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Comment

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 #1

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The paper gives a nice comparison of tropopause heights determined by different methods, including VHF radar. This provides a valuable empirical test, in an average sense, of the usefulness of radar-defined tropopause height, which can be measured (at point locations) with much better time resolution/continuity than other methods. The study would be improved by more detailed analysis of the differences between the radar tropopause and the others, and how these differences depend on conditions. Also there is a lack of discussion of the physical basis of the different methods which seems to lead to some misinterpretations. Finally there are some speculative results proposing quantitative relations between tropopause height variability and inertia-gravity wave and tropopause folds, which are not well motivated.

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In more detail :

1. Regarding the physical basis of the different methods and the interpretation of differences :

The radar tropopause is found by looking for the maximum gradient in range-corrected radar signal power. Very many comparisons between radiosondes and VHF radar have shown that, at tropopause heights, this is the same as looking for the maximum vertical gradient in static stability.

The lapse-rate tropopause found using radiosondes defines a particular temperature gradient as the tropopause, so it corresponds to some value of static stability, varying somewhat depending on the background potential temperature.

The PV tropopause is defined as a particular value of PV (-2 PVU in this case). Potential vorticity is the product of the vertical gradient of potential temperature and absolute vorticity, divided by density. For high latitudes, the Coriolis term dominates the absolute vorticity, and the PV tropopause is also determined to a large extent simply by the potential temperature gradient (i.e. static stability), with differences introduced when the relative vorticity becomes large enough compared to the Coriolis term ($\sim -1.4e-5$ s⁻¹ at Davis).

The ozone tropopause is defined as a particular gradient in ozone mixing ratio and is expected to correspond to the lower boundary of stratospheric air. High PV is also considered a good tracer of stratospheric air so the ozone tropopause and the PV tropopause can be expected to be close to each other and follow the same height variations.

So, there is no a priori reason why the radar tropopause (maximum gradient in static stability) should coincide with the others (various values of static stability). Only when the transition from low static stability in the troposphere to high static stability in the stratosphere is sharp, and the relative vorticity is small, can we expect all to coincide.

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In other conditions - slow transition, high relative vorticity - they can be expected to diverge.

Figure 9a,c would therefore be more informative if it also included the height of the -2 PVU tropopause. A comparison of the differences in tropopause heights in relation to the sharpness of the troposphere-stratosphere transition would also be useful. Otherwise it is impossible to tell if the differences between 9a and 9c are just due to the slow transition in summer compared to winter (seen in Fig, 5) and a slower transition in cyclonic conditions compared to anticyclonic ones. Also the statement in the abstract that the radar tropopause corresponds closely to the 2 PVU level in both cyclonic and anticyclonic conditions is both surprising and not substantiated by any of the data presented unless this comparison is also included in Fig. 9a,c. (Although the statement is contradicted by Fig. 4b, so maybe it needs to be reworded anyway).

On page 9, lines 16-18, the 2PVU contour is said to represent the dynamical state, the lapse-rate tropopause the thermal state. This is not really a fair separation since both are in fact determined by static stability. The 'dynamic' (relative vorticity) contribution to the 2PVU contour is a minor part in most conditions.

Altogether, the interpretation of Fig. 9a,c in sections 3.3, 4 and 5 really is not complete or correct without including consideration of the height of 2PVU tropopause and the sharpness of the transition as a function of season and vorticity, and the close relation between PV and static stability.

2. Tropopause folds

Although tropopause folds are usually connected to changes in the tropopause height, the converse is not necessarily true. Any warmer air masses arriving over Davis will lead to a jump in tropopause height without necessarily being associated with a tropopause fold. The larger number of tropopause jumps found in the winter can be simply a consequence of the larger number of cyclonic storms (Fig. 9b,d) bringing those air masses. If radar winds are not available, ECMWF winds could perhaps be

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used for a more careful search for conditions likely associated with folds (jet streaks, strong wind-shear)

3. Tropopause altitude power spectra

These are interpreted as gravity-wave spectra on the basis of an approximate $-5/3$ slope. This seems extremely speculative - almost and process with a cascade of scale sizes gives a $-5/3$ slope. The obvious source of the tropopause height variability, is simply the many cyclonic storm systems around coastal Antarctica. The seasonal differences in storms are well known (e.g. Simmonds, I., Modes of atmospheric variability over the Southern Ocean, J. Geophys. Res., 108(C4), 8078, doi:10.1029/2000JC000542, 2003.)

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