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Interactive Comment

Interactive comment on “The water vapour distribution in the Arctic lowermost stratosphere during LAUTLOS campaign and related transport processes including stratosphere-troposphere exchange” *by* A. Karpechko et al.

Anonymous Referee #2

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This paper presents a set of high quality water vapor measurements (coincident with ozone measurements) in the winter arctic tropopause region. They first arrive at a number of conclusions, generally consistent with previous work, about the overall statistical character of the measurements, such as: (1) a reasonable penetration depth of tropospheric air into the lowermost stratosphere; and (2) the idea that the water vapor measurements are reasonably lined up with the dynamical tropopause and potential temperature surfaces. They then present a case study, showing the origin and development of an observed filament of high water vapor, demonstrating that it originates

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from a developing anticyclone in the eastern Atlantic. A number of other similar cases are briefly discussed. Though observations of streamers, either by aircraft or sonde, have been made frequently in the past and explained by model studies (usually RDF or contour advection), it is remarkable that the standard model water vapor output from a forecast model does as well as it does in reproducing the observations (Figure 4). For these reasons, I believe that there is publishable material in the paper.

The authors then attempt to quantitatively evaluate cross-tropopause fluxes (CTF) in connection with these anticyclonic events. Here, I am not sure what they are trying to do. They note that along-trajectory changes are strong along the edges of the anticyclones. They also note that these are regions where clear air turbulence could occur, based on calculated wind shears from the analyses (CAT index). The CTF is calculated from equation 1 based on 12 hour trajectories. F values are large along the flanks of the anticyclones because the PV gradients are large. Thus, small deviations in the flow give significant movement across PV surfaces and large values of F (of both signs). CAT indices are large in these regions because regions of strong PV gradients have strong winds. This kind of coincidence is not enough to prove that the turbulence is causing the exchange. Do they claim that the turbulence is causing the streamers? I don't think so, since on page 4738, lines 1-5 they mention the more traditional explanation, namely that wave breaking processes cause the streamers, followed by nonconservative processes actually changing the PV (diabatic heating) or mixing the PV into its surroundings (turbulence).

I do think that significant exchange of some kind is occurring along the flanks of these anticyclones (i.e., I do not think the trajectory calculations are in error on a statistical basis). Much of it is "back and forth" exchange (see, for example, Wernli and Bourqui, 2002). With trajectories of only 12 hours, it is hard to tell what is permanent and what is not. Perhaps smoothing the results over 5 degree lat-lon bins or something like that might give a more reasonable picture (averaging over the positive and negative strips of CTF at the anticyclone edges). Nevertheless, it is not clear what this result has to do

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with the formation of the streamers and how it relates to their water vapor observations.

The authors also assert that changes from stratospheric to tropospheric PV (and vice versa) due to the turbulence are more important than diabatic changes (page 4740, lines 14-16). They also assert that diabatic processes produce a one way transfer from troposphere to stratosphere (page 4739, line 20) based on a comparison with an idealized axisymmetric radiative calculation (Zierl and Wirth, 1997). Neither of these assertions can be proven without some diabatic calculations. In fact, diabatic mass transfer across the tropopause in midlatitudes is downward (Schoeberl, 2004) and quite large because of the prevailing radiative cooling. The authors are correct that the centers of anticyclones, where the tropopause is cold and elevated, have either minimal cooling or (possibly) some heating (implying upward diabatic mass transfer).

My suggestion for improving the paper is to actually try to understand which processes are contributing to the evolution of the streamer they are observing, and how much. The remarkable result of figure 4 is that the model is potentially good enough to answer this question. I don't think that CAT or diabatic processes are generating the streamers from the edge of the anticyclone (and neither do the authors, I think). So, I would suggest actually writing the equation for the evolution of PV along a trajectory (in the region of the streamer at some appropriate time) and evaluating the terms (which are mixing of PV into the surroundings by turbulence and diffusion and change in PV by the vertical gradient of diabatic heating). A less ambitious tack would be to relate the CAT index and change in PV to the streamer of interest at various times.

The results of their CAT analysis and PV change analysis along the flanks of the anti-cyclone may be worth noting. However, I think they are peripheral to the main point of the paper, which is the origin of the streamer and the ability of the model to simulate it.

Specific Comments:

Abstract: "..for stratosphere-troposphere exchange (STE) AT HIGH LATITUDES."

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Abstract: What is the difference between clear air turbulence and "developed" clear air turbulence?

Page 4729, line 28: What is LAPBIAT?

Page 4734, line 6: "This is slightly higher than THE value of 25K found by Hoor et al..."

Page 4735, line 15: "Only for a short period (18 February-20 February 2004) was the station located on the anticyclonic side ..."

Page 4736, line 18: Why not show the sounding being discussed?

Page 4739, line 29: "..northeast of the Greenland SEA??" or northeast of Greenland?

Page 4742, line 4: Gettelman and SOBEL

Page 4739, line 15: "..(and therefore for CTF), small-scale turbulence and .." (no i.e.)

Page 4739: Specifically it is vertical gradients of diabatic heating that produce changes in PV. It would be useful to go into a little more detail on how diabatic heating changes the PV. I am not certain that it leads only to upward (troposphere to stratosphere) transfer. If the authors choose to make so many assertions about the role of turbulence, we need a sharper discussion of how turbulence changes the PV along a trajectory (it mixes it). This has been a contentious issue in the past, with much controversy about Shapiro's turbulence generating PV mechanism (referred to in Traub and Lelieveld).

Page 4741, line 1: "THE influence of turbulence ..."

Page 4742, line 3: "...; however, there is no clear way to choose a threshold..." (no 'how')

Page 4742, line 21: "Indeed, there is observational evidence ..."

I think it is interesting that both the downward flux and the CAT index have large, correlated variations in Figure 7. The upward flux has much less variation (though it is also correlated, albeit less strongly, with the CAT index). This is intriguing, suggesting (but

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by no means proving) that CAT drives the downward transport (negative CTF), while the upward CTF is due to some other mechanism.

With some exceptions (like Figure 6e,f over Greenland), the downward and upward fluxes are right next to each other in long strings. Is some of this due to error in the trajectory approach to CTF (as suggested on page 4741 and line 9 regarding inconsistency in the analyses)? Might it not make sense to smooth the results and come up with a hopefully more believable "net" flux?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4727, 2006.

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