Reply to comments of Reviewer #1

We thank the reviewer for carefully reading the manuscript and his constructive and helpful comments and suggestions. They helped us to improve the paper in several aspects. We considered them point by point as illustrated below. We like to remark that line numbers mentioned in the reviewers comments refer to the first submission of the paper. We re-run the retrieval code to simulate OH radiances as measured by SABER from SCIAMACHY spectra and corrected radiance contamination to the previous unfiltered 1.6 μm simulations from other band emission lines due to the selected wavelength range. This problem was found when we run the retrieval using Einstein coefficients obtained by van der Loo and Groenenboom [2007, 2008] as suggested by the reviewer #2. The abstract was rephrased to make it more clear. Following the reviewer #3, we also simulated the in-band data as measured by SABER without considering the filter transmission effect for comparison.

Specific Comments*:*

Line 5*: "OH 1.6 μm and 2.0 μm radiances as measured by SABER were retrieved from OH limb measurements recorded by SCIAMACHY"*
This sentence is somewhat misleading, particularly the "retrieved from". In your study, OH concentration profiles were retrieved from SCIAMACHY limb measurements and these concentrations were then used to "simulate" SABER measurements, right? I suggest rephrasing the sentence - right now the sentence also suggests that SCIAMACHY measurements around 2.0 micron were used, which is not the case.
Reply: Following the reviewers' suggestion, we rephrased the abstract to make it more clear and to be well understood. Please refer to the abstract for details.

Line 7*: "Systematic deviations of up to 88% were found"*
In my opinion, the abstract is too negative and not representative of the obtained results. Only the large differences are mentioned. However, the mean differences are on the order of 10% for the 2.0 micron channel and 35% for the 1.6 micron channel. I suggest mentioning this as well.
Reply: We agree with the reviewer's suggestion. The mean difference is calculated using the formula:

$$\sum^{N} \frac{Radiance_{saber} - Radiance_{scia}}{Radiance_{scia}} / N$$

The average differences are on the order of 20% for the 2.0 μm channel and 40% for the 1.6 μm channel. The detailed description is here:

On average, SABER "unfiltered" data is on the order of 40% at 1.6 μm and 20% at 2.0 μm larger than the simulations using SCIAMACHY data.

Line 46: *"absolute volume radiances"*
Do you mean volume emission rates? To my knowledge "volume radiance" is not standard terminology. Radiance (usually) has the units: photons/s/m^2/sr (and /nm in case of "spectral radiance")
Reply: Thanks for the reviewer pointing out this issue. We changed "volume radiance" to "spectrally averaged radiances at 1.6 μm and 2.0 μm as measured by SABER".

Figure 1: *y-axis label: is this really a "radiance"? Are the units correct? Radiance should also include a solid angle dependence, right? I assume this should be "volume emission rate"?*
I also suggest mentioning in the figure caption, whether these are modelled or measured spectra.
Reply: Yes, the unit is "volume emission rate", not "radiance". The label has been changed to "VER". The spectra are modelled, which is also mentioned in the caption.

Line 59: *"In this study, only the spectral range of channel 6 up to 1650 nm"*
I think you only used wavelengths up to about 1600 nm. Channel 6+ (having a different detector material) starts shortly below 1600 nm. This would also be consistent with the shading in Figure 1.
Reply: We only use the spectral range of channel 6 up to about 1589 nm. This was corrected accordingly in the main text and in Figure 1.

Line 86: *"monthly zonal median data"*
The median was probably determined for each altitude separately? Was there a specific reason to use the median rather than the mean?
Reply: Yes, the median was determined separately for each altitude. There was no specific reason for using the median rather than the mean, because we tested the median and mean spectra before and found no big differences between them.

Line 96: *"by dividing the corresponding Einstein"→ "by dividing BY the corresponding Einstein"*
Reply: Corrected.

Line 102: *"Boltzmann factor"→"Boltzmann constant"?*
Reply: Corrected.

Line 107: *"the SCIAMACHY OH limb measurements can be expressed as"*
The SCIAMACHY OH limb measurements can also be expressed in this form

without the 2 conditions mentioned in the first part of the sentence. I suggest just stating that the SCIAMACHY measurements can be expressed in this form and that the two assumptions are made.
Reply: Following the reviewer's suggestion, the sentence was rephrased as below:
The SCIAMACHY OH limb measurements can be expressed as

$$\boldsymbol{y} = \boldsymbol{F}(\boldsymbol{x}, \boldsymbol{b}) + \boldsymbol{\epsilon}$$

In our setup we assume that each atmospheric layer emits OH airglow homogeneously, and we set the retrieval grid to be identical to the tangent altitude grid of the averaged OH limb measurements.

Line 109: *"measured SCIAMACHY OH limb spectra measured."*
Please delete one of the "measured"
Reply: Done.

Line 112: *"of interested properties"*
I suggest to replace this by "properties of interest". If the properties are interested in the retrievals, we dont know :)
Reply: Yes, we definitely agree :). It is corrected.

Line 113: *"In general, the inverse problem is ill-conditioned"*
This is only minor point and Im not asking for changes, but in your case, with the retrieval altitude grid being identical with the tangent height grid, the inverse problem is not ill-posed in the sense that there are more unknowns than knowns, right?
Reply: A well-posed problem should meet the three Hadamard criteria: 1. Having a solution; 2. Having a unique solution; 3. Having a solution that depends continuously on the parameters or input data. An ill-posed problem is the one which does not meet at least one of the criteria. In our case, we do the retrieval from the SCIAMACHY limb spectra and there are more knowns (sampling points) than the unknowns, but the solution is not unique. That is why we call it ill-conditioned.

