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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global methane budget and trend, 2010–2017: complementarity of inverse analyses using in situ (GLOBALVIEWplus CH_4 ObsPack) and satellite (GOSAT) observations" *by* Xiao Lu et al.

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This paper presents an analysis of the global methane budget and trend from 2010-2017 by simultaneously optimizing the source distributions, the OH sink (through hemispheric scaling factors), and linear trends using an analytical inversion approach with the GEOS-Chem model. Overall it is clearly written and structured and the figures are sufficiently clear and complete. From the subject matter it fits well within the scope of ACP.

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Discussion paper

At first glance this paper seems extremely similar in approach and content to Maasakkers et al. (2019) who used a very similar setup with the same model over an overlapping period (2010-2015) to do basically the same thing. The main difference that I can see is that here surface measurements are also included as a data constraint in order to show their complementarity (and consistency).

There's something a bit worrying showing up in Figure 6. Figure 6 seems to show that the both the in-situ-only and GOSAT-only inversions overestimate concentrations in the southern hemisphere and underestimate them in the northern hemisphere (more in the mid-latitudes in NH than in the Arctic). Interestingly, this consistent latitude-dependent bias does not seem to be present in the priors, or at least not as strongly. (Note that the 60-90N and 60-90S curves are more or less on top of each other when compared to the observations for the prior runs.) The fact that they then diverge so systematically after optimisation seems to imply that something is going wrong with the OH hemispheric optimisation - or is there another explanation?

Interestingly this pattern appears least distinct when considering the in-situ-only posterior sampled at GOSAT locations, whereas it is most pronounced in the GOSAT-only posterior. Can you explain this? Does this have something to do with the seasonal latitudinal coverage of the GOSAT measurements? In the comparison of the GOSAT-informed concentrations (both with and without the in-situ data) to the ObsPack measurements (panels 6c and 6d, less evident in 6b) there seems to be almost an temporal anti-correlation in the model-data mismatch between the 30-60N stations and the 60-90N stations.

It seems to represent a systematic error in the interhemispheric gradient, which can be explained through either the distribution of the sink, the distribution of the sources, or errors in the transport – or most likely a combination of all three. However as both the sink and the sources are being optimised, it seems surprising that such a zonally-dependent offset is emerging. Even if there are transport errors (and there always are), I would expect a solution to emerge that was consistent with the interhemispheric

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gradient of the measurements. Of course the OH sink is only being optimised as a hemispheric scaling: might this reflect a problem in the spatial or temporal distribution that is being scaled? Still, usually the fluxes will adapt to compensate, provided they have sufficient flexibility. The fact that Zhang et al. (2018) found the inversion results to be not so sensitive to different OH fields suggests that this is not the case.

Some explanation of the source of this systematic error should be included. The only mention of transport errors is the claim that the regularisation factor gamma should help account for error correlations in the observations due to transport and source aggregation errors. Interestingly this does not seem to appear in the very similar simulations from the same group with a similar set-up, as seen in Figure 3d of Maasakkers et al. (2019).

Perhaps the most interesting (while also troubling) result is in Figure 13: the negative correlation between methane lifetime and estimated (anthropogenic) emissions is not in and of itself surprising. What is surprising is the fact that none of the three solutions are in any way consistent with each another. This can be explained by an underestimating of the posterior error covariances, as the authors do in L505-509. The fact that the GOSAT+in situ result does not lie somehow between the GOSAT-only and in-situ-only result is, however, worrying. The authors suggest that this is due to a correction of a bias in the GOSAT-only inversion by ingesting the in-situ measurements. This bias was diagnosed as being in both the OH (too low, because the methane in the SH was overestimated) and the fluxes (too low, because the methane in the NH mid-latitudes was underestimated). From this perspective it makes some sense that it would correct in the direction that it did, but why would it overshoot the in-situ-only solution? Is there some fundamental inconsistency in the two types of measurements (or an error in the model) that makes it impossible to match them both simultaneously?

This result seems to suggest that the measurements themselves are not really consistent with each other, which the paper claimed to set out to test (L91-94). Thus this result seems to contradict the conclusion that "the GOSAT and in situ data are generally con-

sistent and can fit each other independently through our inversions" (L535-536). Even if the concentrations in the different inversion come closer to each other, is the result really consistent if the emissions and the lifetime are so very divergent?

