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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.

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Comment: 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 bio-sphere distribution?

Reply: CASA has been widely used to calculate biosphere CO2 flux. For example, an international collaborative activity for carbon cycle transport model intercomparison

(TransCom) has used output from CASA as providing a relatively reliable estimate of biospheric flux (e.g. Law et al., TransCom model simulations of hourly atmospheric CO2: Experimental overview and diurnal cycle results for 2002. Global Biogeochem. Cy. 22, doi:10.1029/2007GB003050, 2008). In our study we employed a similar model simulation procedure as in various TransCom exercises, employing CASA.

Comment: 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.

Reply: We have added the following sentences in the section 3.1; "There was no dominant wind direction and no particular difference in wind direction between day and night, allowing for an assumption of relatively homogeneously distributed sources of CO2 and CH4 over the tower footprint; that is, sources of these gases were essentially co-located."

Comment: 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)?

Reply: The environment of these sites is similar; they are placed in the middle of the taiga and surrounded by extensive bogs. The bog around DEM seems to be denser, which is already regarded as wetland in GISS and VISIT. As written in the section 3.2, many small ponds and lakes are distributed throughout the taiga and extensive bogs surrounding the KRS tower; these small water bodies can act as a substantial source of CH4 particularly during summer. We explained the difference between calculated flux and GISS with the emission from these small water bodies.

Comment: 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 clariïňĄed) in work if one wants to conclude that precipitation drives.

Reply: The present simulation took account of temperature variability on the basis of NCEP/NCAR reanalysis. We have added the simulation results without considering precipitation anomaly, which does not show large anomaly in the summer 2007. We checked the time-series of temperature anomaly during the experimental period, and found that it was not anomalously warm through the summer of 2007 around the towers. As referred in the section 3.3, Bohn et al. (2007) found that higher temperatures alone did not always increase CH4 emissions from wetlands but higher precipitation alone raised water tables and expanded the saturated area, resulting in a net increase in CH4 emissions. Although CH4 production is sensitive to temperature, as seen in seasonal variability, we are sure that the CH4 flux anomaly in 2007 summer is not attributable to temperature anomaly.

Comment: 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.

Reply: No anomalous CH4 emission was observed in 2008 from the forested bog regions. We have modified the conclusions and mentioned this.

Comment: 5/ The text of the paper lacks precisions and explanations making the reading difïňĄcult and not ïňĆuid (see speciïňĄc 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 clariïňĄed and precised. I am not a native English speaker but it seems to me that several sentences/paragraphs should be re-written. Please use "present" for verbs.

Reply: As shown in acknowledgment, an English native scientist rechecked English. We modified the description for VISIT model as follows; "2.3 Ecosystem model Monthly

CH4 fluxes of wetlands were estimated with a process-based ecosystem model, VISIT (Inatomi et al., 2010; Ito, 2010), to evaluate the variation of gas fluxes responding to weather and biological conditions. Fig. 1 shows a schematic diagram of the CH4 exchange scheme employed in VISIT. The model consists of carbon, nitrogen, and water cycle sub-schemes, each of which is composed of several functional compartments such as leaves, stems, roots, dead biomass, and organic soil. Plant photosynthetic CO2 uptake, allocation, biomass growth, and mortality are simulated in the carbon cycle scheme in an ecophysiological manner (Ito and Oikawa, 2002). Wetland CH4 flux is simulated using a semi-mechanistic scheme (Walter and Heimann, 2000), in which three processes of CH4 emission flux are considered: physical diffusion, plant-mediated transportation, and ebullition. The physical diffusion rate depends on the CH4 concentration gradient between the surface and soil air, which is affected by CH4 production and oxidation within the soil. In the soil, the CH4 production rate is determined by microbial activity and substrate supply from plants, producing sensitivity to temperature variability that leads clearly to seasonal cycle in the CH4 emission. Spatial heterogeneity in diffusivity through soil pore spaces is considered on the basis of sand/clay composition data (Hall et al., 2006) and water table depth. The plant-mediated transport of CH4 is dependent on the plant growing stage determined by the cumulative temperature and biome-specific rooting depth (typically, 20 cm for wetlands). The ebullition flux occurs only when the CH4 concentration exceeds 500 μmol liter–1 (Walter and Heimann, 2000). Wetland distribution is determined on a 0.5° $\times$ 0.5° grid based on Global Lakes and Wetland Database (GLWD, Lehner and Döll, 2004) (Fig. 2), and a distribution of natural vegetation type including both uplands and wetlands is derived from the global data set (Olson et al., 1983; Ramankutty and Foley, 1999). For performing broad-scale simulations, wetland soils are stratified into 20 layers of 5 cm thickness each. To include the spatial heterogeneity of wetlands, CH4 fluxes are separately estimated for flooded (i.e., inundation) and non-flooded (i.e., drainage) fractions of the ground surface, each of which has different water table depths. Thus, the total CH4 emission (E) for each grid cell is obtained as: E =

