Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-467-RC1, 2018

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

M. Krol (Referee)

m.c.krol@uu.nl

Received and published: 29 June 2018

This is an interesting approach that has the aim to get OH information from CH4 satellite retrievals. The paper presents an OSSE and is generally positive about the possibilities to get this information. After reading the paper I am much less optimistic and the annotated manuscript contains my comments and suggestions. Some main points are summarised below.

First of all, the overly optimistic summary "We find that the satellite observations can constrain the global tropospheric OH concentrations with a precision better than 1% and an accuracy of about 3% for SWIR and 7% for TIR." relies very much on the

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OSSE set-up. I like the fact that the authors took the effort to run the inversion with various different OH distributions, because this highlights the problem: we are far from sure what the OH distribution is, and how OH varies in time. While for the emissions a grid-optimisation is performed, OH is optimised as only 1 parameter. This hides the fact that you assume knowledge of the distribution. I acknowledge that the inversion uses a (slightly) different OH distribution, as well as perturbations to the meteorology. However, these differences are poorly quantify. It would help to show the impact of different OH/meteorology on forward CH4 column simulations (see comments in the manuscript). I think it is appropriate to tone down the conclusions considerably and to acknowledge that the result is sensitive to the set-up of the OSSE. An optimisation of the 3D distribution of OH together with an emission scaling would give totally different results I guess.

Second, the authors claim that "GEOS-Chem relates linearly x to y". This is true for emissions, but not for OH. In that sense the analysis might be flawed, although I believe that non-linearities are small. Nevertheless this should be corrected and a work-around for the non-linearities should be found.

Third, figure 6 appears wrong to me since lifetime and emissions should be negatively correlated (I guess OH is analysed in the plots). Also the authors should try to find a work-around for determining the regularisation parameter gamma. I understand that the massive amount of observations in the cost-function has to be de-weighted, but in practical application the "true" emissions are not available and the methods breaks-down. Chi-square statistics or another form of regularisation are possible alternatives.

All the above sounds rather negative. However, overall the manuscript is well-written and the idea is really nice and deserves publication. As said above, I made many more comments in the annotated manuscript. I hope this helps to further improve this interesting manuscript.

Please also note the supplement to this comment:

https://www.atmos-chem-phys-discuss.net/acp-2018-467/acp-2018-467-RC1-supplement.pdf

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