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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Attribution of the accelerating increase in atmospheric methane during 2010–2018 by inverse analysis of GOSAT observations" *by* Yuzhong Zhang et al.

Anonymous Referee #1

Received and published: 23 October 2020

Zhang et al uses an analytical inverse method to estimate methane emissions from the GOSAT data from 2010 to 2018. The study contains a lot of information that is poorly structured and explained so I found it difficult to extract any new or notable scientific insights. I recommend a major revision that takes into the recommendations I have made below to help draw out the key points of the paper. Given the eminence of the coauthors I feel slightly aggrieved I have needed to make some of these points.

Minor Comment

I think the language is a bit odd in places. Easily fixed but needs an overhaul.

Major Clarifications

Cleanly separating wetland and non-wetland emissions, including rice paddies, etc is a bold claim. The authors' motivation, based on WetCHARTs, is that wetlands have relatively coherent spatial behaviours. From what I understand the authors' state vector is at grid-point resolution for non-wetland sources and their trend, but the wetland emissions are described on much larger spatial regions. I remain unclear whether using a combination of small and large geographical regions will decrease or increase posterior correlations between wetlands and anthropogenic emissions. An argument could be made for both sides. Certainly, more discussion/description is needed.

These two statements are apparently contradictory: Line 155: "Our prior estimate assumes no 2010-2018 trends in non-wetland emissions. . .." Line 160: "We specify an absolute error standard deviations of 5%/a for linear trends of non-wetland emissions. . ."

Line 161: Given the diversity of emission estimates from the ensemble members of WetCHARTs and the resulting uncertainty covariance structure, I am left wondering how sensitive the authors' posterior solution is to this information.

Line 221: Given your reliance on the residual error method I think it would be useful to explain this a bit more.

Line 224: Please clarify whether you use the brute force method to calculate the Jacobian matrix or take advantage of the tagged CH_4 simulation, as described in section 2.4.

Line 255: Comparison with NOAA is an important first step to determine whether your posterior solution is consistent with the observed atmospheric growth rate. Unfortunately, Figure 3 and the accompanying text is not sufficiently clear for this reviewer to make that judgement. For the lower right panel of Figure 3 it would be better to use a smaller y axis range given the small differences. At least then a reader can eyeball the comparison. I recommend "better fit" is substituted with some quantitative statistics, e.g. bias, correlations, etc.

Comment on arrangement of paper: showing your ability to independently estimate wetland and non-wetland emissions on the spatial scale you are using (Figure 14) would be useful up front before the more detailed discussions begin. See my comments below.

A broad comment relevant to more than one figure, e.g. Figure 3: the period 2010 to 2018 covers a wide range of climate variations (e.g. both phases of ENSO) so that taking the mean or differences over this period hides a lot of useful information about how methane has changed.

Line 289: averaging kernel sensitivities? The matrix of averaging kernels already describes sensitivity. Do the authors mean the sensitivities described by this matrix?

Line 292: from what I understand the state vector has a length of 1000s but the DOFs is limited to 179. What implications does that have for being able to resolve the state vector?

Line 291: "By applying the posterior/prior correction factors to the prior distribution of each anthropogenic emission sector, we obtain improved estimates for anthropogenic emissions for that sector." This statement does not make sense. Improved how? Better fit to measurements? Smaller uncertainties? Applying correction factors does not improve estimates for anthropogenic emissions. Which sector?

Comments on reporting results: some posterior estimates are accompanied by their uncertainties and some are not. The authors need to be consistent. They should certainly report them on Figure 6.

Line 315: I am really interested to hear more about how their results point towards an underestimate in livestock emissions. This whole paragraph contains so much hand waving I thought I was at a pre-pandemic music festival. I urge the authors to justify their result in light of it being inconsistent with Maasakkers et al and other published studies that attribute most of these changes to wetlands. I acknowledge they take a

second swing at this point on page 15 but I was unsure which they used for their prior (for Figure 8) and more importantly how confidently they could separate wetland and livestock emissions.

Line 358: why are the constraints strongest over India and China?

Line 539: That's a very long lifetime for methane against OH oxidation. This reviewer is left wondering whether the authors' inverse problem remains ill-posed given their state vector. The joint PDF clearly shows strong correlations between different parts of the (global mean) state vector. Given the authors reporting of regional methane missions, it would be useful to show the regional joint PDFs for which I suspect the correlations between anthropogenic and wetland emissions can be much higher. Their conclusion on page 24 doesn't fill me with confidence.

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