$w \times$ (finund $\times$ Einund + fdrain $\times$ Edrain) (2) where w represents the wetland fraction in each grid cell, and f and E denote the land fraction and CH4 exchange flux of inundation and drainage parts (subscripts), respectively. Monthly average inundation fraction (finund) is derived from the SSM/I observation for 1993–2000 (e.g., Prigent et al., 2007). Because we estimate the inundation fraction on the basis of seasonal variation for each grid cell, in some cases, snow cover and extensive floods after snow melting could affect the base line. To avoid these apparent variations (e.g., too much severe drying after a spring flood) during the growing-period (May–August), we have decided to use the average inundation fraction derived from the SSM/I observation during the period. The baseline water table depths of the inundation and drainage wetland surfaces are assumed as 0 and -25 cm, respectively, on the basis of an observation at West Siberian wetlands (Bohn et al., 2007). At layers lower than the water table, CH4 production is estimated as a function of temperature and plant carbon supply, which is obtained from the vegetation production scheme of the model. We also evaluated the influence of precipitation rate on the CH4 emission from wetlands. Inter-annual variability in the water table depth was estimated from the cumulative precipitation anomaly at each model grid as deviation from the 2001–2009 mean obtained from the reanalysis data of the NCEP/NCAR (Kalnay et al., 1996). To assess the possible range of estimation, a high (+1mm water table depth/+1mm precipitation anomaly) and a low (similarly, +0.2 mm/+1 mm) response cases are conventionally examined. To validate the CH4 flux estimated by VISIT with widely used CH4 flux distribution map from the wetlands (bogs, swamps, and tundra), we used the climatological data published by the NASA Goddard Institute for Space Studies (GISS) (Fung et al., 1991)."

Comment: P27760, lines 6-11 : "Although . . . base)" : this sentence is not in proper English. Please rephrase.

Reply: As replied already, an English native scientist rechecked English.

Comment: P27760, line 23 : I propose : "and its role in the photochemistry of the atmosphere"

Reply: We modified it based on this suggestion.

Comment: P27762, line 26 : Why only keeping 3 minutes per 20 minutes ? 17' ïňĆushing? Please clarify this point.

Reply: Yes, it is for flushing and to stabilize the sensor. We added "(flushing)".

Comment: 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.

Reply: We modified and added details of the method.

Comment: P27763, line 21 : "efïňĆux" should be ïňĆux

Reply: We have replaced efflux with flux.

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

Reply: We modified and added details of the method. Further detail of the VISIT model itself is described in the referred manuscripts (Inatomi et al., 2010; Ito, 2010).

Comment: - The authors mention that CH4 ïňĆuxes are estimated separately for ïňĆooded and non ïňĆooded areas but they do not say clearly what is done for non ïňĆooded areas. - What is "unrealistic inundation fraction" (line 4) ? How are they ïňĄltered out ?

