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Atmospheric Measurement Techniques Discussions

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 #1

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Ground-based measurements of winds in the stratosphere and mesosphere are sparse, basically due to a lack of a relative simple instrument to make such observations. Based on older efforts, some quite recent studies have put emphasis on that microwave radiometry can derive wind information, both from satellite (Baron et al., 2013) and ground (Rüfenacht et al., 2012 and 2014). Rüfenacht et al. selected an ozone transition at 142 GHz, while in this manuscript measurements using some transitions around 230 GHz are explored by simulations.

The transition selected by Rüfenacht et al. is probably the best general choice for ground-based observations, while transitions above 200 GHz can be of interest for high altitude and polar sites. The manuscript describes very clearly and in a detailed manner the potential of using the 230 GHz transitions for conditions found above Antarctica. Based on the experience from the articles mentioned above, it is no surprise that winds can be retrieved also from these transitions, but it is still a valuable contribution to point out and document these options. The manuscript is very well written and this manuscript should be published in AMT after some minor corrections and consideration of the title.

The text is concisely written, but the number of figures still makes the manuscript quite long and gives an impression of a technical report. I think some figures can be removed. A figure to explain the effective area at different altitudes and elevations should not be needed and Figure 2 could be removed. Figure 4 is neither critical, it suffices to comment the important features in the text.

I am also a bit sceptic about Fig 9. First of all, it could suffice to mention in the text that there is no change in the performance for resolutions up to 300 kHz. However, this figure also brings up the main drawback of the manuscript, it just includes simulations assuming a more or less ideal instrument and it can be questioned if this is a "proof of concept" (more below). My point here is that a perfect knowledge of the "instrumental line shape" is assumed and then very good results can be obtained in simulations, even for relatively poor resolutions, but these results are necessarily not valid for practical measurements. The impact of errors in the assumed instrumental line shape should increase strongly when deteriorating the frequency resolution, and in practice a high frequency resolution is always to prefer. That is, for me Fig 9 is of little interest, it could even be misleading. The authors should also consider if the assumption of an ideal instrument affects other results. For example, could antenna sidelobes be a problem for a 80 degree zenith angle?

My second main comment is the assumption of a constant wind field over the areas shown in Fig 2. The spatial resolution is well described on lines 10-16 of page 5. When the text then jumps to ECMWF data, I then expected a discussion of gradients inside the coverage area, but instead just wind speed histograms are discussed. That is, at least a new paragraph should be started on line 19.

Even better would be if wind gradients could be also discussed. A hint to the authors is to include wind gradients in the Monte Carlo simulations. I don't demand this extension, but it would rise the value of the manuscript considerably, as the manuscript would introduce some really new analysis (as far as I know).

Two smaller comments:

Page 3, line 25: Is not the main frequency uncertainty normally originating from the mixer LO signal?

Page 10, lines 24-26: No information is lost by switching to a higher frequency resolution. The lowed "SNR" in individual channels is compensated by the higher number of channels. That is, I don't agree with the argumentation here.

Don't the term "proof of concept" imply a practical demonstration, to show that an idea or some simulated results work in practice? This was at least my understanding of the term before reading this manuscript, and I think this description from Wiktionary supports this view:

A short and/or incomplete realization of a certain method or idea to demonstrate its feasibility.

That is, please, consider changing the title.

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