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Interactive comment on "Global carbonyl sulfide (OCS) measured by MIPAS/Envisat during 2002–2012" *by* Norbert Glatthor et al.

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Response to reviewer 1:

We thank reviewer 1 for her/his helpful comments. Please find below our responses describing how the manuscript has been modified with respect to the comments. Blue passages denote changes or updates in the revised manuscript.

1. General Comments

"Please give less focus to the uncertain uppermost levels."

Reply: Actually we do not give much focus to the uppermost levels. In the discussion of Figures 5 and 8 there are only a few sentences, in which we list some general stratospheric features visible in OCS: the QBO, upwelling in the tropics and subsidence in the polar vortices. Further, we trace back the difference between measured and modelled upwelling to the MIPAS averaging kernels. We think these stratospheric observations are robust and can be maintained.

Change: The discussion of the insignificant trends at 40 km altitude in the last two sentences of Section 4.4 has been removed (also on demand of reviewer 2).

2. Specific Comments

Comment: *"In the abstract a sentence on the model simulations is missing."*

Reply: The sentence "Simulations with the ECHAM-MESSy model reproduce the observed latitudinal cross sections fairly well." has been added.

Comment: *"In the introduction on page 3, line 10 also the recent data by NOAA available on the internet should be mentioned since they are obviously used in Figure 7."*

Reply: Thank you for pointing this out. We added the sentence "NOAA/ESRL data are available at http://www.esrl.noaa.gov/gmd/hats/gases/OCS.html." in paragraph

5 of the introduction. Further, we added an acknowledgement for provision of the NOAA/ESRL/GMD flask data.

Comment: *"Near the end of section 2.2 it should be explicitly said that the averaging kernels cause a significant high bias in the layers above about 30km."*

Reply: As suggested by reviewer 2 a plot of the averaging kernels has been added to Figure 1. In the discussion of this plot we now state that "In the stratosphere the AKs are centered at increasingly lower altitudes with a displacement of up to 2 km at 40 km altitude. This indicates that the OCS signal is actually from somewhat further below and that symmetrical averaging kernels would lead to lower OCS values."

Comment: *"It might be dangerous to interprete the sparse data in the upper troposphere in the tropics in detail because of possible biases due to longitudinal features like for example more clouds above the West Pacific warmpool (sections 4.1 and 4.2.2., first paragraphs). Please add more information here."*

Reply: To account for the reviewer's concerns, we added the sentence "Measurements at 10 km altitude above Indonesia and the western Pacific warmpool are nearly permanently impeded by clouds, while cloud contamination in the remaining tropical latitude band exhibits a moderate sesonal variation." However, in our discussion of tropical upper tropospheric data in sections 4.1 and 4.2.2 there are no conclusions on strong interannual OCS differences, which might be biased by varying cloud coverage.

Comment: *"The Montzka data cited at the beginning of section 4.2.1 do not represent a zonal mean in Northern midlatitudes, but are low biased because most stations are located near the Eastcoast, where the influence of the uptake by vegetation and soil is*

strongest because of the prevailing winds."

Reply: The reviewer is right, but there has already been an explanation for this low bias and the discrepancy to upper tropospheric zonal means in section 4.2.1 of the discussion paper. To make things clearer, we changed the wording into "The largest deviations occur at two ground-based stations at northern mid-latitudes, where the OCS amounts are lower by 50 pptv. These stations are located in the central (LEF, 45.9°N, 90.3 °W) and eastern United States (HFM, 42.5°N, 72.2 °W), where the OCS amounts are strongly reduced by vegetative uptake during the growing season (Montzka et al., 2007)."

Comment: *"Please cite the comparison of the OCS data with EMAC in a different setup in Brühl et al (2015, J. Geophys. Res. Atmos. 120, 2103) in section 4.2.2 (third paragraph)."*

Reply: The sentence "A comparison of MIPAS OCS with EMAC simulations from a different setup has been presented in Brühl et al. (2015)." has been added in section 4.2.2 (third paragraph).

Comment: *"Does the version in the manuscript contain a flux boundary condition for OCS?"*

Reply: Yes, it does. To point this out the sentence "In the EMAC simulations presented here, monthly varying OCS emissions taken from the scenario of Kettle et al. (2002) and modified by enhancement of the tropical vegetation uptake, are applied as flux boundary conditions." has been added to paragraph 3 of Section 4.2.2 of the revised manuscript.

