Reply to comments of Reviewer #3

We thank the reviewer for carefully reading the manuscript and his/ her 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.

General comments*:*

1)*: Although the differences found between the instruments are important themselves, the authors should expand the laconic sentence that "the differences may be explained by the radiometric calibration of both instruments". I believe both instruments have been well characterized and estimates of their errors been given. The authors should discuss if the differences found are or not within the calibration errors of the instruments. Could they attribute the differences to a given instrument? e.g. on the basis that the calibration errors are smaller for one instrument than for the other?*
Reply: The observed accuracy of SABER 1.6 μm and 2.0 μm channels is about 3% at 80-90 km and about 20% at 90-100 km ("SABER Instrument Performance and Measurement Requirements" published on http://saber.gats-inc.com/overview.php). The propagated radiometric uncertainty of SCIAMACHY channel 6 is about 1.2% [Zoutman et al., 2000]. Both instruments have been well characterized. The differences of SABER data and the simulations from SCIAMACHY data are not within combined calibration errors of two instruments. However, we simulated OH radiances as measured by SABER using Einstein coefficients calculated by van der Loo and Groenenboom [2007, 2008] as recommended by the reviewer #2 and found that the uncertainty resulted from different datasets of Einstein coefficient could potentially explain the large differences of SABER data and the simulations from SCIAMACHY data.

2)*: I would also like the authors comment on the expected impact of these differences on the retrieved atomic oxygen from both datasets. Can the* *current O differences discussed in the introduction be explained by these differences? Would they reconcile the O differences or, on the other hand, would they still support or even enlarge the O differences?*

Reply: We have analyzed the impact of these differences on the retrieved atomic oxygen from the two datasets and found these radiance differences almost reconcile the retrieved atomic oxygen differences, especially at low altitudes. A paragraph is added at the end of the text:

The OH 2.0 μm data measured by SABER and O(^{1}S) green line emission and OH(9-6) nightglow observed by SCIAMACHY were used in the past to obtain atomic oxygen abundances. Significant differences in atomic oxygen absolute values were reported [Kaufmann et al., 2014, Mlynczak et al., 2018, Zhu and Kaufmann, 2018]. These differences are of similar magnitude as uncertainties in the Einstein coefficients and other model parameters used in the retrieval of those data.

Minor comments and suggested minor changes and typos*:*

Line 8*: Worth to state here already which one (SABER/SCIA) is larger/smaller.*
Reply: Stated.

Line 30*: Just over 1+1/2 solar cycles. Worth mentioning it is still measuring.*
Reply: A sentence is added:
covering one and half solar cycles, and is still measuring

Lines 44-48*: Consider some merging. Some information is somehow duplicated*
Reply: The duplicated sentence in line 44-45 was deleted.

Legend of Fig. 1*: Include that they are "simulated radiances".*
Reply: "Simulated" is included.

Line 56*: Delete "between two adjacent tangent heights". It is redundant.*
Reply: Deleted.

Line 57*: from vibrational states 2 to 9 at → from UPPER vibrational states in the range of 2 to 9 at ...*
Reply: Corrected.

Line 58*: Bring all that information (particularly the radiometric calibration) to the discussion on the reason of the differences in Sec. 4.2 below.*
Reply: Done.

Line 62*: Start new paragraph with "The SABER ..."*
Reply: Done.

Lines 73-76: *Consider remove or re-write and merge with the next paragraph the sentence "The SCIAMACHY channel ... for SABER".*
Reply: The sentence was merged with the next paragraph.

Legend of Fig. 2: *Explain the meaning of the "fit" and "raw" symbols in the legend, so the reader do not have to read the text for understanding it.*
Reply: An explanation is given in the caption of Figure 2.
raw: the raw limb spectra measured by SCIAMACHY; fit: simulated limb spectra as measured by SCIAMACHY from retrieval results.

Line 80: *Mention here that rotational non-LTE is considered, and that it will be assessed later in the paper.*
Reply: It is mentioned in the text.
details are discussed later.

