April 12, 2013

Geoscientific Model Development Discussions

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

Author Reply to Referee Steve Frolking:

Dear Editor,

This is our author reply to Steve Frolking's referee report for our paper, 'Present state of global wetland extent and wetland methane modelling: methodology of a model intercomparison project (WETCHIMP)'. We wish to thank Dr. Frolking for his time and care in providing comments on our manuscript. We will answer each comment below.

Our comments are presented in blue font. Dr. Frolking's original comments are in black.

Overall organization of the paper is very good, and the writing is clear and generally concise. The paper (along with Melton, Wania et al., which I have not seen) will provide the research and policy community with a clear picture of the state of global scale modeling of the wetland methane emissions, and is therefore a valuable contribution to the literature. The tables presented outline the WETCHIMP process and protocol well. Figs. 6 and 7 are an effective way of summarizing and differentiating between key modeling characteristics.

Thank you, we are glad to hear of the positive comments on our work.

A FEW GENERAL COMMENTS/QUESTIONS THAT NEED SOME CLARIFICATION.

Peatland vs. wetland vs. inundated area. These terms overlap but all have different meanings (and probably each of the terms can have more than one meaning, depending on the source). I suggest that you clearly define these three terms early on, and then make sure that they are used consistently in all the model descriptions (they may have been, but I wasn't sure).

We have added the following text: 'Following the definitions set out in Melton et al. (2013), we define wetlands for the purpose of large scale modelling as grid cells, or fractions thereof, where the land surface has inundated, or saturated, conditions. Peatlands are a form of wetlands characterized by fixed extents, at least on timescales of decades, and contrasting hydrologic and nutrient regimes between dry nutrient-poor bogs and wet nutrient-rich fens (Melton et al., 2013). Inundated areas are assumed to be wetlands (unless masked out with a rice agriculture or lake dataset) with the water table above, or at the soil surface, but do not include areas that are unsaturated at the soil surface but saturated at depth.'

How did models that used GIEMS wetland extent, available for 1993-2004, treat wetland extent in the other experiments (only Exp. 3 was for that period); e.g., Exp. 2 transient 1901-2009? Do they all do this in the same way, or does each model have it's own approach?

Experiment 2 had the same period of observations (1993 – 2004) embedded. Outside of those years, which were the only years of observations, we have added into each model's WETCHIMP set up section a description of how they deal with the inundated area outside of this time period for the models that actually use GIEMS (LPJ-WSL, LPJ-Bern, DLEM). The approach taken did differ between models.

I was uncertain how much rice paddies influenced differences in MPA and emissions. Based on Table 2, most global models had GIEMS input into MPA (all but SDGVM & UVic?), but in Table 4, only DLEM, LPJ-Bern, and LPJ-WSL mention applying the Leff rice area mask. GEIMS must include rice paddies, so was this a source of inter-model discrepancy? A brief discussion of this in Section 4.1 would be good. To the extent that rice paddy area was included (as paddy or as generic wetland), the discussion should include mention of the fact that in many inventories wetlands (natural) and rice paddies are listed separately.

The models with a direct input from GIEMS are DLEM, LPJ-WSL and the wetlands component of LPJ-Bern. Other models only used GIEMS to parametrize a model response that was then independent of GIEMS during the model simulations. For the actual influence of the rice regions, the models that directly used GIEMS all accounted for the rice influence. Regardless, this might be a source of inter-model discrepancies as models that simulate wetlands independently could be placing wetlands in regions that actually contain rice agriculture in reality. Models that independently simulate wetlands could also be placing wetlands in regions that have had significant alteration due to land use, wetland drainage, etc. Both of these possible sources of error have been added to the discussion in Section 4.1. as follows: 'An additional area of uncertainty that should be noted is the influence of anthropogenic changes to the land surface. Models that explicitly use the GIEMS dataset account for rice agriculture by masking out those regions (Table 3) while also implicitly including areas of human alteration such as wetland drainage, conversion to farmland, etc. Models that independently simulate wetland extent will not be sensitive to these alterations and this could lead to an overestimate of wetland area in these regions.'

