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Atmospheric Chemistry and Physics Discussions

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 #2

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Several studies have pointed to the importance of methane emissions from lakes, but so far no attempt has been made to include those estimates into global atmospheric transport model and assess their influence on inverse modeling results. This study makes an useful contribution by filling this gap. Estimates are provided of Arctic lake and wetland emissions before and after optimization using inverse modeling. This is all fine, but in the end it is still not so clear whether or not the model has improved by the inclusion of lake emission and what it means for the overall Arctic methane budget. In my opinion, some more in depth analysis in this direction would increase the usefulness

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of this study. Right now, the conclusion section has some general statements that don't seem to be supported by the results, or at least not in the way the results are presented. Improvements in this direction will be needed, as explained in further detail below, to make this manuscript suitable for publication.

GENERAL COMMENTS

The statement in the conclusion section that "biogeochemical models tend to overestimate natural sources in the Arctic" calls for a comparison of numbers, together with their uncertainties and a discussion of possible factors influencing the comparison. The numbers are given in Table 3. Looking at the ranges they seem to support the conclusion. However, does the range of posterior estimates reflect the posterior uncertainty? If not, the difference between prior and posterior fluxes may not be significant. Since only a single lake estimate is used this part of the uncertainty is in any case not accounted for judging only emission ranges. What factors could influence the comparison? Without the lake emission estimates the biogeochemical models would be fine. Could it be that by simply adding up lake emission estimates to the process model results, emissions end up being double counting? For example, if lakes appear in places that already count as wetlands in those models. Particularly when the model prescribes inundated area using satellite data there is no clear boundary between the two. Some further discussion is needed of how these contributions fit together and what the implications are for the uncertainty of the estimates.

It is difficult to judge the added value of the regional inversion from the way in which results are presented. Table 3 is the only place where a direct comparison between prior and posterior is made. Looking at the ranges, the results actually suggests that the inversion increases uncertainty. Otherwise the plots for the regional inversions show either prior or posterior fluxes, but no differences between the two. This makes it hard to judge where inversion results converge or diverge in the inversion process. The impact of accounting for lakes is discussed in the text – where suggestions are made that it is important to do so. This is the kind of discussion that is expected from a paper,

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which investigates the role of lakes. However, only one figure in the supplementary information shows any results supporting this discussion. Since it only shows posterior results, it is difficult to compare with any of the other figures. The point about the importance of including lake emissions has to be demonstrated more convincingly.

Figure 8 and 9 demonstrate how the inversion-optimized fluxes improve the fit to various measurements. What I find missing in these figures is the range of a priori RMS values (I mean from each inversion). I wonder also whether posterior RMS's correlate with the priors. In other words, does the pattern of posterior mismatches reflect that of the prior or not? A more important omission, however, is a quantification of the role of lakes in these figures. Is there any gain in terms of RMS by including a pattern of lake emissions in the inversion?

The final conclusion that the nested modeling approach improves the simulation of methane mixing ratios is not supported by results. The same is true for the sentence that follows about the understanding that is gained about Arctic emissions by simulating methane with more spatial detail. Either provide the supporting evidence or otherwise remove the conclusions.

SPECIFIC COMMENTS

Abstract, line 13: "Canadian and Siberian lakes contribute most of the estimated lake emissions " What do you mean here, to Global or Arctic lake emissions?

page 32475, equation 2: where does "XCO2" come from?

Page 32479, line 16: The Southern bound of the Arctic nested grid is 56N. Does this mean that all reported total fluxes from the nested grid inversion represent fluxes northward of 56N? In several places there is mentioning of 60N, and somewhere even 50N. Confusion should be avoided on what is called "Arctic".

page 32483, line 25: Why is this condition restricted to measurements between 50S and 50N? It hints at something that requires further specification. In the studies by

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Bergamaschi et al and Houweling et al, SCIAMACHY retrievals are filtered out outside this latitude interval. Figure S1, indicates that higher latitude measurements are used in this study, although this line 25 suggests that data are treated differently. This should be clarified.

page 32486, line 8: "this suggests that the global emissions . . ." It should be noted here that the convergence of global totals relies on the assumed atmospheric lifetime being correct. There is no mentioning that atmospheric sinks are optimized. If they were, then the measurement constraint on the global total emission would have been substantially less.

page 32487, line 24: "They probably underestimated . . ." This difference could be caused by a different assumption on the methane lifetime, the uncertainty of which may well exceed 10 TgCH4/yr.

page 32488, line 4: "This adjustment could be primarily driven . . ." Then a list follows of every element in the inversion that influences the a priori fluxes. Therefore, effectively this sentence doesn't say anything. However, it would actually be interesting to know the relative importance, for example, of the satellite and surface data. This has been studied in the past by others for the global domain, but not specifically for the Arctic sub domain.

page 32490, line 11: "We conducted a nested grid inversion . . ." Somewhere in the part that follows a reference is missing to figure S3.

page 32493, line 27: "But our study also suggests that . . ." Here a reference is missing to Berchet et al, ACPD, 2015 (doi:10.5194/acpd-15-25477-2015).

page 32512, fig 3: It is not clear if the totals refer to Global or Arctic emission totals. Furthermore, please put the totals under the figures to improve readability.

page 32512: figure 3: Is the resolution of CLM4Me indeed so much lower than the other models?

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SPECIFIC COMMENTS

page 32473, line 24: “Previous” i.o. “And previous”

page 32483, line 23: “SIAMACHY”

page 32489, line 3: “by that the”

page 32491, line 26: “the CH4 budget of”

page 32492, line 24: “help”?

page 32510, figure c: axis titles are missing (they should be along the axis instead of in the caption).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 32469, 2015.

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