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Atmospheric Chemistry and Physics Discussions

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 #2

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This manuscript is a well-written, clearly presented description of an inverse modelling study focussing on high latitude sites. Other studies have focussed on this region, but this study is unique for the collection of sites used in the inversion, as well as the significant time period covered in the inversion. It also has somewhat higher spatial resolution than what is commonly used for global inversion studies, making use of different grid resolutions to reflect the measurements constraint on the fluxes for different regions.

The combination of the extensive collection of measurements employed, the still uncertain and hotly contested topic of the (Arctic) methane budget, and the inversion approach make this study appropriate in terms of content for ACP. The results are

interesting, and can be used to evaluate the performance of wetland models, and point to potential shortcomings of the anthropogenic emission inventories. Overall the manuscript is very well presented, although the discussion does run a bit long. (I support the previous reviewer's suggestion to summarize the key results in a table if possible, and condense the text somewhat.) I have only a few relatively minor suggestions, as outlined below.

Why was GFED3 at 0.5 degree/monthly resolution used? There are certainly newer versions of GFED, and temporal resolutions of higher than monthly are the norm now. (The emissions from GFED are still given at monthly resolution, but there are daily and even 3-hourly fields to scale the monthly emissions appropriately.) When trying to capture the synoptic scale variability in methane fluxes, it really does make a difference if the fire burned on July 1-5 vs. July 20-25, which at this point you're neglecting. Because the fire flux is of a relatively small term in your budget the impact is likely not critical, but it is an easily rectifiable methodological shortcoming. If not for this study, than certainly for future work.

In Figure 2 (and in the model in general), is this sensitivity shown only for the lowest model level? Or is the addition from fluxes from other grid boxes within the PBL included?

Figure 5 rather underwhelms in terms of improvements through inversion, however Figure 6 shows (for one set of independent data) a clear improvement in the bias from prior to posterior. Would it be possible to include the mean or median bias per site in Figure 5? This can be done with, for instance, the size of the marker. It would be interesting to see if this was the case for other stations as well, which would help support the argument that the posterior is a significant improvement on the prior, and the flux corrections are really robust.

P11, L35: I think the section starting with "For other continential sites..." should go immediately after the previous paragraph i.e. the comment about the IGR representation error should come later. Or at least remind the reader what "this criterion" is, as it doesn't follow clearly as it's written now.

The citation to (Winderlich 2010) is incorrect. There are several full references missing the full stop.

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