Atmos. Chem. Phys. Discuss., 12, C9856–C9860, 2012
www.atmos-chem-phys-discuss.net/12/C9856/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "High resolution VHF radar measurements of tropopause structure and variability at Davis, Antarctica (69° S, 78° E)" *by* S. P. Alexander et al.

Anonymous Referee #2

Received and published: 27 November 2012

General comments:

This manuscript evaluates the significance of tropopause details (i.e. altitude and structure) derived from VHF radar observations at an Antarctic location. The techniques of analysis, including the comparisons made with tropopause details derived from other measurements, are similar to those used in a number of earlier studies (which have been referenced). However, this is he first time that such an analysis has been carried out for an Antarctic location.

Although I ultimately have a favourable opinion of the work, I was struggling to see the full scientific significance on my first read. It was only after I had read a few of the cited papers - notably Zaengl and Hoinka (2001) and Birner (2006) - that the manuscript started to make sense. At present, the introduction section is rather generic. I think that it would be greatly improved if it gave a summary of the Antarctic UTLS features that are relevant to the subsequent analysis. Admittedly, some of these details are referred to in the discussion. However, many aspects of the analysis appear somewhat mysterious before the background is understood. Once these points have been made, the significance of the current work, when compared with similar radar-based studies, will be immediately obvious.

I have only one significant scientific concern - point S1.

Specific comments:

S1) The only part of this manuscript that I have a significant disagreement with is section 3.5 (page 26184). Frequency spectra with gradients of close to -2 can be found under all sorts of conditions and do not necessarily indicate gravity wave activity - refer to Worthington and Thomas 1998 (Q. J. R. Met. Soc., 124:687-703) for a detailed discussion. Since the authors cannot demonstrate the presence of inertia-gravity waves (IGWs) using the radar dataset, I recommend that this section (and the corresponding discussion items) be removed. As an aside, it should be possible to infer IGW activity from the radiosonde profiles - see. e.g. Cadet and Teitelbaum 1979 (J. Atmos. Sci., 36:5, pp 892-907).

S2) A number of algorithms have been used to determine the tropopause altitude from radar echo power, e.g. based on the peak in echo power (e.g. Vaughan et al. 1995, Hall et al. 2009), based on an absolute value of echo power (e.g. Gage and Green 1982), and based on the peak in echo power gradient (e.g. Vaughan et al. 1995, Hooper and Arvelius, 2000). The statements made on page 26176 lines 5-7 and on page 26177 lines 15 - 20 obscure this - and appear to be contradictory.

S3) page 26178 lines 3-5. "A few isolated z_radar outliers are removed by constructing a 24 h running mean time series and removing those z_radar which are more than

2 standard deviations outside this running mean". Does this method cause a problem when dealing with tropopause fold conditions, when sharp changes in tropopause altitude can occur?

S4) Fig. 3 is slightly redundant since it is hardly discussed - page 26179 lines 13-17. Either more use should be made of it or it should be removed.

S5) I think that Figs. 5 and 8 are supposed to be demonstrating the differences in tropopause sharpness during the winter and summer months. However, this is not initially clear from the brief discussion of Fig. 5 on page 26180 lines 12 - 15. A bit more background, together with an explicit reference to Birner (2006) and/or Birner et al. (2002), would help to introduce readers to the idea of the tropopause-relative co-ordinate system. As an aside, I first thought that the term "tropopause relative radar power", on line 13, referred to a relative power, not to a relative altitude. The hyphenated term "tropopause-relative power" would be much clearer. The discussion of Fig. 8 on page 26181 line 24 - page 26182 line 5 is similarly brief/obscure. The following sentence on the last line of page 26181 is particularly confusing: "There are larger differences in N^2 during JJASO (Fig. 8b) due to the large JJASO differences."

S6) The significance of showing wind speed in Fig. 6 was not entirely clear. Unless it is being used to make a useful point, it could be removed.

S7) The main conclusion of this work is that the radar-derived tropopause corresponds to the dynamical tropopause rather than the "radiosonde" tropopause. Although I broadly agree with this point, I think that the nomenclature is slightly misleading. The radar-derived tropopause is inherently a lapse rate tropopause, since it is the sudden increase in static stability (i.e. a decrease in lapse rate) that gives rise to the increase in backscattered power. The inappropriateness of the "radiosonde" tropopause during the Antarctic winter is a failure of the WMO (lapse-rate-based) definition - not of the radiosonde measurements themselves. As the authors have shown in Fig. 7c, it is possible to derive an M^2-based tropopause altitude from the radiosonde measurements

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that closely corresponds (as should be expected) to the radar-derived one. Moreover, the planetary approximation of Potential Vorticity (PV) - see e.g. eq. 2 of Birner (2006) - depends on the vertical gradient of potential temperature. As such, the PV-based tropopause is also, to a certain extent, a lapse-rate-based one. Consequently it would be more-appropriate to refer to the WMO tropopause rather than to the radiosonde one. Moreover, it would be useful to extend Fig. 7 to include the ozone-based tropopause, which is the only one that is independent of lapse-rate considerations.

Technical corrections:

T1) The WMO criterion for the tropopause is shown incorrectly in the middle panel of Fig. 2 as "dT/dZ < 2 K/m". Since dT/dz = -lapse rate, the label should read "dT/dz > -2 K/km".

T2) page 21677 lines 5-8. The wording in these sentences is a bit muddled. The received echo power is proportional to the "mean vertical gradient of [generalized] potential refractive index", M (Ottersten, 1969, Radio Sci., 4, pp 1247–1249) - not to the "vertical gradient of refractive index" or to the "generalized refractive index". It would be useful to also state the 1/zˆ2 dependence at this stage.

T3) The thin grey line indicating the 2 PVU level in Fig. 4 was almost impossible to see when I primnted it out (it was okay when viewed on-screen). A different colour and/or a thicker line would help. The red crosses would also be more-easily visible if made slightly larger and/or the lines were thicker.

T4) The panels and text of Fig. 7 are really rather small. They would be clearer if made a little larger.

T5) I think that the word "with" in the following sentence, on page 26186 lines 7-10, should be "in": "During the Northern Hemisphere mid-latitude (30–60 N) winter, the lapse-rate tropopause decreased WITH altitude for weaker anticyclonic activity and remained relatively constant (< 0.5 km variability) under cyclonic conditions (Randel et

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al., 2007)."

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 26173, 2012.

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