

Review of manuscript

“The ground-based MW radiometer OZORAM on Spitsbergen – description and validation of stratospheric and mesospheric O₃ measurements” (by M. Palm et al.)
Atmos. Meas. Tech. Discussions (2010)

The main goals of this manuscript are to describe microwave emission measurements and results from the OZORAM ground-based instrument, which has provided O₃ retrievals in the upper stratosphere and mesosphere since late 2006, from the high latitude Arctic region (79°N). Comparisons versus Aura MLS and TIMED/SABER O₃ retrievals are provided. Some oscillatory artifacts (as a function of height) are ascribed to the OZORAM average profiles, as the features are observed in comparisons of two independent datasets (MLS and SABER). Error characterization considerations lead the authors to implicate spectroscopic parameters as a primary source of error that could cause such oscillations, but further work seems needed.

The manuscript is not presented well enough yet for a publication-quality paper. There are some inaccuracies as well as a simplified presentation style which should be improved/corrected. The honest discussion of results and potential sources of error is well intentioned and should resonate with other ground-based investigators for such emission measurements, despite some shortcomings in the results and analyses. I recommend that some aspects be improved (see more details below) but I am hopeful that this can be presented in large part “as is” as a status report for these measurements. In the future, a combination of hardware and retrieval analyses and improvements could lead to even better results and a longer-term dataset for analyses of polar ozone variations (e.g. partly as a result of temperature changes); unfortunately this measurement system is less useful or tuned to lower stratospheric ozone changes (or ozone recovery).

Specific comments

1. Section 2.3 avoids a lot of detailed work necessary for a forward model description, and the “ARTS user guide” is not accessible to the public, so proper review is not possible. Since “validation” and detailed review of such work and code is usually a difficult issue that could (need to) involve cross-checks versus other forward models in this wavelength region, with few such models available for easy comparisons, the approach taken here is judged to be adequate, albeit not “great”. Other discussion comments point to potential sources of error (relating to baseline removal or sideband suppression), and aspects of this measurement system could be improved upon in order to “take out” the profile oscillations, after more work on such potential error sources and their impact on the retrievals. Oscillatory artifacts do not seem to appear in past measurements/retrievals from other investigators, at least not to the extent “observed by” OZORAM. This would seem to point to possible improvements for OZORAM, although exact identification would take some time. In terms of the impact on this manuscript, it may be sufficient to publish this (revised) work as a “status report” and address the problems at a later time.
2. If the oscillations have time-varying characteristics (do they?), this would impact the long-term potential and stability of this measurement system. More comments on this aspect are warranted.

Also, it is intriguing that the daytime profiles tend to show larger oscillations (double peaks) than the nighttime retrievals – this must be telling you something... (any ideas?).

3. Equation (6) does not add anything useful/specific to the paper, and the confusion between “alpha” here versus “alpha” in equation (3) should be removed (so removing equation 6 or using different variables would be indicated).

4. Section 2.4.1 mentions that variance is defined to be 20% of the ozone a priori, etc... but this presumably refers to the standard deviation, not the variance (which is usually the squared quantity).

5. The discussion of item (3) in section 2.4.1 is limited and a statement of potential artifacts leaves the reader wondering what is being done about standing waves and non-linearities (or how they might contribute quantitatively to the error budget or to oscillations). This might be difficult (time-consuming) to pinpoint or investigate... but one could also conclude that the lack of more thorough investigations could be (in part) the reason for poor retrieval performance (versus satellite datasets).

6. Section 2.5.5

You should specify if the noise component is applicable to typical (1 hour?) integration times or something else (as this component can always be reduced with longer integrations). This is probably not as clear as it could be.

7. Section 4.2

The statement regarding “unreliable” MLS data for heights above 0.1 hPa seems too strong; the reference mentioned does not discourage the use of such data, but it is true that the uncertainties (systematic and random) increase as a function of height. “less reliable” is probably sufficient wording, and averaging several (or many) profiles should significantly reduce the random component... Similarly, in section 4.4, an “upper limit” of 0.1 hPa may have been chosen by Palm et al., but this was not the MLS recommendation per se.

8. Section 4.3.

For SABER (as well as for MLS), please specify (or repeat) how the comparisons are performed, i.e. are you using equation (1) to provide a “smoothed” version of the SABER profiles for comparison to the OZORAM profiles? Simply referring to the appropriate equation would suffice.

9. Figure 11 is supposed to tie oscillatory behavior in the OZORAM retrievals to an error in spectroscopy, but this is not convincing (enough)... Even if one can get the 30% error indicated by these plots, the oscillatory nature is much smaller than this and does not seem to match the oscillations (strength and height dependence) observed in the comparisons versus MLS and SABER profiles (Figure 10).

10. You should try to be more specific regarding the topmost altitude that one should consider for OZORAM retrieval (usefulness).

11. Oscillations in comparisons such as those discussed in this manuscript have been seen before (e.g. Boyd et al. reference mentioned by the authors; see also Hocke et al., ACP, 2007, 7, 4117, 2007, where no large oscillatory behavior is observed versus MLS, although some smooth/smaller oscillations do exist...). Referring to this latter reference would seem appropriate as well. These apparently better results point to instrument/retrieval specifics for OZORAM as presented in this manuscript.

Stating that more investigations are needed is not such a horrible statement to make, as this does seem to be indicated... (and there is a desire to publish regardless of the OZORAM retrieval quality and robustness issues, apparently).

With enough caveats on OZORAM, this can still be a useful paper, hopefully as a stepping stone towards future improvements. Regardless, more discussions with other ground-based microwave investigators seem warranted. Inherent issues with the coarse vertical resolution may be another reason for oscillatory structure in comparisons versus finer resolution retrievals.

Minor editorial issues

- AURA should be "Aura" as it is not an acronym.
- P. 1936, line 12: "in the far infrared" would be better with "and microwave", as this spectral range is often referred to as "microwave" [whether "technically correct or not"]...
 - > line 21, why "compare Fig. 1" and not just "see Figure 1"?
 - > "Frontend" is not one word (I would favor "front end"...).
- P. 1937, and elsewhere... avoid abbreviations (like "approx.")
- P. 1938, change "acusto-optical" to "acousto-optical" and "in parallel to" to "in parallel with"; also delete "spectrometer" after "(CTS)".
- P. 1940, bottom 2 lines, "In the range in between, it is determined by a mixture of pressure and temperature effects commonly represented by the Voigt line shape" [is a suggested rewording].
- P. 1942, line 4, change "is taken" to "are taken".
 - > also, item 3 below this, you are not really "retrieving" standing waves or non-linearity offsets (are you)? [but the retrieval system is sensitive to such effects...]
- Section 2.5.3: One might consider something "state of the art", but this is hard to prove, so it should be avoided, unless the "community" can readily agree to that (I cannot easily do so).
- Section 3, change "path of view" to "viewing path" [several places].
- P. 1946, change "vortex demises" to "vortex breaks down".
 - > also, line 22, change "tells that" to "indicates that"; also define "AVK" ...
- P. 1948, line 23... It does not seem that "Eq. 17" is the right one to point to for a standard deviation in the comparisons...
- P. 1950, line 20, change "than for the retrieval has been used" to "than has been used for the retrieval".
- P. 1951, line 10, you may need to point to "higher altitudes" rather than "lower altitudes" ...
 - > also, line 18, delete "have been described" [or reword].

- P. 1952, line 7, delete “the” in front of “using”.
 - > also, line 9, delete “from”.
 - > how many SABER daytime profiles were used?
- “Russell” is misspelled in the text and reference(s).