

Review of manuscript amt-2019-328

General Comments

This manuscript reports a comparison of nightglow OH Meinel band emission observations obtained by the SCHIAMACHY and SABER instruments on the Envisat and TIMED satellites, respectively. Several attempts in the literature to retrieve O-atom density profiles from OH Meinel band emissions have yielded inconsistent results. This situation appears to have improved recently, but significant discrepancies persist without a clear explanation. These retrievals depend on the assumed model for OH collisional relaxation and the associated kinetics parameters, the OH radiative transition probabilities (Einstein coefficients A), as well as the calibration of the observing instruments. The manuscript attempts to shed some light into this problem by comparing coincident measurements from the two instruments, thus removing the role of the OH kinetics model for production and loss processes and examining the possible influence of the other inputs. Establishing techniques to assess data from different space-based instruments that cannot be directly compared—as is the case here—is an important consistency check to better understand previous work and improve future analysis efforts.

The approach of the manuscript is sound overall. It is suitable for publication in Atmospheric Measurement Techniques. Below I will raise some concerns for the authors to consider. Depending on their findings, some additional analysis checks may be helpful, but at least some additional discussion is needed.

Specific Comments

Abstract: When reading the abstract for the first time, some parts are rather confusing. The meaning of the first three sentences is not clear and one needs to read the manuscript further for the abstract to make more sense. Somehow the references to the words “as measured” do not help and the second sentence “on the retrieval model to perform an inversion of OH(v) number densities in order to simulate OH ro-vibrational emission radiances using a non-linear regularized global fit technique” only complicates things further. The abstract needs to be well understood by itself and without referring to the rest of the manuscript. It must be clear to the readers that there are two sets of coincident / co-located measurements by these space-based instruments and one set is used to simulate the other and compare with it.

It seems to me the abstract (and possibly the manuscript in general) understates the uncertainties of the Einstein coefficients and exaggerates the observed deviations between the SCHIAMACHY observations and SABER simulations. Additional comments will be discussed below.

Main Manuscript:

- I am somewhat concerned that the truncation of the “overpopulated higher rotational levels” of the SCHIAMACHY spectra introduces systematic errors in this analysis. Non-local thermodynamic equilibrium conditions imply that we are dealing with a distribution that is not a true Boltzmann distribution. This applies to all rotational levels, including the ones with low rotational quantum numbers. For the lowest OH vibrational levels, it appears this effect is less significant and may be neglected in many cases, but the deviations become increasingly important for the highest vibrational levels and can, for example, lead to significant errors in the determination of the rotational temperatures. There is a sentence mentioning the authors performed a check of the effect of non-local thermodynamic equilibrium conditions and found it to be approximately 2%. It would be helpful to reconsider these checks and whether additional assessment is needed, and also provide some brief information on what these checks entailed. Moreover, it seems the SABER transmission windows include additional lines from other bands, e.g., there are a few lines of the 7-5 band in the 2.0-micron window. By neglecting these lines another systematic error is introduced, once again effectively “underestimating” the SCHIAMACHY measurements. Verifying that the above effects do not introduce significant bias in the simulation procedure and explicitly stating it would strengthen the manuscript.
- I find it rather difficult to accept the notion that the role of Einstein coefficients introducing bias is not significant. The fact that two rather similar sets of Einstein A coefficients were used provides some idea as to what differences can be expected, but the possibility of significant absolute systematic errors introduced by the coefficients cannot be excluded. The conversion of SCHIAMACHY observations to SABER simulations essentially depends on the ratio of the respective Einstein coefficients for the vibrational levels of interest, e.g., the ratios $A(9-6)/A(9-7)$ and $A(8-5)/A(8-6)$ for the SABER 2.0-micron channel. The values of these ratios for the HITRAN coefficient set are ~13% larger than those of Brooke et al., but approximately 26% larger than the Einstein coefficient set of van der Loo and Groenenboom (2007, 2008). With older A coefficient sets, the discrepancies can be much larger, but it is quite reasonable to consider that these three most recent Einstein coefficient sets are the most appropriate choices available.
- Figure 1 displays the region of overlap between the two instruments (note: it is difficult to view the shaded area, please modify this figure to make it legible). Would it be meaningful to make another comparison by using the overlapping spectral region of the 4-2 band to simulate the SABER 1.6-micron channel? If the fraction of the 4-2 band that

is covered is substantial, then such a comparison would be direct to some extent and would rely less on Einstein coefficients. Maybe testing one or two examples would clarify whether there is anything meaningful to be learned and any additional effort is warranted.

Technical Corrections

- Line 22: “imaged” or “monitored” may be more appropriate verb choices rather than “captured”
- Lines 112, 113, 114: “inverse” or “inversion”?
- Lines 162-163: “It was also found the positive deviations of SABER in-band data from the simulated values, especially for OH 2.0 μm data.” Something is missing in this phrase.
- Line 167: “than the corresponding” instead of “than corresponding
- Figure 7: “slope” instead of “slop” in the label for the y axis and “slope” instead of “slops” in the caption
- Line 211: reference pages are missing
- Line 244: “Astrophys. J.” This is the only journal that appears as an abbreviation (the preferred AMT format).
- Line 271: “Astrophysics” instead of “Atrophysics” (but still needs abbreviated title).