

Atmos. Chem. Phys. Discuss., referee comment RC1  
<https://doi.org/10.5194/acp-2022-528-RC1>, 2022  
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## Comment on acp-2022-528

Christian von Savigny (Referee)

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Referee comment on "Hydroxyl airglow observations for investigating atmospheric dynamics: results and challenges" by Sabine Wüst et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-528-RC1>, 2022

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### General comments:

This is a generally well written paper on the exploitation of measurements of the OH Meinel airglow emissions with a focus on studying dynamical processes in the mesopause region. The paper does not really contain any new science results, but rather has the character of a review paper. In my opinion it is a useful contribution to the field, because it provides background information that is usually not included in that detail in more specific papers.

I have one general comment: I'm a bit surprised that the dependence of derived rotational temperatures on the vibrational excitation (works by Noll et al.) is not really mentioned in this paper. The paper discusses several potential issues regarding the measurement of temperatures from the OH emissions and this important aspect is not addressed. I suggest adding a brief discussion on the current level of understanding of the dependence of retrieved temperatures on the vibrational level.

Below I offer some comments and suggestions for improvements for the authors to consider.

### Specific comments:

This is only minor comment, but the abstract appears a bit short. All relevant topics seem

to be covered, but perhaps you want to work out the abstract a bit more?

Line 23: "At present, nocturnal hydroxyl (OH\*) airglow measurements are performed from the ground, aircraft, balloon and space."

There were also some ship-based measurements (on Polarstern) by the group of the author, right? Perhaps this should be mentioned, too. In addition, I'm not sure, if the tense of the sentence is correct. These measurements are not all performed right now or these days, right? I guess, you want to express that these measurement platforms have been used in the past?

Line 24: "1950's" -> "1950s"

Line 25: "More recently, satellite-based and airborne measurements have been carried out that are not severely affected by clouds in most cases."

Well, satellite measurements are not affected by clouds at all, right?

Line 35: "Some of the transitions with low rotational quantum numbers are approximately in local thermodynamic equilibrium"

Wouldn't we say that the rotational population is in LTE, rather than stating that the transitions are in LTE?

Line 50 (and a couple of other lines): "Von Savigny" -> "von Savigny"

Lines 81 – 85: I suggest also citing the corresponding instrument papers (i.e. Russell et al., Llewellyn et al. (2004) (Llewellyn et al., Can. J. Phys. (2004) is probably better than Llewellyn et al. (2003), see below), Waters et al.)

Line 89: „the main points“ -> „the main processes“ ?

Line 113: "Lower levels are populated in a radiative cascade by spontaneous emission"

or by collisional relaxation?

Line 138: "The produced OH\* has a very short lifetime,"

Perhaps you can provide a number here? Some people may think that you mean typical lifetimes on the order of  $10^{-10}$  s or so.

Same line: "it takes less than 1 s before it relaxes through collision"

I suggest explicitly mentioning a typical value of the collisional frequency at around 87 km altitude.

Line 148: One might also cite von Savigny & Lednytskyy (GRL, 40(21), 5821 – 5825, 2013) providing experimental evidence on the importance of O.

Figure 1: I suggest mentioning in the figure caption that a) shows nighttime conditions

Line 193: "A semi-annual cycle is also often found"

At low latitudes the semi-annual variation is dominant and there is no doubt it is present! See, e.g. von Savigny & Lednytskyy (2013) and also many other studies on this topics. But I completely agree that the interpretation problem is present at mid and high latitudes.

Figure 2: I hadn't heard about these factors before and didn't understand from the explanations in this paper what they exactly are. Please explain in more detail, if possible. Why are they negative?

Line 240: „... the relation .. need to be ..“ -> „... the relation .. needs to be ..“

Line 242: „In general, the lowest rotational transitions of the lower OH\* vibrational transitions are sufficiently close to LTE“

Same question as above: are the transitions in LTE or rather the rotational population?

Line 249: „two primed variables“ -> „double primed variables“ ? But the native speakers among the authors will know this better than I do.

Line 252: „If LTE holds for the rotational transitions“ -> “If LTE holds for the rotational populations” ?

Equation (8): again, only a minor point, but if the rotational population is in LTE, then  $T_{rot} = T_{kin}$ . Perhaps this can be mentioned.

Line 275: „In many cases, three or more rotational lines are used, which makes the fit more robust, and provides an immediate measure of the uncertainty of the temperature retrieved (e.g., French et al. (2000), Sigernes et al. (2003)).“

The problem with the non-LTE population of the higher rotational states could be mentioned here, as it affects the derived temperatures.

