

Interactive comment on “Annual variation of methane emissions from forested bogs in West Siberia (2005–2009): a case of high CH₄ and precipitation rate in the summer of 2007” by M. Sasakawa et al.

Anonymous Referee #5

Received and published: 15 February 2011

GENERAL COMMENTS:

I.) This manuscript focuses on the inter-annual variability of CH₄ emissions from forested bogs in West-Siberia. Methane emissions are estimated by a combination of observations of CH₄ and CO₂ concentrations in the nocturnal atmospheric boundary layer and nighttime CO₂ fluxes estimated by the terrestrial biosphere model CASA. West-Siberia is a highly relevant region for the global budget of atmospheric CH₄ due to its vast areas of potentially CH₄-emitting wetlands and peatlands. Since measurement data from this interesting region is still very limited, the observational data presented

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by this study is of great interest for the scientific community. The goal of the manuscript fits well into the general scope of Atmospheric Chemistry and Physics.

II.) However, the presented approach for estimating the CH₄ fluxes from the ratio of the nighttime CH₄ accumulation and the nighttime CO₂ accumulation and CO₂ fluxes from a rather general terrestrial biosphere model like CASA appears for me questionable. This is due to the following main problems: 1.) The CASA model was originally not designed to model wetland and peatland CO₂ fluxes. Thus, how representative are the modelled CO₂ fluxes for the real fluxes if the area of interest has such a large coverage of wetlands and peatlands? Were special parameterisations for areas with a large wetland coverage used? Generally, a big problem of this study is that the description of the CASA modelling (Which input data, parameterisations?) is far from being sufficient. And the few sentences which are given makes me rather skeptical: Why do you assume “zero annual mean (CO₂) flux” for an area dominated by peatlands (page 27763, lines 12–14)? Peatlands are known to accumulate carbon! 2.) When combining gridded data of CO₂ fluxes derived from a biosphere model with point measurements of CO₂ and CH₄ concentrations, it would be necessary to provide footprint analyses of the concentration measurements. What is the size of the footprint of these measurements compared to the model grid? Do the scales of the CASA model results and the measurements fit to each other? 3.) In wetlands, the water table is an important control on the CH₄ emission, but it can be expected that it also affects the aerobic soil respiration and consequently the nocturnal CO₂ emission. A smaller $\Delta(\text{CH}_4)/\Delta(\text{CO}_2)$ ratio at a given observation time can be explained either by higher CH₄ emissions or by lower CO₂ emissions or both. Can you exclude reduced CO₂ emissions under the high precipitation situation in summer 2007? 4.) Generally, it appears problematic that no CO₂ and CH₄ flux measurements are available to validate the model-based flux estimates. Now, the magnitude of the estimates of the CO₂ fluxes and consequently also of the CH₄ fluxes are completely dependent on the correctness of the CASA model output which is not validated for the area of interest.

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All the assumptions and the problems due to potential violation of these assumptions should be stated more clearly and discussed thoroughly in the paper. Furthermore, the measurements of CH₄ and CO₂ concentrations have uncertainties due to measurement errors and to the mismatch of their footprint to the model grid cell area. Also the CO₂ fluxes modelled by CASA have uncertainties. These uncertainties should be propagated when calculating the CH₄ flux estimates and be shown and discussed.

III.) Similar to my critique regarding the CASA modelling, I agree with reviewer #4 that the CH₄ modelling with VISIT should be explained and discussed in much more detail. It does not become clear how much the model resembles the Walter and Heimann approach and how much it deviates from it. More information on input data and parameterisation is needed. Like reviewer #4, I am skeptical about the approach of driving the VISIT model with the estimated inundation fraction. How this important input data was exactly derived does not become clear enough. And the assumptions that the water table is zero for flooded and -25 cm for nonflooded situations needs some justification. CH₄ emission is controlled in a highly nonlinear way. E.g., water tables significantly above the soil surface can lead to reduced CH₄ emissions. As no validation data is available to constrain the model results, it is all the more important to explain the modelling in detail and to state and discuss the underlying assumptions and uncertainties of the model results.

IV.) The manuscript would benefit from structural changes and in grammatical improvements. There are many occasions where articles “the” or “a” are omitted where they would be needed and some occasions where these articles were placed where they actually do not fit. There are too many sentences in which the writing style is suboptimal leading to difficulties to understand the message. It might be good to have a native speaker checking the grammar, orthography and style before re-submission.

