ACPD Article: acpd-10-22415-2010

Magnitude and seasonality of wetland methane emissions from the Hudson Bay Lowlands (Canada)

RESPONSES TO REFEREES' COMMENTS (comments are in red, responses in black)

Anonymous Referee #1

Received and published: 31 December 2010

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
C11816

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?

Our model is actually based on Kaplan 2002. We now say so. We do not mention comparison with observed ecosystem fluxes. We now reference the LPJ model for the use of the carbon pools and are more precise about the nature of these pools.

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 authors have an explanation? Are these variations visible in the wetland flux? Are there transport feature that could explain them?

We now mention this multi-peak structure in the model in Figure 3. From the standpoint of containing HBL emissions, what matters is Figure 4, and we now point out that this multi-peak structure is reduced when the effect of background (Alert) is subtracted. We believe that any residual multi-peak structure is largely due to changes in the background methane concentration at Fraserdale relative to Alert. The residual multi-peak structure is not a result of HBL flux variations within the model. The reviewer points out that there is still more fine temporal structure in the model than in the observations in Figure 4, and we now hypothesize that this could be due to greater inertia of soil temperatures relative to surface temperatures.

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?

We point out that this is due to model onset of HBL emissions in April. We then delay this model onset by using continued snow cover as an additional constraint. This improves the consistency in the model onset in emissions with the observed onset at Fraserdale.

Could the author add the curve from the model run with snow-free 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?

That information is already in Figure 4. We think that Figure 3 is complicated enough as it is and that the information is more clearly presented in Figure 4.

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 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)

We now point out that the similarity between the Alert and no-HBL Fraserdale model curves in Figure 3 support the assumption of using Alert as background for Fraserdale. We are also now more specific with the time lags in the plots.

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

We have provided more specific flux estimates for the two study regions of the NOWES field campaign and ABLE-3B airborne campaign. Specific flux estimates from field sites and aircraft measurements are provided. We also provide comparable model flux estimates for the two regions.

Specific comments

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

Typo corrected.

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

Please precise.

Added.

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 seasonally? More precisions are required here.

We have added additional text that more precisely describes our methodology in generating the climatologies depicted in figures 3 and 4.

P22422 – line 14: the lag seems to be closer to 1.5 years from fig 3.
Corrected to 4-6 weeks.

P22422 – line 17: "the model shows the same . . . but shifted one month early". This sentence is 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.

Rewritten,

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.

Done.

P22424 – line 3: put also the snow-free plot on figure 3

We would rather not – see response above.

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

Done.

Anonymous Referee #2
Received and published: 2 January 2011
This paper uses a combination of model simulations and upwind and downwind methane observations to further constrain methane emissions from the Hudson Bay Lowlands (HBL). Overall the paper should be publishable following some revisions to address the comments given below.

Major Comment: Because the GEOS-Chem model does not simulate the observed July minimum at the generally upwind station at Alert, then the model-observation differences at the generally downwind station at Fraserdale are presumably due to a combination of HBL emissions and the aforementioned upwind differences (Figure 3).

The authors therefore appeal to the Fraserdale-Alert difference (Figure 4) to assess the validity of their modeled HBL emissions. A case is apparently made for the GEOS-Chem snow-free results being a reasonable simulation of these differences. However there remain some significant model-observation differences in Figure 4 that require at least some further explanation; in particular the reasons for the 3 peaks in the model (June, Sept., Nov.) versus the single peak (July-Aug.) in the observations need to be better addressed.

The 3-peak structure in Figure 3 is reduced when corrected for the Alert background in Figure 4. We believe that any residual multi-peak structure is due to variations in the methane background concentration at Fraserdale vs. the background at Alert. The multi-peak structure is not associated with similar variations in the HBL model emissions. There is still more structure than in the observations and we now hypothesize that this reflects lower variability of soil temperatures.

Minor Comments: Pg. 22417-Line 15 and 22426-Line 3: What previous estimates are being referred to here? The authors estimate is larger than some previous assessments but seems comparable to others. Be explicit about these previous estimates.

Done.

Pg. 22420-Lines 6-25: For clarity, please define the terms in equation 1 in the order in which they occur (W, etc.). Also why not include A in the exponential? And why is To negative? Pg. 22424-Line 26: How is the 0.3 Tg/yr error in the emission estimate derived? It should be related to the model-observation differences (e.g. in Figure 4) and not just to model results.

From an operator standpoint it seems to us more logical to define the terms in the order that they are applied (from right to left). We did not include *A* in the exponential in order to facilitate traceability to Lloyd and Taylor (1994).

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General
This is an important contribution and should be published.
The paper quantifies the methane output of a major northern wetland, and addresses the vexed question of the timing of emissions – whether they are dominantly in the warmest time of year, and the relative roles of the shoulder seasons.

Hitherto, estimates of methane emissions from this wetland have been surprisingly low: this work suggests the emissions from the Hudson Bay lowlands are actually significantly larger, which is better in accord with wider Arctic studies and global models.

Specific

Page 22421 lower: Justify the comment that fire plumes are a minor source of methane: some isotopic evidence suggests that there is a significant input of 13C heavy methane reaching Europe from Canada.

We removed ‘minor’

Page 22423: Any model that suggests emissions in early April must have problems! The ground and the water are frozen. In my experience in muskeg further west, emission bursts from stored methane in ponds under melting ice cover occur in May.

Page 22424: Note that local surface temperatures can be significantly different from meteorological temperatures. Peat hummocks can create and sustain marked microclimates (nano-climates?) that are much warmer than the surrounding air. This stabilization of temperature of methanogenesis, decoupling from ambient air temperature, could in part account for the discrepancies between model and observations in Fig 4, where the blue line is much steadier than the jagged double-peak red lines.

Thank you for the suggestion. We have adopted it.

Summary

A good paper – should be published with minor revision.