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Interactive Comment

Interactive comment on "Mapping pan-Arctic methane emissions at high spatial resolution using an adjoint atmospheric transport and inversion method and process-based wetland and lake biogeochemical models" *by* Z. Tan et al.

Anonymous Referee #1

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It should be noted that the referee co-published articles with one of the authors of the reviewed manuscript.

This study addresses the key issue of quantifying uncertain methane emissions from Arctic environments, with a particular focus a lake emissions, which are not always accounted for. Here, the authors carry on their previous study on lake emissions and try to get insights on the emission magnitude using top-down inversions. Bottom-up studies face difficulties in gathering enough data in remote Arctic to deduced methane emissions with uncertainties small-enough to reliably close Arctic budget. Atmospheric

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regional inversions should contribute to reduce uncertainties on the methane budget.

This work is very relevant in this context. It is one of the first regional inversions on the region and it is based on already tested methods and systems. Moreover, this manuscript attempts to take full advantage of the available data and assimilates both in situ surface measurements and satellite total columns. Lake emissions, which are not always accounted for, are specifically addressed here as a potentially significant contributor to Arctic emissions. The inversion system is based on a global circulation model with a coarse resolution and a zoomed version around the Arctic with a relatively high resolution for better representing transport patterns close to the pole. The sensitivity of inversion results is assessed using a small set of prior emission scenarios.

The topic suits very well to ACP and the manuscript should be published when all revisions and needed clarifications will be addressed.

1 General comments

Though the authors already carried out an extensive work and analysis, the following points need clarification and revision publication in ACP.

1.1 Satellite observations and bias correction

Using satellite observations in an inversion system is a difficult task. Using SCHIAMACHY at high latitudes in support to surface in situ observations is even more difficult. The authors acknowledge this difficulty and apply filters on satellite data. They also worked on bias correction to minimize any misuse of satellite data in the inversion. However, in its current form, some questions remain unanswered and should be discussed.

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1. Satellite bias is corrected along natural parameters (latitude, air mass factor, etc.) before inversion. Using the same data for debiasing and then for the inversion can be very hazardous. One should make sure that the bias patterns are totally decorrelated from the patterns used in the inversion (concentration gradients in this case). As methane emissions are dominant in tropical regions, concentration patterns could be somehow correlated with satellite bias. In this case, you risk misleading the inversion or at best reduce the number of usable information in the satellite observations. Has it be tried to include the bias correction in the inversion procedure?

2. Though efforts are done to deploy new observation sites around the Arctic ocean, satellite datasets could fill some gap in the observations. In my opinion, this paper has all the elements to partly address this question and should address it. What is the impact of using satellite data on the inversions? This could be estimated by computing the sensitivity matrix (Cardinali et al., 2004). It could also be inquired into by comparing inversions with and without assimilating satellite observations.

1.2 Inversion system and uncertainties

1. The description of the system is somehow hard to follow. Section 3.3 should be clarified, in particular, concerning the nesting procedure and the spin-up periods. It looks like observations are used several times in the different inversions, spin up and nesting procedure. This could artificially increase the weight of the observations multiply used, compared with those used only once. Please discuss this point. It may be necessary to stop the spin-up period when the inversion period starts to avoid multiple use of information, biasing the inversion.

2. The global inversions are used as boundary conditions for the regional inversions. It would be interesting to see the impact of the higher resolution on the inversion results. Could the posterior fluxes from the global and the regional inversions be

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compared for equivalent regions? Anyway, I have some concerns about the way the nesting is carried out. If I understand well, the nested regional model is run on a grid, which does not extend North of 80°. This means that the transport across the Arctic ocean is totally excluded from the regional inversion. Thus, for instance, ZEP only sees the influence of the global boundary conditions as it is really close from the side of your regional domain. ALT is excluded from the regional domain while it is expected to provide some regional information, etc. In the best case, this is a pity of missing some potential information with air masses crossing the Arctic ocean and reaching remote sites. In the worst case, it totally biases the regional inversion and, at the end, the regional is not better (or maybe worse) than the global inversion. This problem must be addressed, especially as you use a relatively scarce network with Arctic sites relatively close to the border of the regional domain.

