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Geoscientific Model Development Discussions

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

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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.

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).

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?

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.

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)?

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’.

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?

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.

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)?

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

p. 4082 line 26: ‘transport’ rather than ‘transportation’

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’.

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

p. 4087, line 14: ‘led’ rather than ‘lead’

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

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

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.

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

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?

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.

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

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