Line 283: „Therefore, the free spectral range is larger than for the Q- and R-branches“

What does „free spectral range“ mean here exactly? I only know this expression in the context of transmission functions of, e.g. etalons.

Line 298: „Comparison of result“ -> „Comparison of results“

Line 326 and line 329: „based on a single detector cells“ -> „based on a single detector cell“

Line 327: „The size of the FoV depends on the optics of the spectrometer but also on the zenith angle at which the instrument is operated.“

Well, the FoV (given as a solid angle) is independent of the zenith angle, right? I guess you mean the size of the sensed air volume / area at OH layer altitude?

Line 329: „in the context of satellite validation the reduced sensitivity“

What does „reduced sensitivity“ refer to here? This is not entirely clear to me.

Line 337: „The only publication reporting a temperature imaging system, so not a scanning one, is by Pautet et al. (2014)“

The SATI measurements (also using OH emissions) have some imaging (although limited) imaging capability, too.

Line 415: space between  $\lambda_z$  and „and“ missing.

Line 438: „He showed“ -> „They showed“

Line 442: „Gravity waves and tides are not easy to separate in OH\* airglow measurements“

That’s an interesting point. I would have thought that tidal variations will be similar on subsequent days. Then a tidal variation could be extracted with a composite (or superposed epoch) analysis applied to many nights in a month or a season. The paper by Shepherd & Fricke-Begemann (2004) comes to my mind, where tidal temperature variations based on K lidar measurements were determined:

Shepherd, M. and Fricke-Begemann, C.: Study of the tidal variations in mesospheric temperature at low and mid latitudes from WINDII and potassium lidar observations, *Ann. Geophys.*, 22, 1513–1528, <https://doi.org/10.5194/angeo-22-1513-2004>, 2004.

Line 464: „and into the residual meridional circulation“

I'm not sure I understand what this means?

Line 496: „Franzen et al. (2018)“ is not listed in the reference list.

Line 539 and following: GOMOS on Envisat was also used to study OH emissions, e.g.: Bellisario et al., O<sub>2</sub> and OH night airglow emission derived from GOMOS-Envisat instrument, *Journal of Atmospheric and Oceanic Technology*, 31(6):1301-1311, 2014.

Same text block: perhaps the instrument papers should be mentioned here?

Table 1, WINDII line: reference to von Savigny is misplaced here.

Table 1, SABER line: "Russell Iii" -> "Russell III"

Table 1, SCIAMACHY line: SCIAMACHY operations ended on April 8, 2012.



Same line: "von Savigny (2015)" is listed twice. Thanks for the honour, but one time is sufficient.

Line 617: "which is relatively broad compared to the other airglow lines mentioned here"

Why is it broader? There are two lines (630 nm and 636 nm), but each line is not really broader than the green line, I think. I checked GLO spectra by Lyle Broadfoot and the individual red lines don't appear to be broader than the green line.

Llewellyn et al. (2003): this paper certainly also deals with the OSIRIS instrument, but the „official“ OSIRIS paper ist the following:

Llewellyn, E. J., N. D. Lloyd, D. A. Degenstein, R. L. Gattinger, S. V. Petelina, A. E. Bourassa, J. T. Wiensz, E. V. Ivanov, I. C. McDade, B. H. Solheim, J. C. McConnell, C. S. Haley, C. von Savigny, C. E. Sioris, C. A. McLinden, E. Griffioen, J. Kaminski, W. F. J. Evans, E. Puckrin, K. Strong, V. Wehrle, R. H. Hum, D. J. W. Kendall, J. Matsushita, D. P. Murtagh, S. Brohede, J. Stegman, G. Witt, G. Barnes, W. F. Payne, L. Piche, K. Smith, G. Warshaw, D.-L. Deslauniers, P. Marchand, E. H. Richardson, R. A. King, I. Wevers, W. McCreath, E. Kyrölä, L. Oikarinen, G. W. Leppelmeier, H. Auvinen, G. Megie, A. Hauchecorne, F. Lefevre, J. de La Nöe, P. Ricaud, U. Frisk, F. Sjoberg, F. von Scheele, and L. Nordh, The OSIRIS instrument on the Odin satellite, *Can. J. Phys.*, 82, 411 – 422, doi:10.1139/P04-005, 2004