Similarly for small lakes – some fraction of GIEMS is small lakes —- Prigent et al. (2007) put their uncertainty at roughly 10% of the pixel resolution of 773 km2, so these small lakes are only relatively small. How was this accounted for in the models (it is only mentioned explicitly in a few cases, one of which, LPJ-Bern, masks permanent water with another data set, Table 4)?

Yes, this is a possible source of uncertainty. The following was added to the paragraph inserted for the comment above: 'Small lakes could also contribute to an overestimated wetland area for some models. Presently only LPJ-Bern and UW-VIC mask these lakes (Table 3).'

Atmospheric oxidation – to me this means methane oxidation in the atmosphere (e.g., by OH radical); however, I think that you are referring to oxidation of dissolved methane in non-inundated soils, by methanogens, that leads to a diffusion gradient from atmosphere to soils, and results in net soil sink of methane. If that is what you mean, I think that you should use some terminology other than 'atmospheric oxidation'.

You are correct, this is confusing. We have changed this to read 'soil oxidation of atmospheric methane' for better clarity.

p. 4105-6: list of missing features: I suggest adding disturbance (e.g., draining for agroforestry); and management (e.g., water management in paddies; floodplain inundation management by dams and reservoirs). Also, doesn't the topography/TOPMODEL approach used by some of the models implicitly include lateral transport of water and groundwater dynamics?

Thanks for the suggestions, they have been added. For TOPMODEL, yes it makes assumptions about lateral transport - but they are implicit, as you note. TOPMODEL assumes a distribution of water table depth in the gridcell, contingent on a number of assumptions about stationarity of recharge, topographically-driven flow, etc. In other words, models using TOPMODEL are not actually simulating any lateral transport between gridcells, but rather assumes intra-gridcell lateral transport occurs to an extent that is necessary to make the water table distribution follow the TOPMODEL formulation. Explicit intra- or inter-gridcell lateral transport would allow deviations from the assumptions of TOPMODEL– as may often be the case in wetlands, where topographic gradients are slight, or floodplains, which could involve transport between several gridcells via a river network.

The list of missing features points predominantly to 'local issues' (nutrients, microtopography, vertical profiles of peat properties, ...). However, the largest differences are in MPA not flux (as noted in discussion). What are your recommendations for improving our ability to simulate MPA and evaluate these simulations? Maybe this is discussed in Melton et al., but an additional paragraph here would be a good addition.

Indeed, most uncertainties are related to MPA and not directly to flux. However, we should bear in mind that appropriately simulated methane producing area (MPA) is not directly equivalent to wetland area, but instead is a product of an effective simulation of hydrology (i.e. the creation of anoxic conditions) and carbon dynamics (to supply substrate to be reduced to CH_4). Therefore, we envision that uncertainties in MPA might be reduced by improvements related to improved understanding and quantification of both hydrology and carbon dynamics. Suggestions of improvement include (and are listed in the manuscript): (i) lateral transport of water and groundwater dynamics, and explicit treatments of floodplains and mangroves; (ii) microtopographical features such as lawns, hollows or hummocks; (iii) feedbacks between peat or carbon dynamics and thermal and hydrological processes in soil; (iv) hydrology affected by thawing permafrost; (v) wetland specific vegetation (improvements for boreal peatlands, introduction of tropical wetland PFTs) which will have influence on soil water; and (vii) anthropogenic disturbance (such as wetland drainage) and management (such as dams and reservoirs). While we agree that some of those suggestions are 'local issues' (such as point ii for example), the implications of small scale processes commonly lead to large scale changes (for e.g the coalescing of small amounts of run off eventually lead to large rivers and massive water transport to floodplains). In addition, our ability to adequately evaluate simulations of MPA is contingent upon appropriate observational datasets. We discuss the present problems with that issue in more detail in Melton et al. (2013).

As to scientific reproducibility – I can't see anyone reproducing 10 different models doing 6 different simulations; however all of the models have been described in detail in other publications, so it might be possible. Some of the recent modifications described here could use more detail (noted below), but there was enough detail provided in most cases.

SOME SPECIFIC COMMENTS:

Did Exp. 2 end in 2004 (Table 1) or 2009 (text p 4077, ln 6)?

It went into 2009. However since the satellite inundation product coverage ends in 2004, we have limited our comparison to 2004. We have corrected Table 1 to make this less confusing.

p. 4082, lines 3-4: excessive self-citation of DLEM papers; limit to only those relevant to methane.