The abstract should be rewritten. It should have an introductory sentence which introduces the general topic. Then, you should give concise information about the investigation site and the methods. Then, present the most important results, and finally provide

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also the main conclusions from your study. And please no unexplained acronyms in the abstract (CASA). The introduction gives useful information on the CH₄ concentration changes in the atmosphere. On the other hand, it does not reference to the CH₄ flux studies which have been already conducted in Siberia. Please check for the work of Glagolev et al., van Huissteden et al., Corradi, Merbold et al., Wille, Kutzbach, Sachs et al., Walter, Zimov et al.. Also the discussion lacks such references. The methods section is in many regards not detailed enough: Some more information on the CH₄ semiconductor sensor (measurement principle, precision) would be needed. Were the gas concentrations corrected for the drying effect? There is not enough information on the input data and parameterisations of the CASA modelling. The estimation procedure of the flooded/non-flooded areas for the VISIT model is not described in sufficient clearness. What is a “base line inundation fraction”? How was it included in the estimation of the flooded/non-flooded areas. I think that the manuscript would be clearer if “Results” and “Discussion” would be separated in two sections. In the current “Results and discussion” section, there is also methodological information (page 27766, lines 8-12) which should be moved to the Methods section. The conclusions section should be rewritten. It is now a mixture of summary and even new results (integrated CH₄ emissions from the VISIT model) which were not shown before. But it does not present conclusions from this study now.

V.) I recommend the manuscript for publication only after major revisions that address carefully the comments I have listed above and below.

SPECIFIC COMMENTS

Page 27761, line 25: “Expanding”? Is it still expanding?

Page 27762, line 6: What is a “semi-climatological” flux?

Page 27762, line 14: What do you mean with “a marshy lake”?

Page 27762, line 20: Please give some indications on measurement principle and

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precision of the sensor.

Page 27762, line 26: Is 17 minutes of pumping needed to flush the sample cell? Or why the average of only 3 minutes of concentration data is taken as representative for the 1 h period?

Page 27763, lines 9-11: Which variability was generated? And how exactly? Generally, more information on the CASA modelling is needed (see General Comment II.).

Page 27764, lines 1-11: This should be better explained and discussed (see General comment III.)

Page 27764, lines 19-20: Rephrase this sentence. There is also respiration during daytime.

Page 27764, lines 22-23: Do you have measurements or literature references which support this statement for the typical wetlands in the investigation sites? CH₄ emission from some wetlands can show strong diurnal variability.

Page 27765, lines 18-19: Why exactly this rectangular area? I suggest that a footprint analysis for the concentration measurements considering the meteorological conditions would be useful to define the optimal area which should be used for the combination of concentration measurements and CASA model results and the according CH₄ flux estimation. (see General comment II.)

Page 27766, lines 8-12: This belongs to the Methods section.

Page 27766, lines 23-28ff: The discrepancy could also be due to a biased estimation of this study, e.g., by an overestimated CO₂ release by the CASA model for wetlands. Potential biases of the here presented approach should be included in this discussion.

Page 27775, Fig. 3: This important figure is very difficult to read. Data symbols are much too small.

TECHNICAL COMMENTS

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Page 27759, title: Please use consistently CH₄ or methane. Specify if you mean high CH₄ concentration or high CH₄ emission.

Page 27760, lines 2-4: The first sentence is not so important for this paper and can be removed.

Page 27760, line 12: Explain the acronym CASA.

Page 27760, lines 21-23: I suggest rewording: "Atmospheric CH₄ is the second most important greenhouse gas after CO₂ and plays an important role in atmospheric photochemistry." Are the photochemical reactions of CH₄ the reason for it being the second most important greenhouse gas?

Page 27760, line 26: Remove "some".

Page 27761, lines 2-3: Use articles "the": "The long-term trend. . .", ". . .in the global CH₄ content. . .". I will not list all the many missing articles "the" or "a" in the paper. Please check it yourself.

Page 27761, line 28: Please write more precise: "CH₄ concentration" instead of just "CH₄".

Page 27763, line 26: "drainage" does not fit here.

Page 27764, line 2: Explain acronyms. "SSM/I"?

Page 27764, line 5: "drainage" does not fit here. These areas were not drained, or?

Page 27764, line 6: remove hyphen: "water table depth"

Page 27764, line 18: Write more precise: "CO₂ and CH₄ concentrations"

Page 27764, line 21: A concentration cannot be amplified.

Page 27764, lines 25-26: What do you mean with "ML . . .is seasonally pronounced. . ." Please rephrase.

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Page 27765, line 25: A concentration does not accumulate.

Page 27766, line 22, Insert comma after "KRS".

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

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