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Atmospheric Measurement Techniques Discussions

Interactive comment on "VESPA-22: a ground-based microwave spectrometer for long-term measurements of Polar stratospheric water vapor" *by* Gabriele Mevi et al.

Anonymous Referee #1

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This manuscript nicely describes the VESPA-22 instrument for measuring water vapor, and provides important details of the data analysis technique that seem to me to be appropriate for AMT. I do, however, have some significant concerns with several important claims made in the manuscript.

Main concerns:

There is a second order polynomial which is "added to the retrieval", but there is no discussion of the details of this process. If it is fit individually during each retrieval, and is properly included in the error analysis, then it will have a large effect on the lower stratospheric sensitivity. I am therefore somewhat skeptical of the accuracy of the

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sensitivities shown in Figure 8 at ∼30km and below. The other choice is to treat this as a systematic term, in which case it affects the systematic error but not the sensitivity. In either case, this can introduce an important uncertainty, and the authors need to discuss exactly what they have done.

In the error analysis, many of the terms have been classified as systematic, when in truth, with the exception of the spectroscopy, almost all of them have a significant random component. As a result, the "retrieval uncertainty" shown in Figure 11, which by implication is the only random component, is, in my estimation, absurdly small. I think it would be acceptable to not specifically label the bulk of the errors as either completely systematic or random, but the present suggestion that the precision is <1% over much of the atmosphere would require a great deal of additional evidence.

The VESPA retrievals are performed using a seasonally varying a priori based upon 3 years of seasonally varying MLS data, hence the statement on page 29 that these are "independent datasets" is not true. Claims of high correlation between the measurements are therefore unsubstantiated. If the authors wish to publish claims related to correlations they should either perform their retrievals with a constant a priori (which would probably still leave them with good correlations), or compare deviations from the a priori for the two datasets (which is a tough test). Alternatively they could drop the whole discussion of correlation, along with Table 6 and Figure 14. Also, as long as the retrievals are done with a varying a priori Figure 15 should include a point indicating the a priori for each month.

Equation (4) – presumably 'x' is known for this transition. What is it?

Page 6 line 20 "negligible". Perhaps "small" would be better here. The numbers are given later in the paragraph, so everything here is okay, but for some applications a 1.5% difference might not be considered "negligible".

The use of Tatm is a bit confusing, as this appears to be the atmospheric temperature for the troposphere, but not for other parts of the atmosphere. Would it be better to

label this as Ttrop?

Figure 4 is very nice. Page 10 line 7 – Where does this “constant in frequency within 1.5%” number come from? The diode data sheets? Or perhaps a calibration measurement?

“Tsup” is a rather odd abbreviation for surface temperature.

Page 11 line 7: “explicit” should be “explicitly give”

Page 15 line 13 – “Selee” should be “Seele”

Page 17 line 8 – missing ‘(‘

Page 14 line 8 – How is this second-order polynomial calculated? Is it constant? Is it fit for in the retrieval somehow?

Page 17 - The phrase “Figure 6 a shows a VESPA-22 spectrum integrated for second-degree polynomial 24 hours (blue)” does not make sense to me. What exactly is being done here?

Figure 9 –“specttrocscopy” should be “spectroscopy”

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