Review of "Smithsonian Astrophysical Observatory Ozone Mapping and Profiler Suite (SAO OMPS) formaldehyde retrieval", by González Abad et al.

General comments

The paper presents SAO formaldehyde retrievals from OMPS. It is a continuation of the SAO OMI formaldehyde retrieval work. The paper is well written and the results are presented clearly. The manuscript is well suited for AMT. However, the retrieval method and settings are mainly identical as in González Abad et al. (2015), and the retrieval of formaldehyde from OMPS has previously been presented in Li et al., 2015. The scientific discussion of the results is not fully developed, and contains some imprecisions.

I recommend publication after the following major revisions:

Too many qualitative affirmations are given in the abstract and in the rest of the paper. The impact of the OMPS and OMI different spatial and spectral resolutions on the detection limits should be better quantified and discussed.

The description of the method to select an optimal fitting window is difficult to understand, see the specific comments. While the selected fitting window is certainly appropriated for H2CO retrievals, the explanations are not convincing. This part needs to be improved.

The discussion of the AMF error contains some shortcomings. For one particular parameter, the given error estimates will depend on the other parameter values, and on the profile shape. For example, an error on the cloud pressure will have a different impact on the AMF depending on the profile shape. This is not discussed at all.

Section 4 should be extended. Comparison with NASA OMPS formaldehyde product (Li et al., 2015) is missing, since a direct comparison of two different algorithms applied on the same instrument would bring interesting information on the products. It is not clear why two OMI products are used in the comparison. Furthermore, the use of the BIRA OMI product v14 needs to be verified. Averaged columns over some regions do not match the gridded files distributed on the TEMIS website, in particular over the Pacific Ocean and Southern Africa.

Specific comments

Abstract, p9210, line 17: Please explain how the detection limit (7.5e15 molecules cm-2) is derived from the RMS (5e-04).

Intro, p9211, line 16: Please add a reference to Stavrakou et al., 2014.: Stavrakou, T., Müller, J.-F., Bauwens, M., De Smedt, I., Van Roozendael, M., Guenther, a., Wild, M. and Xia, X.: Isoprene emissions over Asia 1979–2012: impact of climate and land-use changes, Atmos. Chem. Phys., 14(9), 4587–4605, doi:10.5194/acp-14-4587-2014, 2014.

Intro, p9211, line 27: Please add a reference to De Smedt et al., 2015: De Smedt, I., Stavrakou, T., Hendrick, F., Danckaert, T., Vlemmix, T., Pinardi, G., Theys, N., Lerot, C., Gielen, C., Vigouroux, C., Hermans, C., Fayt, C., Veefkind, P., Müller, J.-F. and Van Roozendael, M.: Diurnal, seasonal and long-

term variations of global formaldehyde columns inferred from combined OMI and GOME-2 observations, Atmos. Chem. Phys., 15(8), 12241–12300, doi:10.5194/acpd-15-12241-2015, 2015.

Section 2.2.1, p9213, line 26: the GOME, SCIAMACHY, GOME-2 and OMI H2CO retrievals of the BIRA product are all performed in the 328.5-346 nm window. Please correct.

Section 2.2.1, p9214, Figure 1: What is shown on the figure? The correlations between the cross-sections or between the fitted SCDs along one orbit? From the second part of the legend, I would say that these are correlations between SCDs. Please clarify the legend and the text. Please specify which orbit has been used, and the step of the fitting window wavelength increment.

Section 2.2.1, p9214, line 5: In the middle and right panel of Figure 1, the increased correlation between HCHO and O3 below 328 nm is not apparent. Correlation between absorption cross-sections is not the reason why wavelengths shorter than 328nm should be avoided, but rather the increase of the ozone absorption (SCDs).

Section 2.2.1, p9214, line 7: There seems to be a confusion between the O2-O2 and molecular Ring, at least in the text, and maybe also in the Figure. The description of the correlations do not match the subplots (on Figure 1, the correlation with Ring does not increase at shorter wavelengths). Besides, why is the term “molecular Ring” used? The first order of the Ring effect is not on molecular absorption but on Fraunhofer lines.

Section 2.2.1, p9214, line 9: There is an explanation for selecting the lower limit of the fitting interval, but nothing is said about the upper limit.

Section 2.2.1, p9215, line 7: Please explain the meaning of molecular Ring.

Section 2.2.1, p9215, line 17: Please comment on the added value of using two closure polynomials instead of one (baseline and scaling, as called in table 1).

Section 2.2.1, p9216, line 11: How do you define and estimate the detection limit?

Section 2.2.2, p9216, line 18-19: please explain how gas concentrations are taken into account in AMF calculations. It is stated later that O3 profile variations are considered. Please explain how it is done.

Section 2.2.2, p9216, line 19: the surface *height and reflectivity* of the spatial pixel?

Section 2.2.2, p9217, line 20: A number is given for the variation of w(z) with wavelength (<7%). Please specify at which altitude z, or if it rather corresponds to a variation of the total AMF, and in this case, for which H2CO profile?

Section 2.2.3, p9218, line 24: please define ΔVCD. I guess ΔSCD/AMF.

Section 3, p9219, equation 7: please define m and n.

Section 3, p9220: The sensitivity test on the profile shape is not clear. What is meant by a 10% bias of the profiles? Changing the total H2CO concentration of the a priori profile should not have any impact on the AMF. But varying the only surface concentration can change the AMF by much more than 16%. Please develop.

Section 3, p9220, line 23: The final estimation of 38% is only valid for one particular observation condition, and it would be more insightful to provide a range of uncertainties, and the conditions in which the AMF errors are the largest/lowest. Alternatively, a figure showing the calculated error could be added to the figures 3 and 4, since an equation (equ.7) is provided.

Section 4, p9221, line 10: Please refer to De Smedt et al. (2015) for the BIRA OMI product.

Section 4, p9222, line 8: Please check if version 14 of the BIRA product has been used. Differences can be significant between v13 and v14 over Southern Africa.

Section 4, p9222, line 21: Considering the respective spectral and spatial resolutions of OMI and OMPS, the noise level of the OMPS retrievals should be lower than OMI. This needs to be discussed in the paper. I'm not sure that "the averaged VCDs variability considering all regions and months" give any information on the precision of the product. The variability of background values (Pacific Ocean) is generally used to estimate the precision, and should correspond to $\varepsilon_{\Delta SCD_{random}}$.

Conclusion, p9223, line 6: This sentence is in contradiction with the numbers given in section 4 (see comment just above). Besides, please consider the fact that if the detection limit numbers are weighted by the square root of the respective pixel areas (50x50 or 13x26 km²), then the OMI detection limit is twice lower than OMPS.

Conclusion, p9224, line 5: Please mention TROPOMI/S5P, to be launch in 2016.

Review by Isabelle De Smedt.