While trying to understand this rather surprising result I realised that I would like to see some more figures: OH was scaled per hemisphere per year (16 state vector values). A time series of these scaling factors (perhaps as an additional panel or two in Figure 7?) would be interesting to see, rather than just an average lifetime over the whole period (similar to Figure 7d in Maasakkers et al. (2019)). This might also help convince me that scaling OH based on surface-based methane measurements alone makes sense - do the OH scaling factors in this case stay close to one throughout?

Another plot that might help convince the reader of the adequacy of the transport model and the improvement of the sources and sinks would be geographical (zonal + altitude?) plot of the model-data mismatch for aircraft data presented in Figure 5d. Even if it has to go into a supplement, it would be a useful piece of information for the reader to assess if this very surprising result might make sense.

Once these concerns are addressed, I think the paper would be appropriate for publication in ACP.

Postscript: I have only just now seen the short comment by Western, and do not have time to read deeply into this at the moment, but wanted to submit my review all the same in the meantime.

Minor comments:

I would recommend adding how many independent pieces of information are contained in the GLOBALVIEW measurements alone to the abstract. This information is contained in the paper, but the way the numbers are presented in the abstract (which is as far as some readers get), it rather underplays the observation constraint brought about by the in-situ measurements alone.

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One point that should be added into the discussion: When looking at the ability of a measurement system to assess long-term trends it is critical to consider the length of time over which these measurements are available. In this case, the surface-based network still has an advantage, and does not suffer from the same comparability issues that can arise when new sensors/sampling are introduced. This is mentioned briefly in lines 567-568, but they are first mentioned as a method for satellite validation. Unless this measurements are being made across a profile (such as AirCore or aircraft), I cannot see how this could be the case.

In line 475-476 you mention in passing that your optimisation approach can only solve for constant linear trends over the whole inversions period, which may not be appropriate for China. I wonder if it is really appropriate for other regions either? This is a clear drawback to the choice of state vector in your analytical inversion setup, and should be more clearly stated as such. If you want to test if this lack of trend is consistent with the findings of Sheng et al. (2019), showing an increase to 2012 and a decrease afterwards, perhaps you could perform the same inversion but broken up into two chunks: 2010-2012 and 2013-2017. Yes, this would require new transport simulations, but it would be interesting to check the robustness of the other trends as well. However this might be beyond the scope of the current study. (Perhaps something to add to the discussion?)

I noticed that the panels labelled "China" and "Canada" in Figure 12 are identical. I suspect that they're both showing the results for Canada? In any case, this should be checked carefully and corrected.

Typographical/language remarks:

Co-author Hartmut Boesch's last name is misspelled.

L127: with largest -> with the largest

L162: WETCHART -> WETCHARTS

L169: "full-chemistry" should not be hyphenated here (not a compound adjective before the noun)

L172: closed -> close

L218: challenged -> challenging

L225: Bayesian -> The Bayesian

L231: underestimate -> underestimation

L238: change -> changes

L266: be somewhat deviated -> deviate somewhat; overfit -> overfitting

L278: overfit -> overfitting

L284: Analytical solution -> The analytical solution

L288: I would suggest adding a colon after "analyses"

L290: capitalisation of "In situ-only" seems odd. Perhaps "in-situ-only" would be better as a compound adjective.

L339: year -> years

L345: by year -> by the year

L349: has insignificant -> has an insignificant

L364: higher information than in situ observations -> more information than do in situ observations

L375: I guess that "." should just be ","?

L392: In situ observation is -> The in situ observations are

L418: Thompton -> Thompson

L453: US -> the US

Figure 11: I guess this percentage change is over the full period (rather than per year)? This should be clarified in the caption label. It also makes it a bit hard to compare to the text, where % trend per year is given. I assume that this is not a compounding percentage change, but rather the total percentage change divided by the number of years? In any case, this should be clarified.

L501-502: This might seem like a small thing, but this is one of the most interesting findings of the paper, and as such should be perfectly clear. I would suggest the following change in phrasing: "are more effective than the satellite observations in independently constraining methane emissions from the sink by OH." -> "are more effective than the satellite observations in constraining methane emissions independently from the OH sink."

L553: weak -> a weak

L560: remove "the"

L561: and methane lifetime -> and a methane lifetime

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