Yes, removed excess.

p. 4082 line 26: 'transport' rather than 'transportation'

Done.

p. 4083, lines 6-11: DLEM improvements through '... coupling of TOPMODEL and other models ...' What other models? subsequent list of improvements (more soil layers, fraction vegetation structure (what is that?) and river routing), don't seem to be linked to 'TOPMODEL and other models'.

We have revised the text extensively, please see the revised MS section 3.2

p. 4083: Yang et al. (2012) is listed as 'in prep.' I don't think that this qualifies as a valid citation.

Removed citation

The last paragraph of 3.4.1 (LPJ-Bern setup; top of p. 4087) is much more a discussion of results than set-up. It doesn't belong in section 3.4.1, but might not belong in the results section either, as its level of detail/specificity is beyond what is reported for the other models.

This particular paragraph is referenced to in a sister publication and we would like to keep it in place as is.

p. 4087, line 14: 'led' rather than 'lead'

Corrected.

Eq. 3: should that be sigma(x) rather than sigma(t)?

Yes, thanks for picking that out. It has been corrected.

p. 4093, line 10: typo: ‘is the is the’

Corrected

p. 4094, lines 23-25: is the orographic correction described in SDGVM publications? If so, which one(s)? If not, please provide more detail here.

No, it does not appear in other publications. More detail has been added: ‘ The orographic correction, F_{corr} is computed as:

$$F_{corr} = \frac{0.01 - S_{max}}{0.01} \tag{1}$$

where S_{max} is the maximum gradient between a grid-cell and its 8 nearest neighbours.’

p. 4096, line 9: provide citation for TRIFFID dynamic vegetation model.

Added.

p. 4097, line 2-3: is there a citation for the lake emission rate of 375 mg/m2/d? or is that the default UW-VIC value from Bohn et al. 2007 or B&L 2010?

Thank you for noting this. There is no citation for this lake flux rate. The UW-VIC results that used this flux rate were part of an ensemble of simulations (not part of WETCHIMP) using various lake flux rates ranging from 0 to 500 mg/m2/d. This value was recently determined to be in error (caused by an artefact in the satellite data used for its derivation). A new value is now used of 0 mg/m2/d, which is more in-line with local measurements. As a result of this the UW-VIC results have been replaced with ones using this new value (although we make both versions available for anybody who wishes to download them). This impacts upon the wetland emissions in Figure 8j. We have also changed the text as follows: ‘ A lake emission rate of 375 mg $CH_4\, m^{-2}\, d^{-1}$ was originally assumed during the ice-free season and half that rate during ice-covered season. This rate was found to be in error due to an artefact in the satellite data used to parameterize the lake emission rates. A value of 0 mg $CH_4\, m^{-2}\, d^{-1}$ is now used. The new lake emission rate results in lake CH_4 emissions that more closely resemble observations from the area. The influence of the new value can be observed by comparing Melton et al. (2013) Appendix Figure A1 panel j (375 mg $CH_4\, m^{-2}\, d^{-1}$) and Fig. 8j (0 mg $CH_4\, m^{-2}\, d^{-1}$).’

Eq. 6: is WTP measured positive down from the ‘soil’ surface? Does methane production increase with increasing water table depth, up to 10 cm? Please clarify.

The WTP is measured positive upwards from the soil surface in SDGVM and emissions increase with increasing water table depth. This has been clarified in the MS.

Fig. 1: caption should explain what ‘white’ areas are (presumably zero, but just to be sure).

Yes, zero. We have added to the figure caption to make that explicit.

Sincerely,

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

Melton, J. R., Wania, R., Hodson, E. L., Poulter, B., Ringeval, B., Spahni, R., Bohn, T., Avis, C. A., Beerling, D. J., Chen, G., Eliseev, A. V., Denisov, S. N., Hopcroft, P. O., Lettenmaier, D. P., Riley, W. J., Singarayer, J. S., Subin, Z. M., Tian, H., Zürcher, S., Brovkin, V., van Bodegom, P. M., Kleinen, T., Yu, Z. C., and Kaplan, J. O.: Present state of global wetland extent and wetland methane modelling: conclusions from a model intercomparison project (WETCHIMP), Biogeosciences, 10, 753–788,