Reply: Methane fluxes at flooded and non-flooded areas were evaluated using the same process scheme, but assuming different water table depths. Namely, non-flooded area, top soils (0–25cm) were assumed to be aerobic, while lower soils were assumed to be anaerobic both in flooded and non-flooded areas. We have modified the section 2.3.

Comment: - Water table depth of 0 and -25 cm are arbitrary ?

Reply: Although the measurement of water table depth for wetlands in West Siberia is scarce, Bohn et al. (2007) reported that the deepest depth was approximately –25 cm. According to this manuscript, we determined it as –25 cm as the deepest value. We have added the reference in the section 2.3.

Comment: - Line 9 : grid cell ?

Reply: Yes, we mean grid cell.

Comment: - 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.

Reply: In the CH4 evaluation scheme used in VISIT, CH4 production rate is highly sensitive to temperature change, such that warming by 10 deg-C results in six-time higher specific CH4 production (Walter and Heimann, 2000). Also, in the northern wetlands, higher temperature can result in enhanced plant growth and carbohydrate input to soil. This effect was included on the basis of monthly net primary production estimated by VISIT.

Comment: Is it that in the high case 1 mm precipitation anomaly induces +1 mm water table depth?

Reply: Yes. In the high case, we assumed that 1 mm precipitation anomaly results in 1 mm water table depth anomaly, irrespective of the initial position of water table.

Comment: P27764, line 24 : ARE instead of IS

Reply: We replaced “is” with “are”.

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

Reply: We added “generally”.

Comment: P27765, line 21 : What are “consecutive three nocturnal ïňĆuxes” ? I suggest “Three-hourly averaged nocturnal ïňĆuxes from 20 LST . .

Reply: We believe the original expression is proper. Consecutive three nocturnal fluxes indicate the three 3-hourly data for 22:00, 1:00, and 4:00.

Comment: P277666, line 1 : SHOW

Reply: We modified it.

Comment: 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.

Reply: We can say that the ratios in July 2007 were relatively higher than those in other years, but it is difficult to say that the ratios in July 2008 were also higher because the numbers of calculated day for July 2005, 2006, and 2009 are small. We modified Figure 3 (It is new Figure 4; Relationship between ΔCO2 and ΔCH4 during summer at KRS (upper panels) and DEM (lower panels). ΔCO2 and ΔCH4 are defined as the measured concentration difference between the concentration at 21:30 LST and the accumulated concentration in the early morning the next day at 4:30 LST. Dotted lines indicate expected relationship from environment in CO2-flux:CH4-flux = 100:1, 200:1, and 400:1.).

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

Reply: We believe the original expression is proper.

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

Reply: We added “June”.

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

Reply: We do not know why this outlier occurred.

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

Reply: We have shown only “mg/m2/day”.

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

Reply: Anomalously high CH4 emission already occurred in June 2007. We have modified the expression. We also have added the following sentence in the section 3.3; “The slight time lag between the time of high precipitation rate and the time of increased CH4 emission is likely due to the poor drainage in the West Siberian wetlands (relatively flat terrain) and low evapotranspiration rate.”

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

Reply: We modified with your suggestion of “wetland methane emissions from the GISS model”.

