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Atmospheric Chemistry and Physics Discussions

Interactive comment on "High-resolution inversion of methane emissions in the Southeast US using SEAC^{4}RS aircraft observations of atmospheric methane: anthropogenic and wetland sources" *by* Jian-Xiong Sheng et al.

Anonymous Referee #2

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General comments

This study uses observations of CH4 from aircraft campaigns in Aug-Sep 2013 to estimate CH4 emissions in the southeast US, a region with importance to the US total anthropogenic CH4 emission and with significant areas of wetlands. The authors use a Bayesian inversion method to estimate the emissions and uncertainties. The method is scientifically sound and the manuscript is well-written. However, there are a few outstanding points that should be clarified before publication. In particular, I think the results of the study would be strengthened by adding a sensitivity test to determine the

sensitivity of the results to the prior wetlands emission estimate used (see also specific comments). In summary, I recommend publication after minor revisions.

Specific comments

P1, L13: The authors state that the results of previous inversions finding higher emissions than EPA estimates were owing to too low estimates for wetland emissions. What estimate for the wetland emissions is the reference here?

P2, L19-20: While errors in the prior can bias the results, the extent to which this is a problem is dependent on the constraint from the observations and on the uncertainty assigned to the prior estimates, or in other words the degrees of freedom to adjust the prior. This statement should be qualified bearing in mind these other factors too.

P3, Eq.1: Strictly speaking, the RHS of this equation should be multiplied by 1/2

P3, L14: Here the authors say that inversions of methane are usually solved numerically, however, that is not the case. While global inversions of CH4 over many years usually use numerical adjoint methods, regional inversions (as in this study) also often use the analytical solution.

P3, L15: While it is true that the analytical method allows the posterior error covariance to be calculated directly, some numerical methods allow it to be estimated.

P3, L26: Did the global simulation optimized with GOSAT also include surface observations? This should be mentioned. Also, it has been shown that satellite-only optimizations can lead to be biases due to errors in the retrievals. Have comparisons of the CH4 mixing ratios from the optimized simulation against surface and/or aircraft observations been made?

P4, L12: I think it would be helpful if the authors would briefly explain the residual method, which is used to calculate the observation error variances.

P4, L28-29: I'm not sure how the inversion can return information at the fine scale

when it is performed at coarser scale. I think further explanation would be helpful here.

P5, L20-25: There is a little bit of overlap in the locations of the anthropogenic and wetland sources (see Fig. 1). Has this been factored into the posterior emission estimates per source type?

P6, L8-9: The WetCHARTs ensemble mean was used as the prior for the inversion, therefore, it is not surprising that the ensemble mean has the lowest RMSE compared to the posterior emissions, as the two estimates are not independent from one another. This should be stated. Also, have the authors looked at the Gain matrix to determine how well constrained the wetland emissions are by the observations? It would be valuable to test how sensitive the results are to using a different prior for the wetland emissions.

Technical comments

P1, L5: I don't think the term "state-of-science" exists (or at least I've never heard it before). I suggest replacing with "state-of-the-art" or "up-to-date" (also elsewhere in the manuscript).

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