That being said, I finally do not see what exactly brings the regional inversion to this study.

3. Concerning the prior uncertainties in the inversion, the current system uses a regularization term γ to control the weight of prior information compared with observations. How this term is computed? Is it based on a χ^2 criterion? Couldn't the same procedure be used to also adjust the in situ vs satellite observations? It has been proven that prior uncertainties play a key role in inversion, and wrong uncertainty matrices can lead to totally biased or inconsistent results. Furthermore, a critical point in inversions is a correct specification of posterior uncertainties. Posterior fluxes without posterior uncertainties are mostly worthless numbers produced by very elaborated black boxes (to caricature...). The authors acknowledge this issue and try to address it by comparing inversion results for 6 different wetland prior fluxes. I am confident that these different scenarios can be sufficient to qualitatively discuss the performance of the inversion. In addition, it seems that the 6 scenarios are sufficient (by chance?) to reproduce a

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realistic range of uncertainty when comparing to Berchet et al. (2014) numbers for Siberian Lowlands. However, as the author try to draw some conclusions about the emissions from lakes, dominated by other sources, uncertainties might be too high. This is especially critical as the regional inversions seems kind of unsound. Additional inversions with different observation and prior uncertainty matrices would be necessary to really address this issue.

1.3 Structure, content and title of the manuscript

The manuscript in its current form lacks some consistency between the title, structure and content.

The title makes the reader expects an atmospheric inversion accounting for lake and wetland emissions. Section 4.1 deviates in my opinion from the main topic of the paper. What is the objective of this section? In the current state, it looks like an enumeration of aggregated emissions on global regions and compared with previous work. Though by itself not uninteresting, I don't think it is relevant for Arctic inversions. Maybe the entire section could be moved to supplementary materials (or to a different paper dedicated to global ivnersions).

On the other hand, Section 3.4 seems to me a key part of the manuscript. But the authors chose to put it only at the end of the method section with only limited details. I consider the satellite measurements play a key role in this work, especially as the Arctic in situ sites are very scarce during the inversion window. As noted by the authors, bias correction is essential for using both satellite and surface measurements. An amended version of the manuscript should include an extended discussion on the bias correction, on the performance of the different models, on the relative weight of satellite data in the inversion compared to surface measurements. This discussion is already partly done in Section 3.4 but should be extended and moved to Section 4. Some elements of Section 4.1 may also be used for this discussion.

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The title should render the use of satellite observations as it is not common in Northern latitude.

2 Technical comments

The following points are mostly technical points that need reformulation or some clarification.

- p. 32471 l. 20: the last sentence might over-sell the paper or is too vague.
- p. 32472 l. 24: I think putting together “)(“ should be avoided as much as possible. There are other occurrences of this typo point in the manuscript
- p. 32474 l. 9: inversions are even more sensitive to uncertainty matrices; that should be at least partly addressed
- p. 32476 l. 10: are the outliers numerous? What is the impact of this filtering on the inversion?
- p. 32476 l. 19: the selection is relevant, but some details on how it is done are needed for the reader. Couldn't the excluded sites be used for evaluation? A map of all the sites excluded from the inversion, assimilated in the inversion and used for validation should be provided (at least in the supplementary material), with the borders of the nested model.
- p. 32476: Maybe I missed it but I couldn't find anywhere whether surface observations are continuous or flask measurements.
- p. 32478 l. 17: Can you give an exact definition of “lake”? this seems obvious, but the difference between wetlands and lake could be very tiny in some conditions? Does the map of lakes evolves with time?