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

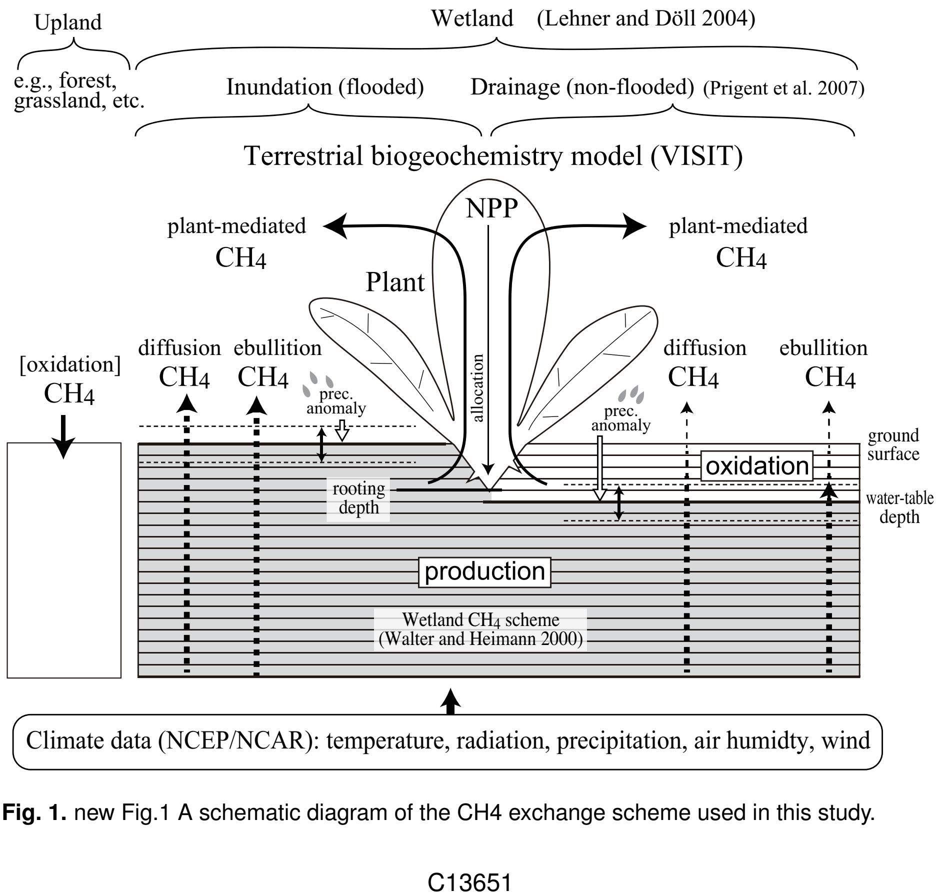

Fig. 1. new Fig.1 A schematic diagram of the CH4 exchange scheme used in this study.

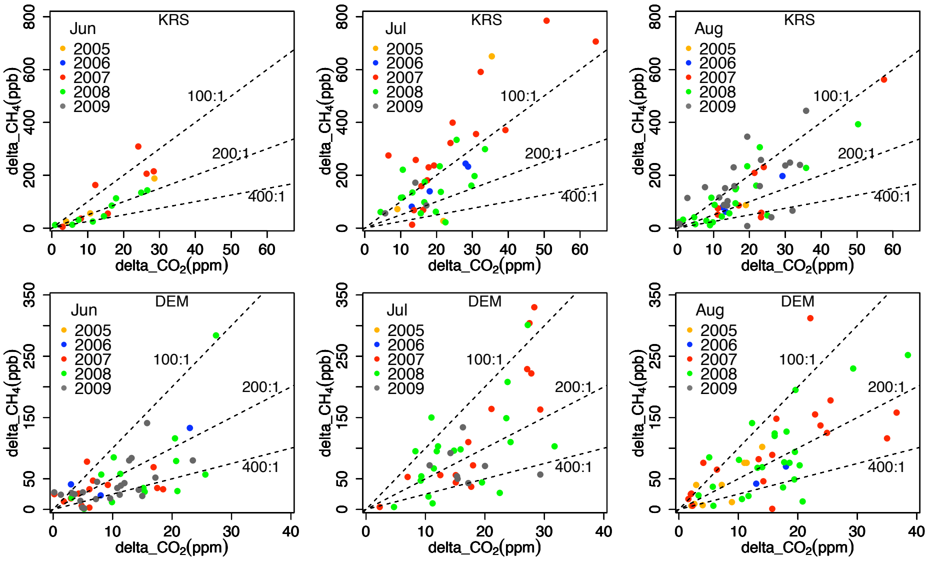

Fig. 2. new Fig. 4 Relationship between ΔCO2 and ΔCH4 during summer at KRS (upper panels) and DEM (lower panels).