

Interactive comment on “Measurement of horizontal wind profiles in the polar stratosphere and mesosphere using ground based observations of ozone and carbon monoxide lines in the 230–250 GHz region: Proof of concept” by D. A. Newnham et al.

Anonymous Referee #2

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The paper by Newnham et al. presents a sensitivity study that shows the potential of ground-based radiometers for measuring high latitude middle atmospheric winds. The effects of the main observational parameters are discussed. This paper addresses an important topic since very few systems provide such information on a routinely basis though radiometers are widely used for trace gas measurements. I found the paper very clear and it should be published in AMT. I have, however few comments.

General comments:

1) The diurnal variation of O₃ is not discussed in the paper. I believe it can significantly impact the wind measurement performances above 60 km. It would be interested if the difference between day and night measurement performances could be assessed.

2) I don't believe that the retrieval calculations performed by the authors fully describe the instrument potential. The altitude dependent retrieval vertical resolution is given by the fixed "retrieval parametrization" including a priori information and probabilistic optimization. I see two problems.

A) The method brings unnecessary subjectivities in the results: results would have been different if a different wind a priori uncertainty is used. (the justification that uncertainties are realistic is not satisfactory since they change with time, altitude, location and a priori data).

B) The best solution depends on the scientific target of the instrument and the vertical resolution is as important as the precision. So, I would have been interested to see error budgets for fixed vertical resolutions (8 km, 10 km, ...). It is then possible to define both the vertical resolution and the observation time to obtain the most satisfactory products. This will be translated into constraints on the a priori parameters for processing the real data (but this is outside the scope of this manuscript).

Such error budget with no a priori contamination and fixed vertical resolution can be assessed, even if the OEM formalism is used. However my point is not to ask the authors to change the study since their approach is commonly used and the results are not wrong. I am simply interested to hear the authors' comment on my comment.

Specific comments:

P3, line 24: The sentence is ambiguous: zonal and meridional winds are derived from perpendicular azimuthal directions. Measurement biases on each component are removed from measurement at opposite directions.

P3, line 30: "showed good agreement . . . 10%". This positive statement seems to be in

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contradiction with that in Line 2 which sounds negative: “deviate increasingly above 40 km ...STD exceeds 20 m/s”. Should we consider that measurements and (re-)analysis are in good agreements in the stratosphere?

P4, line 21: Why such a large difference between the receiver temperature (200K) and the system temperature (1400K)? Could we expect a smaller difference when designing a new instrument?

P4, line 28: I agree with the comment published by R. Rufenacht in the discussion. I think the instrument in Rufenacht et al. (2014) has been carefully designed to mitigate standing waves which, otherwise, could be one of the most significant source of errors. The sentence should be rephrased.

P6, lines 1-3: Are day and night profiles averaged all together? The O3 diurnal variation has an effect on the wind measurement precision and altitude coverage above 60 km.

P7, line 15: Is the covariance matrix set with respect to the horizontal wind or to the line-of-sight wind?

P8, Sec. 2.3: It is not clear for me whether the error estimations are for the average of the two retrievals with opposite directions or a single direction retrieval.

P9, lines4-5: I agree that for real wind retrievals, the O3 priori information is good enough to obtain small uncertainties due to the O3 a priori. However, in my point of view, the calculated errors are too large. The wind retrievals should be rather independent of the O3 a priori? May-be there is an additional error induced by the retrieval procedure itself and, in that case, the other retrieval errors estimated in the paper might also be overestimated. a) Authors should check if the O3 a priori error estimated with the MC analysis match the errors linearly derived from the off-diagonal terms of the averaging kernel. b) why the average of two retrievals with opposite directions does not remove such error?

P10 and 11: Both sections 3.3 and 3.4 deal with the dependence of the retrieval per-

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performances with respect to the signal to noise ratio: increasing the observation time by a factor 4 is similar to reducing the system temperature by a factor 2. I think the discussion should be for both the system temperature and the observation time as in Fig 14. So I would recommend to merge the two sections as well as Fig.11 and 13. (Note that the figure numbers in the text is not consistent with their actual numbers).

P31,32,33: Fig.12 should be Fig.14, Fig.13 should be Fig.12 and Fig.14 should be Fig.13 ?

P27, Fig8 caption: Should be 6hours in each opposite direction (value in parenthesis)?

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