Review of Glatthor et al. “Global carbonyl sulfide (OCS) measured by MIPAS/Envisat during 2002–2012”

This paper presents an overview of the upper tropospheric/stratospheric carbonyl sulfide (OCS) between 5–40 km observed by the satellite instrument MIPAS during 2002–2012. The same dataset has been already used in a previous paper [Glatthor et al., 2015, doi: 10.1002/2015GL066293]. In this manuscript, the authors first compare the vertical OCS profiles observed by MIPAS against balloon profiles, ACE-FTS and ground-based measurements. Then the authors describe in details the spatial distribution and the temporal variation OCS, such as annual variations and interhemispheric differences. The authors also discuss the contributions of tropospheric processes such as biomass burning and surface sources/sinks to the observed variability of OCS in the upper tropospheric region.

The very detailed documentation of the spatiotemporal variability of OCS is attractive. But a rigorous statistical treatment of the OCS data, especially in the upper troposphere, is required to justify the results of the current manuscript. Below are the comments that the authors may consider during their revision.

Major comments:

1. There have not been any quantitative analysis of the retrieval characteristics. A figure like Figure 3 in Millán et al. [2015, ACP, doi:10.5194/acp-15-2889-2015] is needed to justify the sensitivity of their retrievals to OCS at different altitudes. E.g. How do the sensitivity profiles (i.e. Jacobians) and the averaging kernels look like? What is the a priori concentration? These information have not been presented in Glatthor et al. [2015] nor the current manuscript. However, these information are critical if the authors want to discuss the OCS variability in the upper troposphere or below, as to show how much of the retrieved values actually come from the a priori and the measurement.

2. The retrieval error analysis is not complete. In Section 2.2, only an estimate of the total errors in the troposphere (50 ppt) and the stratosphere (120 ppt) are presented. The authors explain that the measurement noise is the dominant error. I assume what has been taken as “total errors” in this manuscript is the sum of the “random error” and the forward model parameter errors, defined in von Clarmann et al. [2003]’s Eqs (2) and (3) respectively. von Clarmann et al’s Eqs (2) and (3) are the same as Eqs (3.30) and (3.18) of Rodgers [2000] respectively, and do not include the smoothing error (Eq. 3.29 of Rodgers [2000]), which is partly due to the deviation from the a priori and partly due to the Twomey-Tikhonov constraint. Indeed, the smoothing error was not discussed in von Clarmann et al. [2003]. In the current manuscript, the authors used a height-independent constant profile as the a priori. But the OCS concentration varies strongly with height across the tropopause. Therefore, the smoothing error due to the deviation from the a priori should depend on the vertically constant concentration they have assumed. Furthermore, the authors should also mention the error due to ambient temperature.

3. In addition to Comment #2, the 1000 micron band used in this work and Glatthor et al. [2015] for the OCS retrieval is 100 times weaker than the strongest OCS absorption band at 2040 micron that has been used by the IASI and TES teams. The OCS absorption signals at some

selected altitudes (e.g. 7 km, 10 km, 20 km, etc) should be compared to the instrument noise, in a similar way in Figure 1 of Millán et al. [2015], to illustrate that the OCS signal is strong enough for retrieval purpose.

4. The fact that the seasonal patterns are obtained with a constant a priori is quite promising but the authors should also plot the evolution of the error terms in the same way as in their Figure 5 to show that the seasonal patterns are not results of errors.

5. The comparison between MIPAS OCS and other OCS measurements are not consistent. The SPIRALE data have been convolved with the averaging kernel before comparing MIPAS whereas ACE-FTS and MkIV have not. The authors explain that it was because the vertical resolution of SPIRALE is higher. However, in additional to the degradation of the vertical resolution, the effect of the a priori in the MIPAS OCS is also applied to the SPIRALE through the averaging kernel. Therefore, the averaging kernel is applied either to all datasets or to none.

Minor comments:

1. Page 1, Line 17: Should "tropospheric OCS" be actually "upper tropospheric OCS"? The authors mostly discuss the OCS in the upper troposphere near 10 km or 250 hPa. But in the abstract (and in the text), the authors sometimes refer to "tropospheric OCS" (e.g. for the trends). The authors should clarify whether they are actually referring the upper tropospheric OCS or really tropospheric OCS, say, in the model simulations or inferred from HCN or ozone data.

2. Page 2, Line 23-24: "A comprehensive compilation of these budget estimations is given in Kremser et al. (2016)." Do you mean Kremser et al. (2015)?

3. Page 3, Line 4-5: How would Lejeune et al. [2011] discuss the trend in 2012? Please check.

4. Page 3, Line 24: Somehow the authors should also mention IASI and TES OCS products for completeness because this manuscript discusses the tropospheric OCS.

5. Page 5, Line3: What's values of a priori used by retrieval? What did you use for constraint of a priori? Could you show the profile of a priori with uncertainties you used in Figure 1?

6. Page 5, Line 15: 41–48 pptv and 10–26% cannot be both right.

7. Figure 2: Is the same a priori profile used at all locations? If not, it may be better to show the a priori profile in each panel.

8. Page 8, Line 3: Has there been any explanation why there was an increase of OCS concentration at 14 km after 2006?

9. Page 11, Line 25: The term "slower convection" is contradictory. Should it be "vertical mixing" or "upwelling"?

10. Page 13, Line 29-31, 'In the northern hemisphere there is a band of enhanced values

extending from the tropical Atlantic to the Chinese coast, which reflects the upper end of the Asian Monsoon Anticyclone including westward outflow.' Could this high OCS extending from Atlantic to China also result from Arabic anticyclone?

11. The discussion of Asian Monsoon anticyclone (AMA) signature of OCS distribution at UTLS is illuminating on the underlying transport. The authors may want to explain more clearly what mechanisms caused the pattern of enhanced OCS on the north end and low OCS on the top of AMA.