Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-467-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Monitoring Global Tropospheric OH Concentrations using Satellite Observations of Atmospheric Methane" *by* Yuzhong Zhang et al.

Anonymous Referee #2

Received and published: 1 July 2018

This paper uses OSSEs to test whether satellite CH4 measurements (SWIR and TIR) can be used to constrain gridded CH4 emissions and global/hemispheric mean OH simultaneously. The paper is well written and within the scope of ACP. I have a few suggestions below.

Major comments:

The key argument here is that gridded CH4 emissions and global/hemispheric [OH] can be constrained independently. To assist such an argument, the paper uses a few sensitivity tests using a global scaling of CH4 emissions and/or [OH]. The argument would be much more robust if additional tests perturbing the spatial/temporal distribu-

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Discussion paper

tion of gridded CH4 emissions can be done.

MERRA-2 and GEOS-FP are used in both “true” and prior simulations. The two met fields are similar in model setups and assimilation system. Thus the effect of transport errors (e.g., in horizontal advection) is largely not taken into account in the OSSE, which may mean an underestimate of the error in the inversion. Although the native resolutions of MERRA-2 and GEOS-FP are different, the GEOS-Chem simulations here are done on a low resolution (4x5), lower than the native resolutions, thus the effect of resolution difference (that would lead to differences in transport) is not in effect here. These caveats should be better discussed. Is it possible to compare the met fields (e.g., wind fields) to ECMWF or other assimilated fields, to better discuss the transport errors? The horizontal transport errors are particularly important here, because the loss of methane mostly occurs in the tropics but methane emissions can be from anywhere.

In Sect. 2.3, please show the equation linking x to y. This will much improve the understanding of the inversion theoretical basis.

Ignoring the off-diagonal component of a priori error covariance matrix (SA) is a concern, and the choice should be better justified. Errors in gridded CH4 emissions are obviously correlated. Also, it is no surprise that errors in [OH] (from v11 simulations) and errors in CH4 emissions (that drive v11 simulations) may be correlated to some extent.

The use of gamma in Eqs. 4-7 changes the weight of a priori versus observation in determining the a posteriori, and is essentially an adjustment of the errors in a priori (SA) versus observation (SO). The very low value of gamma chosen here (0.05) means that SO is scaled up by a factor of 20, assuming SA is not changed, which is a concern. An extensive explanation (beyond the argument about overfitting) is needed. Could this scaling be a reflection of how the off-diagonal components of error covariance matrices (in SA and/or SO) are treated?

Specific comments:

P2, L23, the tropospheric OH-induced lifetime is 6.3 yr in Prather et al.

P4, L27, the “fraternal twin” problem is reduced here, not avoided. For example, see my major comments on transport.

P5, L20-21. The “true” lifetime here appears to be shorter than Prather et al. (10.2 yr) or the multi-model average in Naik et al. (9.7 yr). Please indicate this difference.

P5, L34, please see my major comment. MERRA-2 and GEOS-FP cannot be regarded as two independent met fields.

P6, L19-20, please indicate that you assume the errors to be random, which may not be realistic.

P6, L31, are there particular reasons to exclude ice-covered land, which may contain anthropogenic emissions (e.g., from industries and pipe lines) and/or natural sources (e.g., from seeping).

P7, L1, “linearly” is not correct.

P7, L8-9, the statement that model transport error correlation can be ignored needs better explanations.

P7, 9-10, here the a priori error in gridded CH4 emissions is assumed independent from the error in [OH]. Please justify this argument. I suspect that these two errors are correlated, because the model global [OH] are simulated with inputted CH4 emissions.

P8, L17, TIR is more sensitive to the upper troposphere, which means limited capability of retrieving [OH] (as shown in Figure 5). Please revise the sentence.

P8, L22-34, as mentioned before, additional tests on the spatial distribution of priori gridded CH4 emissions would be necessary for a robust test of the interdependency between inversed gridded CH4 emissions and inversed [OH].

P11, L4-5, see my last comment on testing the importance of a priori gridded CH4 emissions.

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