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- p. 32479 l. 10: Is there any citation comparing GEOS-4 and GEOS-5? As you use different meteorological forcings for the different inversion windows, it could have an impact on the results. The two datasets are probably very consistent and the impact is probably very limited, but this should at least be mentioned.
- p. 32479 l. 14: if I understand well, for instance, if an air mass from Canada crosses the pole and reaches a site in Siberia, you wouldn't be able to recover any information on the emission with your way of dealing with the pole? it would be then mixed with "boundary" polar conditions? You might lose a lot of information on Arctic emissions considering the fast transport of air masses over the Arctic ocean. Wasn't it possible to implement the procedure of the global system in the nested system?
- p. 32480 l. 10: people unfortunately does not always define Arctic the same way... Please give your definition, so that the reader knows on which region your emissions are defined.
- p. 32482 l. 22: does the system guarantee that it is not stuck in a local minimum? I guess it does, but mentioning only the 0.5% criterion might be insufficient
- p. 32483 l. 14: BIC seems a reasonable score but it is not commonly used, so please give a little bit of details on it.
- p. 32483 l. 25: Does filtering outliers influence the bias correction? What is the portion of data filtered out along this criterion?
- p. 32484 l. 15: is there a known reason for the opposite dependence of model-data differences in East Asia? this only comes from wrong emission inventories or is there a relation with regional meteorology or other?
- p. 32484 l. 22: I do not understand why you need these polynomial trends? Is it that you use monthly or 2-weekly flask measurements and extrapolate them to

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hourly residuals? If so, I think this might be a problem for the inversion. Extrapolating data before inversion can only bring additional uncertainties.

- p. 32486 l. 3: Please remind the inversion windows here. It is not always clear when the satellite data are used.
- p. 32488 l. 20: it would be easier for the reader to draw a picture if the same area were compared.
- p. 32489 l. 13: without uncertainties on the posterior, it is hard to see the impact and the confidence of the inversion. The subsequent discussion is thus very speculative in my opinion. The DLEM scenario with no lakes only shows the limitation of inversion methods, I think... I do not really get the choice of DLEM. The way you put it, it only confirms that the inversion has not enough information to redistribute fluxes. But the missing fluxes could also be wetland fluxes.
- p. 32490 l. 18: both numbers looks pretty high, especially for the total column. What the difference between observed and prior total columns? Is the improvement significant? I think this is the most important here. If with the inversion, you only shift the total columns of 1 ppb without the lakes and of 2 ppb with the lakes, you got a signal; but conversely, if the inversion shifts the total columns by e.g., 30 ppb without the lakes and 31 ppb with the lake, you got nothing...
- p. 32490 l. 22: I think this citation is not relevant. They could have achieved 15 ppb of improvement if taking wrong prior fluxes...
- p. 32491 l. 26: Berchet et al. (2014) did find methane emissions of 1–13 TgCH_4/y from Siberian wetlands, which is amazingly consistent with your figure.
- Tab. 1: Maybe you could add correlation coefficients as you show one R in Figure 1.

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- Figure 1c: it would be interesting to compare on the same figure before and after optimization and to have the same figure for all debiasing method (probably in supplementary material to avoid having dozens of figures...)
- Figure 4: Could you please add the prior and posterior uncertainties? Why does the seasonal cycle vanishes after 1998 in the Tropics? As for Section 4.1, I am not sure this figure is really relevant regarding the topic of the paper.
- Figure 8-9: Please add the prior RMS for each different scenario, so that one can see the improvement after inversion.

References

Berchet, A., Pison, I., Chevallier, F., Paris, J.-D., Bousquet, P., Bonne, J.-L., Arshinov, M. Y., Belan, B. D., Cressot, C., Davydov, D. K., Dlugokencky, E. J., Fofonov, A. V., Galanin, A., Lavrič, J., Machida, T., Parker, R., Sasakawa, M., Spahni, R., Stocker, B. D., and Winderlich, J.: Natural and anthropogenic methane fluxes in Eurasia: a meso-scale quantification by generalized atmospheric inversion, Biogeosciences Discuss., 11, 14 587–14 637, doi: 10.5194/bgd-11-14587-2014, http://www.biogeosciences-discuss.net/11/14587/2014/, 2014.

Cardinali, C., Pezzulli, S., and Andersson, E.: Influence-matrix diagnostic of a data assimilation system, Q. J. R. Meteorol. Soc., 130, 2767–2786, doi:10.1256/qj.03.205, http://onlinelibrary.wiley.com/doi/10.1256/qj.03.205/abstract, 2004.

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