**Biogeosciences
Discussions**

Interactive comment on "Present state of global wetland extent and wetland methane modelling: conclusions from a model intercomparison project (WETCHIMP)" *by* J. R. Melton et al.

Anonymous Referee #2

Received and published: 22 October 2012

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.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The findings are worrying in the sense that state-of-the-art models still show such wide disagreement in both simulated wetland area and CH4 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) CO2 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.

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?

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?

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

- 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.

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

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

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

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?

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

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?

Technical corrections (Page/line number) ————————————————-

11579/26 - +3.4 degrees C

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

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).

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

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

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

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

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?

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

11587/8 - "Spearman"

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.

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

11589/10 - What's PALSAR?

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

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

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

11594/6 - reference to CLM4Me publication?

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

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

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

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

11606/5&6 - Could be rewritten.

11609/17 - models'

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

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

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

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

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

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

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

Comments on Tables and Figures ———————————

Table 1 - Expand? See above.

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

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

Table 4 - Link/refer to Fig. 8?

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

Figure 4 - Can this be enlarged?

Figure 6 - "IAPRAS"

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.

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

Figure 9 - Enlarge?

Figure 10 - Enlarge?

Interactive comment on Biogeosciences Discuss., 9, 11577, 2012.