some mentioned by Referee #1, but in the end we had to choose a manageable number that would still allow us to properly investigate our research questions. We feel that our experiments do accomplish that. Other experiments of interest are possible for future iterations of WETCHIMP.

- Also, given that wetland area variability contributes substantially to the CH4 variability (Section 3.2.3), could there not have been an experiment in which all models ran with the same, static wetland map(s)? This would remove that element of variability, at least.

This question is similar to the one above, with a similar response. Specific to this question, yes this could also be an experiment for a further WETCHIMP iteration. Given computational and time constraints, we had to choose a limited set of experiments that would best help us with our research questions. For some model's this would have been non-trivial to set-up a consistent approach to a static wetland map (for e.g. some models don't track water table depth, while others only track soil moisture. Making both types of models consistent against each other would be important).

- In general, the results from years 1901-1990 (approx) in the transient experiment, the most realistic, are underused in the paper. Please justify.

These years are actually less realistic. Any model that takes in an observational dataset (primarily GIEMS) will be using the observed data outside of the time period it was actually observed for these years. We chose the comparison period to coincide with the actual years of observation for the GIEMS dataset (1993-2004) so the models will be compared most realistically.

- Are the results of the third experiment used at all?

Not in this paper, but hopefully in further follow-up papers. As the referee notes, this paper is quite comprehensive as-is.

Section 3.1.2 - Please shorten or move some details to an appendix? Very long, though the final paragraph is a good, welcome summary.

Since there is no page limit in BG, most editors have been reticent to move data outside of the body of the paper.

Section 3.2.1 - you make a good case for the need for tropical studies along the lines of the HBL studies you mention. Indeed, use of the HBL data is a very strong feature of the paper. But, given that these data do not exist, why not use those site data that do exist, both tropical and boreal, acknowledging the uncertainties involved?

The use of the data that may have significant uncertainties due to up-scaling, sample representativeness, or other errors does not reduce the uncertainties in our model outputs. This will be a zero-sum effort where any conclusions from the site-level data will be overshadowed by uncertainties regarding the appropriateness of the treatment of the data.

Section 3.2.2 - Please shorten, or move some details to an appendix?

Again, we prefer to keep it in the body of the paper.

Section 3.2.3, p.11608 - the discussion of El Nino of 1998 is very interesting. Here I think there is a missed opportunity to use results from the transient experiment. Could all the El Nino years have been selected, 1901-2009, and CH4 anomalies analysed relative to normal years?

This is an interesting suggestion. However, we are hampered by (many of) the models requiring an inundation dataset, which is only available for the time period 1993 - 2004. Outside of these years the models that take in, or use, the inundation dataset are still driven by these same years.

Technical corrections (Page/line number) 11579/26 - +3.4 degrees C

Thanks

11581/17 - How about a reference to hypothesised CH4 involvement in the PETM?

The only paper that we are aware of regarding CH4 in the PETM would be the recent one by DeConto et al. (2012), but this paper does not model wetlands, instead discussing permafrost C.

11581/27-11582/10 - Intermediate between the M&F approach and the process-based models is, e.g., the work of Christensen et al. (Tellus, 1996).

Yes, the approach of Christensen et al. (1996) is somewhat intermediate. We have included it in a much more thorough literature review that is in the introduction of Wania et al. (2012). For this paper, we are wishing to keep the introduction brief, as the referee already notes, the paper is quite long.

11583/19 - What does "treated as a generic wetland type" mean?

No distinction is made between different wetland types. This means that the differing biochemical, hydrologic, or vegetation of the different wetlands are not distinguished or treated separately. A good example would be bogs vs. fens, where they are both just treated as 'wetlands'. We have added more explanation to the MS.

11584/4 - "...of natural wetlands." Reference needed here, perhaps.

Yes, we have now added one.

11584/21&22 - prescribing is not simulating. Only the third of these approaches is "simulation".

We changed the wording from 'simulating' to 'determining'.

11584/27 - there are 6 models in this list, not 5

Yes, thanks. Fixed.

11585/19 - Is it realistic to prescibe a PFT cover in, say, the transient simulation or the sensitivity tests? Or is the cover updated somehow?

This would be a nice control for sure. However, some models are not set up to take in prescribed PFT cover, rather dynamically simulating the PFT cover during the simulation. Future WETCHIMPs could require this, if groups are given enough time and have the resources to adjust their models as required.

11585/21-11586/7 - Maybe give an experimental reference here for the relation of CH4 to NPP?

In this case, it is likely not required. We are not claiming that there is a relation between the two, we are simply listing which models do use a relation between the two.

11587/8 - "Spearman" Printer-friendly Version

Not sure what is meant here. We have capitalized Spearman if that was the suggestion

11587 - Why is the first reference to Fig 1 delayed (actually Fig 1d on page 11589, line 27)? K07 and GIEMS are mentioned much earlier.

Yes, thanks for noticing. We have moved the reference to the first mentions of GIEMS and K07.

11588/9 - What's "non-specific measurement of inundation"?

Sorry, this was ambiguous. We were intending to point out that these datasets might show areas of inundation, but no information is given about the depth of ponding. This has now been fixed in the MS.

