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Interactive comment on "Methane fluxes in the high northern latitudes for 2005–2013 estimated using a Bayesian atmospheric inversion" *by* Rona L. Thompson et al.

Anonymous Referee #1

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General comments

The manuscript by Thompson et al. presents estimates for CH4 emission fluxes in northern high latitudes for the time period 2005 to 2013 using an atmospheric inversion. The study is based on the Lagrangian transport model FLEXPART and measurement data from 22 observational sites in northern high latitudes. The sensitivity of the results to prior estimates of wetland emission fluxes and to the number of measurement sites included in the inversion is also investigated.

The atmospheric methane budget is an important topic and under ongoing scientific debate. There are large uncertainties on the total amount of emissions, but also on the importance of individual source categories, as well as on their change over time.

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Especially natural emission estimates based on process-oriented models show a large uncertainty range. Atmospheric inversions are a widely used technique to gain new insights into methane fluxes. The present study makes use of a relatively new observational network in northern high latitudes. Some of these data have already been used for regional inversions, but not in a combined study for the total area north of 50°N. Therefore, the study makes an important contribution to an improved picture of CH_4 emission fluxes and provides further insights in the quality of available CH_4 emission inventories. The results could also be used for evaluation and improvement of wetland emission models.

The manuscript is generally well written, the figures are well prepared and the results are discussed in an appropriate way. My main questions are related to the different time scales applied in the inversion, e.g. monthly or even annual emission fluxes, monthly initial methane mixing ratios, but 10-day backward trajectories and in-situ measurements. I have a couple of remarks and questions on the applied method as well as some suggestions for improvements (see below). After taking these comments into account I recommend the paper for publication.

Specific comments

- Sect. 2.3: I would like to see Fig. 5 of the supplement in the main paper. Since the wetland data set is the main difference between S1 and S2, it is interesting to see how their spatial distributions differ.

- P8, l23-25: How does the dry soil uptake provided by Ridgwell et al. compare to the LPX-Bern model in terms of absolute numbers and seasonality? Is there a strong interannual variability in the uptake calculated by LPX-Bern, which is neglected by the climatology?

- Which meteorological input data was used for the two land surface models? Also ERA-Interim?

- P9, l8/9: Is there any reason for using 50% of the prior flux as uncertainty? How sensitive are your results to that assumption? And how often do your minimum and maximum thresholds apply?

- P9, l20: What is the reason for calculating backward trajectories for 10 days? The prior emission fluxes are provided on a monthly or even annual basis. Are these 10 days an average transport or mixing time scale? Or is that an attempt to minimize the impact of chemical methane loss on the results, i.e. to minimize the uncertainties from using a pre-calculated OH field?

- P9, l30-32: The calculation of methane loss by the reaction with OH is based on pre-calculated OH fields from the GEOS-Chem model. How does the GEOS-Chem OH field compare to other models? Could you provide a reference?

- P10, l24: Here you state that the initial mixing ratios are calculated at a monthly temporal resolution. Again I have some difficulties to bring the different time resolutions together: monthly mean prior fluxes and initial fields, 10-days backward trajectories released every 3 hours, results compared to in-situ measurements. This approach certainly reduces uncertainties resulting from shortcomings in the representation of the chemical sink, the dry soil uptake, mixing processes, etc., which would become more important over a longer simulation period. However, it also neglects short-term variations that are visible in the observational data, e.g. in Fig. 4. Does that have an impact on your inversion results or not, because you are looking at total emissions over a month? My question probably reflects my limited knowledge of the FLEXINVERT framework.

- P13, l5-7: I do not understand this sentence. Please re-formulate.

- P13, l14/15: Does the low bias in the prior mixing ratios indicate any flaws in the method used for calculating the initial and background mixing ratios?

- P14, l4-5: On page 13 you state that the comparison of the inversion results with

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independent observations is a better indicator for the performance of the inversion than the Taylor diagrams shown in Fig. 5. Figure 6 shows at two out of three independent sites only a modest improvement, which by the way is hard to see from Figure 6 (you might want to change the color scale). What does that mean for the quality of the inversion? Any explanation for that? Please comment on this.

- P14, l29: How is the uncertainty in each grid cell defined? Is σprior calculated as defined on page 9, line 9-10?

- P15, l1/2: Why is the largest uncertainty reduction found in Europe, western Siberia and Canada? Please comment on this.

- P15, discussion of Fig. 9: The posterior fluxes in Fig. 9 show several secondary maxima in the annual cycle. On page 19, l22/23 you mention a small secondary peak in March. Is this the same feature as seen in Fig. 9? It would be great if you could comment on these peaks or at least refer to the later discussion.

- Sect. 4.1: The discussion here is rather lengthy and it is hard to keep an overview over the various studies and flux estimates. I would prefer to see a table or a figure, e.g. a bar chart, summarizing the various inversion results.

- Sect. 4.2: I would suggest to merge this section with Sect. 4.1 to make the discussion a bit shorter and therefore clearer.

- P21, l31-32: Are the wetland models LPX-Bern and LPJ-DGVM also driven by ECMWF EI data? I remember that some of these models are driven by CRU data. In that case it might be misleading to explain the increase in the wetland source by an increase in soil moisture found in EI data.

- Table 1: I would like to see the numbers for the region of >50°N as well.

- Table 3: There is only one reference to Table 3 in the text, discussing the lower cost of the prior flux estimate in S2 compared to the other priors. The other values given in the table are not discussed. Is this table really necessary? And what is actually listed in Table 3? How is the cost defined? Does it come with any units?

- Fig. 7 and Fig. 10: I think the color scale could be improved, especially for the difference plots. It is hard to distinguish the different bluish shades.

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