

Interactive comment on “A comparison of OH nightglow volume emission rates as measured by SCIAMACHY and SABER” by Yajun Zhu et al.

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General comments

This is a generally well written manuscript dealing with the comparison of OH nightglow emission rates obtained from SABER and SCIAMACHY limb measurements. These satellite measurements cannot be directly compared, because the spectral regions covered by the two instruments do not fully overlap. Instead, the spectrally resolved SCIAMACHY measurements are used to retrieve the concentration of OH in the relevant vibrational states. These concentration profiles are then used to forward model the OH emission rate profiles as measured by SABER. The results are of interest to the aeronomy/airglow community and I recommend publication of the paper subject to

C1

(mainly) minor corrections. I ask the authors to consider the comments and suggestions for improvements as described below.

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.

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.

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 / m² / sr (and / nm in case of “spectral radiance”).

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.

Line 59: “In this study, only the spectral range of channel 6 up to 1650 nm“

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I think you only used wavelenghts 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.

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?

Line 96: "by dividing the corresponding Einstein" -> "by dividing BY the corresponding Einstein"

Line 102: "Boltzmann factor" -> "Boltzmann constant" ?

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.

Line 109: "measured SCIAMACHY OH limb spectra measured."

Please delete one of the "measured"

Line 112: "of interested properties"

I suggest to replace this by "properties of interest". If the properties are interested in the retrievals, we don't know :)

Line 113: "In general, the inverse problem is ill-conditioned"

This is only minor point and I'm 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?

Line 114: "inverse issue" -> "inverse problem" ?

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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, quit important for this study.

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.

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 what's shown in Figure 4.

Line 165: "A strong annual oscillation was found over the equator region in April"

"A strong annual variation .. in April" doesn't 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.

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. I'm not asking for more simulations etc., but only a qualitative discussion of the expected effects on the comparisons and the agreement.

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

C4