Line 114: *"inverse issue"→"inverse problem"?*
Reply: Corrected.

Figure 3: *Perhaps colors can be used to highlight the 2 and 1.6 micron profiles? The symbols are quite small and difficult to identify.*
Related to Figure 3: I think it is also worthwhile to show some sample OH(v) concentration profiles. They are the intermediate data product linking the SCIAMACHY and SABER measurements and are, therefore, quite important for this study.

Reply: The colors are used to highlight the 2 and 1.6 micron profiles. As suggested by the reviewer, corresponding OH(v) concentration profiles are added in Figure 3.
A sentence is added in the main text:
Corresponding OH number density profiles as derived using Einstein coefficients from the HITRAN database at vibrational states 9, 8, 5, and 4 are also given.
A sentence is added in the caption of Figure 3:
Corresponding retrieved OH number densities of vibrational states 9, 8, 5, and 4 from SCIAMACHY data using HITRAN database (right).

Line 159/160: *I suggest mentioning explicitly what reference profile was used to determine the relative differences. In case of large differences, this choice of reference will be important.*
Reply: The reference data are simulations from SCIAMACHY data. The sentences are modified:
For the "unfiltered" data, deviations of SABER OH 1.6 μm measurements with respect to the corresponding simulations increase with altitude from 30-45% at 83 km to 55-80% at 96 km, depending on latitudes. The difference of SABER OH 2.0 μm measurements with respect to the corresponding simulations is 16% at 86 km.

Line 161: *"It was also found the positive deviations of SABER"*
Please rephrase, something is wrong here.
Same sentence and Fig. 4: I suggest discussing the difference between in-band and unfiltered SABER data in a few additional sentences. It took me a while to figure out whats shown in Figure 4.
Reply: This sentence was deleted and Figure 4 has been updated by adding a comparison of SABER in-band data and corresponding simulations using SCIAMACHY data. The difference between SABER in-band and unfiltered data has been discussed in Line 84-89. To make it more clear, some sentences are added in the main text.
The top two plots show a comparison of the "unfiltered" data and the bottom two figures show the in-band data.

Line 165: *"A strong annual oscillation was found over the equator region in April"*
"A strong annual variation ... in April" doesnt really make sense, does it. You mean an annual variation with a maximum in April, I guess? There is also a semi-annual component in your figures, as, e.g. also clearly seen in Teiser & v. Savigny, JASTP, 161, 28-42, 2017.

Reply: Thanks for the reviewer pointing out this problem. The sentence was rephrased.

A strong annual variation with a maximum in April and a semi-annual oscillation are visible in the radiance data over the equator region, as it was also found by Teiser and von Savigny [2017] in a study of SCIAMACHY OH(3-1) and OH(6-2) volume emission rates.

General comment: *Another general comment on the comparison of SCIAMACHY and SABER profiles: The SCIAMACHY and SABER volume emission rate profiles have different vertical resolutions. SCIAMACHY has a vertical resolution of about 4 km, SABER rather 2 km. The potential effects of this difference should be discussed, too. Im not asking for more simulations etc., but only a qualitative discussion of the expected effects on the comparisons and the agreement.*

Reply: As pointed out by the reviewer, SCIAMACHY has a vertical resolution about 3.3 km and the vertical resolution of SABER is around 2 km. A linear interpolation has been performed to the SABER data for the purpose of making a comparison with SCIAMACHY data. This means that we may underestimate the SABER data at peak altitudes and overestimate the data at two wings besides the peak altitudes. A discussion is given in the first paragraph of section 4.2:

It should be noted that SCIAMACHY and SABER have a resolution of about 3.3 km and 2 km, respectively. A linear interpolation has been applied to SABER data to make a comparison with SCIAMACHY data. This may underestimate the SABER data at peak altitudes and overestimate the SABER data at two wings besides the peak altitudes.

Figure 7: *y-axis label and caption: "slop" → "slope"*

Reply: Corrected.

References

Georg Teiser and Christian von Savigny. Variability of OH(3-1) and OH(6-2) emission altitude and volume emission rate from 2003 to 2011. *J. Atmos. Sol.-Terr. Phys.*, 161:28 – 42, 2017. ISSN 1364-6826. doi: https://doi.org/10.1016/j.jastp.2017.04.010. URL `http://www.sciencedirect.com/science/article/pii/S1364682616303364`.

Mark P. J. van der Loo and Gerrit C. Groenenboom. Theoretical transition probabilities for the OH Meinel system. *J. Chem. Phys.*, 126(11):114314, 2007. doi: http://dx.doi.org/10.1063/1.2646859. URL `http://scitation.aip.org/content/aip/journal/jcp/126/11/10.1063/1.2646859`.

Mark P. J. van der Loo and Gerrit C. Groenenboom. Erratum: Theoretical transition probabilities for the OH Meinel system [J. Chem. Phys. 126, 114314 (2007)]. *J. Chem. Phys.*, 128(15):159902, 2008. doi: 10.1063/1.2899016. URL `https://doi.org/10.1063/1.2899016`.