Near the end of page 4: *About the method used when comparing SCIA and SABER radiances. Although probably the effects are small, I think it is more consistent to apply the SABER filters to the computed SCIA spectra and compared directly to the measured SABER radiances. In the way it has been done, by comparing with the "unfiltered" SABER radiances, the authors rely on the method used in SABER for unfiltering the radiances, e.g., in an OH model, which might be different, from that used in the retrieved OH radiances from SCIA.*
Reply: We agree with the reviewer for applying the SABER filters to the simulated radiance data from SCIAMACHY spectra. We applied bandpass filters of SABER 1.6 μm and 2.0 μm channels to the simulated OH radiances from SCIAMACHY measurements, including contributions of OH(7-5) and OH(3-1) emissions. Corresponding discussions are given in the text.

Line 86: *monthly zonal MEAN?*
Reply: We have tested monthly zonal median and mean spectra before and found no big differences between them.

Line 97: *... dividing BY ..*
Reply: Corrected.

Line 97: *State that this equation is valid under the assumption of rotational LTE. And that rotational NLTE is considered later. Should not E_v be actually $E_{rotational}$ in Eq. 1?*
Reply: A statement is added as below:
This formula is only valid under the rotational local thermodynamic equilibrium (LTE) condition and deviations are discussed later.

Here, $E_v(i)$ represents the rotational energy of the upper rotational state of the ith line and is equivalent to the $E_{(v,J)}(i)$.

Line 111: *"b" not only includes the Einstein coefficients, but also the other factors accompanying* n_v *in Eq. 1.*
Reply: Yes, i.e. → **e.g.**

Line 114: *issue → problem? Full stop → comma? Consider re-writing.*
Reply: re-written. **inverse issue** → **inversion problem**; Full stop → comma

Line 126: *median → MEAN?*
Reply: Yes, here is "mean".

Line 128: *I believe the estimated effects of the temperature errors include not only the effects of temperature on the Einstein coefficient but also on the rotational populations (see Eq. 1).*
Reply: Yes, we agree with the reviewer. According to Equation 1, the change of temperature will redistribute the rotational populations first, and then affects the band-average Einstein coefficient. The effects of temperature on the band-averaged Einstein coefficient and on the rotational populations are not independent with each other. Both of them have been considered in the estimated effects.

Line 132: *Probably worth to be clarified that the Einstein coefficient errors enter not only through the retrieval of SCIA, as it is described in the paragraph above, but also through the A of the transitions of the SABER measured bands (which was not described in the paragraphs above).*
Reply: The estimation of the Einstein coefficient errors in the text have considered these two aspects: one enters through the retrieval of SCIAMACHY data; another one enters through the simulation when using the Einstein coefficient of the SABER measured bands. A sentence is added in the text;
The uncertainty of the Einstein coefficient affects simulated VERs in two ways: In the retrieval of vibrationally excited OH from SCIAMACHY data and in the simulation of the SABER measurements.

Lines 138-139: *Consider rewriting this sentence (somehow redundant with previous one).*
Reply: The sentence is rephrased:
Therefore, we used these results as a proxy to estimate related uncertainties of the Einstein coefficients.

Line 165: *"A strong annual oscillation..." In the radiances or in the differences?*

Reply: It is the radiance and specified:
A strong annual variation with a maximum in April and a semi-annual oscillation were found 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.

Line 169: *Delete "here".*
Reply: Deleted.

Fig. 4: *I would suggest to use different line styles for SCIA and SABER? and/or use larger symbols.*
Reply: Different line styles were used for SCIAMACHY and SABER data.

Lines 187-189: *Please see my two major points above.*
Reply: Considered.

Fig. 6 Right panel: *The error of "b" is 0.00. Please, revise it.*
Reply: Revised.

Fig. 7: *Slop → slope? Both, in the legend and in the y-axis label. Is there any reason for the sudden drop in the slope in 2010? Please comment on it.*
Reply: The word "slope" is correct. The sudden drop in the slope in 2010 results from very discrete fitting data points in 2010 for SABER 1.6 μm channel by comparing to other years.

References

M. Kaufmann, Y. Zhu, M. Ern, and M. Riese. Global distribution of atomic oxygen in the mesopause region as derived from SCIAMACHY $O(^1S)$ green line measurements. *Geophys. Res. Lett.*, 41(17):6274–6280, 2014. ISSN 1944-8007. doi: 10.1002/2014GL060574. URL `http://dx.doi.org/10.1002/2014GL060574`.

M. G. Mlynczak, Hunt L. A., J. M. Russell III, and B. T. Marshall. Updated SABER night atomic oxygen and implications for SABER ozone and atomic hydrogen. *Geophys. Res. Lett.*, 45(11):5735–5741, 2018.

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`.

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