11589/10 - What's PALSAR?

An acronymn that we should have spelt out the first time it was mentioned. Thanks, this has been fixed. (Phased-Array L-Band Synthetic Aperture Radar)

11590/14&15 - "has been shown" Where? Reference needed.

In Sanderson (2001). It was referenced in the previous sentence, but has now been added to the sentence after as well.

11591/6 - "possibly" - where is this suggested?

This comment relates to the dating of the observational datasets that make up K07. Some of these datasets possibly have wetlands that were mapped, or updated, during the period 1993-2004. Most of them have likely been mapped prior to that period.

11593/1 - "... orography." Reference needed?

Added. How SDGVM uses orography is described in Wania et al. (2012)

11594/6 - reference to CLM4Me publication?

Ok, added.

11595/16 - could a better description than "antiphase" be used?

It does describe the pattern between the two models. Other wordings would be longer and less precise.

11595/29 - Fig 4 is referred to before Fig 3 in the text.

Fixed

11596/1-3 - isn't this obvious, if CLM4Me and DLEM use GIEMS in the first place?

No, they use the dataset in quite different ways. CLM4Me uses GIEMS to parametrize their internal wetland detection scheme so that once a simulation is occurring, they do not use the external dataset. DLEM uses the GIEMS dataset to limit the maximal annual wetland extent, but does internally simulate the seasonal dynamics within the limits drawn from GIEMS. This is described in more detail in Wania et al. (2012)

11601/24 - Do Bloom et al (2010) have data for the HBL?

Yes, it is listed in Table 3.

11606/5&6 - Could be rewritten.

We have rewrote it to improve clarity.

11609/17 - models'

Thanks

11611/19-21 - why is there a soil temperature increase/decrease in the extratropics/tropics when only CO2 was changed?

The increase and decrease in soil temperature in the extratropics and tropics, respectively, occurred because of the impacts of CO2 concentration on stomatal conductance, leading to a changed surface energy balance, soil evaporation, and transpiration. These complexities are discussed in more detail in Subin et al.

(2012). This explanation and reference have been added to the MS.

11612/11 - How long did it take for equilibrium to be re-established in the models?

This was model dependent. Some models ran the model for the same length of time as their initial spin-up, other models ran for as short a time as 100 years. The threshold for determining the appropriate length of time was to have the model satisfy the same criteria for 'equilibrium' as in the spin up for Exp 1.

11612/13 - "in the limits they place on" would sound better

Ok

11613/13 - ARE the models' PFTs suffering from water stress then?

There is usually no variable in the models for 'water stress'. In our explanation, water stress is a deduced effect based on the response of other variables like soil moisture, NPP, phenology, etc. (differing depending on the model).

11617/7 - Could a better word than "equifinality" be used?

We adopted the same terminology as the referenced paper.

11618/21 - "common use" - example reference, perhaps?

Added

11618/24 - "state variables" - such as?

These could include NPP, labile carbon production, soil temperature, soil water content, water table position, soil depth, plant species present, $CH_4$ content in soil, $O_2$ content in soil, soil pH, etc. We have added some examples to the text.

Comments on Tables and Figures

Table 1 - Expand? See above.

We have added the main formulation for the methane emissions parametrization.

Table 2 - Could columns for the tropics and extratropics be added, such as in Table 3?

Yes, this has been done.

Table 3 - How are the standard deviations of the mean calculated? NB! The table footnotes do not match up!!!

This is already listed in the caption. Sorry, the table footnotes got jumbled during the typesetting process by the journal.

Table 4 - Link/refer to Fig. 8?

Yes, added

Figure 2 - the grey dashed lines are very hard to see.

Made darker

Figure 4 - Can this be enlarged?

This is up to the BG typesetters, but hopefully it will be larger in the final version

Figure 6 - "IAPRAS"

Thanks, space added to make it IAP RAS.

Figure 7 - Why not LPJ-WHyMe here? And it would be better NOT to change the scale on the y-axis! This would allow the reader to see the differences more clearly. It's hard to see now.

LPJ-WHyMe is not a global model. The differences we are trying to call attention to in this figure is not the magnitude of difference between each model, but the pattern, so a different scale for each is best.

Figure 8 - Refer to Table 4. Enlarge if possible. How are the area and CH4 normalized?

Enlargement is a typesetting issue. The normalization procedure has been added. All values are scaled to the maximum value (which is taken as 1) for each model across the 1993-2004 period for wetland area and methane emissions.

Figure 9 - Enlarge?
Figure 10 - Enlarge?

The size of both figures will be up to the BG typesetters. These will likely take up a full journal page so should be big enough.

Sincerely,

J. R. Melton, R. Wania, E. L. Hodson, B. Poulter, B. Ringeval, R. Spahni, T. Bohn, C. A. Avis, D. J. Beerling, G. Chen, A. V. Eliseev, S. N. Denisov, P. O. Hopcroft, D. P. Lettenmaier, W. J. Riley, J. S. Singarayer, Z. M. Subin, H. Tian, S. Zürcher, V. Brovkin, P. M. van Bodegom, T. Kleinen, Z. C. Yu, and J. O. Kaplan

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