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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Annual variation of methane emissions from forested bogs in West Siberia (2005–2009): a case of high CH_4 and precipitation rate in the summer of 2007" *by* M. Sasakawa et al.

Anonymous Referee #2

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This paper by Sasakawa et al. present estimations of methane emissions around two towers located in western Siberia performing atmospheric methane measurements for the period 2005-2009, and compare these estimates with the fluxes calculated with the process-based model for wetland emissions (VISIT). This paper follows Sasakawa et al paper in Tellus presenting the methane atmospheric observations performed at the two towers. The material of this paper is original and very interesting. As mentioned by the authors, any observations made in Siberia for methane are highly valuable as there are only very few of them especially on a regular and long-term basis. The monitoring

made by the authors is very important for the methane cycle. However, the paper needs important improvements on the analyses performed, on the level of details and explanations given, and on the fluidity of the writing.

General comments ————————-

1/ On the flux estimate method: Errors in FCO2 flux location and magnitude impact directly your FCH4 estimate. Errors on FCO2 are also a possible explanation of the mismatches of the results with the GISS inventory. Did you try another neutral biosphere distribution? One hypothesis used when applying the ΔCH4/ΔCO2 ratio is that sources of CH4 and CO2 are co-located in Western Siberia. Is it at least partly true? This assumption is important but not mentioned in the text.

2/ The comparison between KRS and DEN could be further developed. How the environment of these sites differ ? Can it explain some discrepancies between the different approaches (GISS, VISIT, this work)?

3/ The authors study the sensitivity of their results to precipitation with a low and high scenario to conclude that precipitation is the leading factor of the CH4 anomaly around KRS in summer 2007. But there is also a sensitivity to temperature, at least for flux density ? Is it much smaller ? Why ? Temperature dependencies should also be addressed (or clarified) in work if one wants to conclude that precipitation drives.

4/ There are only few comments on the other years than 2007. It would be interesting to develop a bit the role of western Siberia (as seen from the 2 sites) for the methane anomaly of 2008 as it is still discussed whether high latitude ecosystem play a role in 2008 or not.

5/ The text of the paper lacks precisions and explanations making the reading difficult and not fluid (see specific comments). This is sometimes limiting the comprehension of the work performed. The description and implication of the assumptions have to be more detailed. The description of models has to be clarified and precised. I am not a

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native English speaker but it seems to me that several sentences/paragraphs should be re-written. Please use “present” for verbs.

Specific comments ———————— P27760, lines 6-11 : “Although . . . base)” : this sentence is not in proper English. Please rephrase.

P27760, line 23 : I propose : “and its role in the photochemistry of the atmosphere”

P27762, line 26 : Why only keeping 3 minutes per 20 minutes ? 17’ flushing ? Please clarify this point.

P27763 lines 5-14 : the paragraph explaining how the neutral biospheric does not provide enough information to understand what the authors did or used. Please say a few words about the procedure used by Olson and Randerson. Line 12, the sentence “The respiration was then rescaled . . . NEP” is completely unclear.

P27763, line 21 : “efflux” should be flux

P27763-27764 : ecosystem model. Again the explanations given there must be clarified, more precise and more detailed

- The authors mention that CH4 fluxes are estimated separately for flooded and non flooded areas but they do not say clearly what is done for non flooded areas. - What is “unrealistic inundation fraction” (line 4) ? How are they filtered out ? - Water table depth of 0 and -25 cm are arbitrary ? - Line 9 : grid cell ? - The relation with temperature is not mentioned. Please add some text. - The low and high response cases are totally unclear at this stage. They used in the results so the authors have to introduce them more in detail here. Is it that in the high case 1 mm precipitation anomaly induces +1 mm water table depth ?

P27764, line 24 : ARE instead of IS

P27765, line 14 : I suggest : “generally represent”

P27765, line 21 : What are “consecutive three nocturnal fluxes” ? I suggest “Three-

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hourly averaged nocturnal fluxes from 20 LST . . .

P277666, line 1 : SHOW

P277666, line 1-7 : the ratio seems also high in July 2008 at KRS. Comments? More generally, you do not comment much the other years and DEN ? Figure 3 is not easy to analyse as dots are small. You may enlarge them and use different symbols to better separate the different years.

P 27766, line 10 : I suggest : "the three-hourly averaged nighttime data from 20:00 . . .."

P27766, line 17 : June 2007 is also higher on figure 4 although more spready. Please comment.

P27766 : line 18-20 : do you have an explanation for the outlier data in 2005 ?

P27767, line 3-4 : What are the uncertainties of your approach ? see also general comments

P27767 : please use only mg/m2/day as a unit in the text and for figure 4.

P27768, lines 5-15: is the lag observed between precipitation rates and CH4 flux maxima consistent with the time for bacterial activity to develop ? Please comment more on this lag.

P27768, lines 26: I suggest : "CO2 and CH4 accumulate in the lower. . ." Legend of figure 4 : "GISS wetlands" should be replaced by wetland methane emissions from the GISS model.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 27759, 2010.