Comment: *"Please reformulate paragraph 4 of section 4.2.2: There is no stronger upwelling in the MIPAS data compared to the model, this is just an artifact introduced by the averaging kernels as demonstrated by the third column of Figure 8. Here it is nice to see that the convolution with the model results even reproduces the negatives. This figure is a very valuable contribution for modelers using satellite data."*

Reply: The fact that the apparent stronger upwelling in the MIPAS data is an artefact caused by the averaging kernels has already been outlined in the discussion paper. To make things even clearer we changed the passage "generally exhibit a stronger upwelling" into "apparently exhibit a stronger upwelling" and rephrased the subsequent sentences as follows:

"However, convolution of the EMAC data with the averaging kernels of a MIPAS OCS profile obtained in the tropics (right column) leads to much better agreement in the tropical upper stratosphere and to somewhat better agreement in the northern hemispheric troposphere and in the transition region between 15 and 25 km altitude. Thus the apparent stronger upwelling of measured OCS in the tropical upper stratosphere is an artefact caused by the displaced averaging kernels in this region (cf. Section 2.2)."

Comment: *"For comparison with MIPAS is would be more useful to keep the mountain stations and give the 3 Eastcoast stations a lower weight by just using their average. These numbers should be given in addition in the text of section 4.2.3."*

Reply: Actually, as taken from Montzka et al. (2007), the northern hemispheric ratio has alread been given even for complete omission of the Midwest and Eastcoast station (1.0) at the end of Section 4.2.3.

Additional point: after having re-read the Montzka-reference, we noticed that our statement "In this estimation all northern hemispheric sites situated at more than 3000 m above sea level had been excluded." was wrong and replaced it by "In this estimation the results from the stations THD, MHD and SUM had been excluded, because they did not cover the whole measurement period.".

Comment: *"The seasonal cycle of OCS observed by MIPAS in Figure 9 (section 4.2.4) in high and midlatitudes is influenced by the polar vortices which should be mentioned. In the Southern hemisphere this effect might by reduced by a sampling artifact in MIPAS data because vortex airmasses have PSCs and therefore data gaps. It would be useful to add two frames in Figure 9 with corresponding model results to disentangle dynamical and surface effects."*

Reply: We added the sentence "Of course, other processes, as e.g. subsidence of OCS-poor air masses in the polar vortices, can also contribute to the differences between the variations at the ground stations and observed by MIPAS at higher northern latitudes. Nevertheless, ..." We decided not to show model results because they do not help to explain the differences between the ground stations and MIPAS observations.

Comment: *"Section 4.3.2 should be shortened. It is not necessary to use 2 figures and a table to demonstrate that there is no correlation, at least Figure 11 is superfluous in the main text (skip and replace by a sentence or move to a supplement)."*

Reply: We agree. Section 4.3.2 has been shortened by removal of Figure 11 and of the respective discussion on page 12, lines 14–21 in the ACPD-version of the manuscript.

Comment: *"Please improve text at the beginning of section 4.3.3. Isn't that tongue due to the Northern part of the AMA circulation?"*

Reply: We are not quite sure what the reviewer means here. At the beginning of section 4.3.3 we do not talk about a "tongue", but particularly discuss enhanced OCS amounts at 150 hPa in the AMA. We talk about a tongue of enhanced ozone in section 4.3.4, which like the region of depleted OCS is attributed to meridional transport of extra-tropical air around the AMA.

Comment: *"The discontinuity at the time of the switch between the data versions should be addressed in the text of section 4.4 or the caption of Figure 13. A continuous line would be not in contradiction to the Montzka data and better fit the Jungfraujoch data."*

Reply: We adressed the discontinuity by adding the sentence "As outlined in Section 4.1 there is a discontinuity at or somewhat after the switch from the HR to the RR mode. This is taken into account by an offset as additional fit parameter, resulting in biases of 5 and 14 pptv in the two examples, respectively."

Fitting of a continuous line over both measurement periods would probably lead to moderate positive trends in the upper troposphere, which would be in agreement with the Jungfraujoch data between 2002 and 2008, but in contradiction to the Jungfraujoch data after 2008 and also to the Montzka data. Further, we found discontinuities between HR and RR mode data for several other MIPAS gases and have performed trend analyses for a couple of species by taking into account an offset between both periods (see e.g. Kellmann et al. (2012) or Eckert et al. (2014) in the reference list).

Comment: *"Does the analysis in Figure 14 also include the jump in 2004? Figure 14 points to a kind of redistribution or an oscillation of circulation patterns. Please skip the sentences on the insignificant and uncertain upper layers."*

Reply: The analysis in Figure 14 contains both measurement periods, i.e. also the jump in 2004. Yes, as outlined in our manuscript we also assume that Figure 14 points to a change - most probably a southward shift - of the Brewer-Dobson circulation. The last two sentences of Section 4.4 on the less significant trends in the tropical stratosphere above 40 km altitude and at high southern latitudes between 14 and 20 km have been skipped.

Comment: *"The ticks on the time axis in Figure 5 should be thicker and longer."*

Reply: Figure 5 was modified accordingly.

Comment: *"Include "i.e. on mountains" in caption of Figure 7."*

Reply: We changed the respective sentence into "Surface stations indicated by red stars are situated at low altitudes in the boundary layer and surface stations indicated by blue stars on mountains in the free troposphere."