Atmos. Chem. Phys. Discuss., 15, C13744–C13752, 2016
www.atmos-chem-phys-discuss.net/15/C13744/2016/

Atmospheric Chemistry and Physics Discussions

ACPD

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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.

Z. Tan et al.

tan80@purdue.edu

Received and published: 1 July 2016

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 a 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 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.

Response: We appreciate the valuable comments from the reviewer. To address the concerns raised by the reviewer, we have used a Monte Carlo stochastic approximation method to calculate the uncertainty of posterior estimates. Fig. 5 shows that assimilating satellite retrievals reduced the uncertainty. In Fig. 6, we did a more detailed comparison between the inversion considering lake emissions and the inversion not considering lake emissions. It shows that there should be strong CH4 emissions in the specified yedoma permafrost region that is missed by the DLEM model. Since 56% of the water-inundated landscapes are lakes in the region, there is a non-negligible possibility that the missed emissions by the DLEM scenario could be from lakes. And it is possible that emissions counted for wetlands in other wetland models actually are from lakes. We are cautious to draw a conclusion that CH4 emissions from lakes must be included in inversions or are significant across the pan-Arctic because there is still very large uncertainty. But the point is that the inversions in this study can shed light on this source at large spatial scales that are unachievable from field observations and the inversions are more reliable than biogeochemical models. Also, we have changed the structure of the manuscript to focus on the following questions: 1) how large the impacts do the wetland biogeochemical models have on pan-Arctic CH4 inversions and in which direction can the wetland biogeochemical model can be improved for the use of inverse modeling?; 2) can the inclusion of CH4 emissions from lakes improve the results of inverse modeling?; 3) can the assimilation of satellite retrievals reduce the uncertainty of the posterior estimates?; and 4) to compare the possible debiasing method for global or pan-Arctic scale inversions?

General Comments

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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.

Response: In the revision, the posterior uncertainty was calculated. According to Fig. 5, we can still claim that biogeochemical models could overestimate CH_4 emissions in the pan-Arctic. But now this is not a conclusion we are urgent to draw. Rather, we want to say that according to this figure, in addition to Table 2, the estimated uncertainty caused by unrealistic spatial and temporal patterns of biogeochemical models could be larger than the uncertainty caused by observation and prior emission magnitude uncertainties. This emphasizes the importance of improving biogeochemical models to achieve consistent spatial and temporal variabilities. The value of the estimates for lake emissions here is to shed light on the upper and lower bounds of this source. Because the lake model is combined with different wetland models in which some could have stricter definitions of wetland area and some could have wider definitions, in addition to data assimilation, the results can give us more insights on the magnitude of the source than the lake model alone. It can also be true for CH_4 emissions from wetlands.

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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 suggest 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, 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.

Response: To address these issues, we calculated the posterior uncertainty of emission estimates and showed it in Fig. 5. Fig. 6 shows that the inclusion of lake emissions improves the agreement between the GEOS-Chem model and satellite retrievals. We also compared the RMS of the posterior global and pan-Arctic inversions over the pan-Arctic surface and aircraft observations.

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?

Response: We have improved these two figures according to the comments (see Fig. 7 and Fig. S6). Now the RMS from the prior of each scenario shows together with the RMS from the posterior. For lake emissions, Fig. 6 can show some gain in terms of RMS if lake emissions are included. We think it is difficult to explain the gain in terms

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of RMS using other observations because both surface sites and aircraft missions are far from the regions where lakes are obvious dominant in the GLWD map.

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.

Response: We have revised the discussion and conclusion according to our results. According to the results, the following conclusions can be drawn: 1) the realistic spatial and temporal variabilities of prior CH4 emissions from wetlands are important for inverse modeling; 2) satellite retrievals can be used to reduce the uncertainty of the estimates of CH4 emissions in the pan-Arctic; 3) high-resolution nested grid inversions improve the performance of inverse modeling; and 4) there could be large spatial scale CH4 emissions from pan-Arctic lakes in some specific region.

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?

Response: We have revised it to “Canadian and Siberian lakes contributed most of the estimated CH4 emissions from pan-Arctic lakes.”

Page 32475, equation 2: where does “XCO2” come from?

Response: The XCO2 comes from the CarbonTracker CO2 measurement and modeling system.

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”.

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Response: Although the inversions were conducted northward of 56°N, only emissions northward of 60°N were analyzed. In the revision, we changed “Arctic” to “pan-Arctic” and defined “pan-Arctic” as a region northward of 60°N. For the place 50°N, it is because the cited study does not calculate methane emissions from 60°N separately. In that case, we have not tried to imply the emissions from 60°N and 50°N should agree.

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 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.

Response: Following Bergamaschi et al. (2009) and Houweling et al. (2014), we also filtered out measurements outside 50°S and 50°N because in these regions SCIAMACHY only delivered good-quality retrievals in local summer times and we run whole-year inversion at the global scale. Before, we applied the regression relationship of Fig. 1c to the pan-Arctic inversions. We realized that it could be problematic. In this revision, following the method of Wecht et al. (2014), we used aircraft campaign measurements from Alaska, Canada and Siberia to calculate a linear regression between bias and specific humidity. This relationship was then applied to all nested grid inversions. We showed this new regression and aircraft campaign sites in Fig. 2 and 3.

Wecht, K. J., Jacob, D. J., Frankenberg, C., Jiang, Z. and Blake, D. R.: Mapping of North American methane emissions with high spatial resolution by inversion of SCIAMACHY satellite data, J. Geophys. Res. Atmos., 119, 7741–7756, doi:10.1002/2014JD021551, 2014.

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

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were, then the measurement constraint on the global total emission would have been substantially less.

Response: We have added this assumption into the sentence: “This suggests that the surface observations are of sufficient density to constrain the global emissions if atmospheric CH_4 lifetime is correct”.

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 $TgCH_4$/yr.

Response: You are right. We have changed the tongue of this sentence.

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.

Response: As our focus is on the inverse modeling of CH_4 emissions from the pan-Arctic, we did not do more work to investigate the possible reasons. But it is possibly very complex. We have deleted the sentence to reduce confusion.

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

Response: We have added the reference to Fig. 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).

Response: We have added this reference.

Page 32512, fig 3: It is not clear if the totals refer to Global or Arctic emission totals.

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Furthermore, please put the totals under the figures to improve readability.

Response: The totals refer to pan-Arctic emission totals. We have put the numbers under the wetland scenario or source names to make the figure more readable.

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

Response: Yes, the CLM4Me model has a spatial resolution of 1.9° × 2.5° but many others have a spatial resolution of half degree (SDGVM has a resolution of one degree).

Page 32473, line 24: "Previous" i.o. "And previous".

Response: We have revised it.

Page 32483, line 23: "SIAMACHY"

Response: We have revised it.

Page 32489, line 3: "by that the"

Response: We have changed this sentence to "The exception of the ORCHIDEE inversion could be explained by the very high wetland CH4 fluxes the ORCHIDEE model simulated in Canadian Shield, West Siberia Lowlands and East Siberia Coastal Lowlands where high CH4 fluxes from lakes are also possible"

Page 32491, line 26: "the CH4 budget of"

Response: We have revised it.

Page 32492, line 24: "help"?

Response: "help transport" was replaced by "quickly transport".

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

Response: The problem is that there is no enough space to put them; otherwise this

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subplot will become too small. As this figure has been move to the supplement, we chose to keep the current format.

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

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