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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Magnitude and seasonality of wetland methane emissions from the Hudson Bay Lowlands (Canada)" *by* C. A. Pickett-Heaps et al.

Anonymous Referee #1

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This paper by C. Pickett et al. presents an analysis of the methane emissions from the Hudson Bay lowlands based on 1/ aircraft and surface continuous observations of atmospheric methane and 2/ chemical-transport model simulations forced by a simple wetland model.

The material of this paper is original and interesting. The reading is fluid, the scientific question of estimating the methane emissions from Hudson bay lowlands clearly posed, and the structure clear. I suggest publication in ACP after addressing the following issues:

General comments

1/ Wetland model description. This part should be more detailed. For instance, as a guide : - Why developing a simple model and not using recent more complete models (e.g. LPJ-Sphanni et al 2010, ORCHIDEE-Ringeval et al 2010, J. Kaplan 2002, ...). Did the authors perform CTM runs with other distributions than their simple model to compare? How their model differs from Kaplan 2002? - The authors mention comparison with observed ecosystem fluxes. Which sites? How does it compare? - Why four carbon pools? Which model has been used for modeling respiration?

2/ Phasing of the seasonal variations. GEOS-chem, forced with the wetland model, produces a maximum at FRD end of June and a secondary maximum end of august (fig 3). This double peak structure differs from the observations (max end of august). This is not clearly discussed in the paper. This is only mentioned line 9 of page 22423 briefly. It should be clearly mentioned and commented when discussing fig3. This double peak is also visible after subtraction of ALT on fig 4. Do the author have an explanation? Are these variations visible in the wetland flux ? Are there transport feature that could explain them? In general, the authors are too optimistic on the agreement model/observations (see also specific comments) Modeled FRD also diverges from modeled ALT from end of march (instead of end of mid may for observations). Any explanation? Could the author add the curve from the model run with snowfree assumption on fig 3 as on fig 4 (maybe as a dotted line for clarity), as it should show that part of this difference is removed with this hypothesis? When subtracting ALT from FRD the authors assume that model phasing lags go in the same direction at ALT and FRD. This is an important assumption because if it is not the case, taking the difference would not suppress model phasing error but may emphasize it ! The black curve on fig 3 shows that this is partly true as modeled FRD and ALT are close in phase, although with a $\frac{1}{2}$ month lag. Finally, the magnitude of the lags reported are often a bit optimistic compared to the plots. Please be more consistent with the plots (see specific comments)

3/ Magnitude of the emissions. The authors should be more precise in section 4. What

ACPD

10, C11816–C11818, 2010

are the values found in the two regions (ABLES and NOEW) in g/m2/a? What are the values found in their wetland models for the same regions (also in g/m2/a). Please provide the precise numbers.

Specific comments

P22420 – line 10 : A temperature of T0=-227K seems a bit strange

P22421 – line 21 : the threshold of 200ppb is arbitrary or based on data analysis ? Please precise.

P22422 – line 9-11 : The procedure for plotting figure 3 should be clarified. As I understand, you built figure 3 as a climatology over 2004-2008 by selecting air masses blowing from ALT to FRD (North west winds) ? What percentage of observations does it represent? Is it biased seasonnaly ? More precisions are required here.

P22422 – line 14 : the lag seems to be closer to 1.5 years from fig 3

P22422 – line 17 : "the model show the same . . . but shifted one month early". This sentence if a bit optimistic. Lag is more 1.5 months again and I would not say that the model shows the same variations as a double peak appear not present in the observations (see also general comments). Please rewrite this part to be closer to what the fig 3 shows.

P22423 – line 7 : "maximum in June-August". Again the authors do not mention the double-peak structure. They are a bit too optimistic on the agreement with observations. Please rephrase.

P22424 – line 3 : put also the snowfree plot on figure 3

P22425 – line 22-23 : the maximum is more August than june-july in the obs at FRD. Please be more precise.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22415, 2010.