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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "A quasi-Lagrangian coordinate system based on high resolution tracer observations: implementation for the Antarctic polar vortex" *by* E. V. Ivanova et al.

Anonymous Referee #2

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General remarks

This paper puts forward the idea of a coordinate systems that follows the vortex flow. It is proposed that a a two-dimensional quasi-Lagrangian coordinate system should be used based on the mixing ratio of the long-lived stratospheric trace gas N2O.

The paper analyzes a unique set of measurements, namely high-altitude aircraft measurements in the Antarctic vortex in 1999. There are some very interesting issues raised in the analysis of these data (see below). Further, the approach to use high-quality tracer measurements as a vortex following coordinate and as a means to detect

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Interactive Discussion

Discussion Paper

transport barriers is very promising. I am sure that this manuscript will eventually turn into an important paper in ACP.

However, I have several problems with the manuscript as it stands at the moment; they are described in detail below. I would strongly recommend to revise the manuscript addressing these issues before publication in ACP.

Polar transport barriers

A central point for this paper is the question of polar transport barriers. The paper cites WMO2007 and references therein on this issue but takes apparently a different view. In the manuscript, the authors say "... the strong circumpolar winds, i.e. the polar night jet, and the strong meridional gradients of the potential vorticity P (Ertel, 1942) in the vortex boundary region give rise to a transport barrier and strongly suppress the penetration of air from the surf zone into the vortex core and vice versa (e.g., Schoeberl and Hartmann, 1991; Labitzke, 1999)." In this picture there is *one* transport barrier in the vortex edge region that separates the vortex core from outside of the vortex (surf-zone). In contrast, in WMO2007, sec. 4.1.1.3 Transport, it is argued that "The Antarctic vortex is divided into two distinct regions of approximately equal area: a strongly mixed vortex core and, separated from the core, a broad ring of weakly mixed air extending to the vortex boundary (Lee et al., 2001). This division was deduced from an analysis of a mixing diagnostic (...) based on transport calculations of an artificial tracer field on an isentropic surface (480 K) for the period from July to November. A transport barrier within the Antarctic vortex between July and November, is likewise apparent for the altitude range between 475 and 650 K in the potential vorticity field (Tilmes et al., 2006b) and, moreover, extends to the setup phase of the Antarctic vortex in March and April (Tilmes et al., 2006a). During March and April, ozone-tracer relations in the two vortex regions are clearly distinct (Tilmes et al., 2006a). Further, measurements of ozone and volcanic aerosol show rather different vertical profiles within the vortex core

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and in the edge region (Godin et al., 2001). A better understanding of the dynamical mechanisms responsible for the occurrence of transport barriers within the vortex is important, . . . " Of course, the authors may disagree with the picture suggested in WMO2007. But if they do, they should clearly say so. If not, it should be explained in how far the two different views can be reconciled.

Further, in the paper. the Nash method is employed to define the vortex core and the "vortex edge region". The definition of the vortex core has been employed before in the way it is used here, the definition of the vortex edge region is unusual insofar, as it does *not* employ the maximum gradient in PV (the Eq. 1 in the paper) as the definition of the location of the strongest vortex transport barrier and thus as the definition of the "vortex edge". This point should be discussed in the paper.

Long-lived tracers

The point is made in the paper that N2O is 'long-lived' which is certainly correct in the lower stratosphere regarding chemistry. However, the use of N2O as a coordinate also requires that N2O mixing ratios are not altered by mixing. This point needs to be discussed in the paper. For example, how well would the proposed coordinate system describe conditions during the two splits of the Arctic vortex in 2002-2003?

The variation of the N2O mixing ratio with latitude

Central to the technique proposed in this paper and to the definition of the δ-value is the schematic shown in Fig. 3. What is missing, however, is a demonstration that the N2O measurements during APE-GAIA support the picture shown in the schematic. I suggest to show the N2O measurement against (eq.) latitude for the most frequently sampled potential temperature level in a way that is directly comparable with the schematic.

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For example, it is not obvious that the δ-step should occur at the inner edge of the vortex boundary in Antarctica. The value adopted for the δ-step is taken from Greenblatt et al. (2002), although it was derived for the Arctic vortex. The argument in Greenblatt et al. (2002) for choosing 20 ppb is based on the measurement precision of their N2O measurements. It might be worth pointing out that the two instruments have about the same precision (if correct).

Equivalent latitude of vortex edge

My interpretation of the analysis in Fig. 7 is that the equivalent latitude of the vortex edge (and the inner and outer boundary of the edge region) changes with altitude. Therefore, I do not understand the separation of the data into the bins I to VI that are based on a fixed equivalent latitude with altitude. Moreover, there is a strong variability of the location of the vortex edge (and the edge region) with altitude. Could this be the effect of an insufficient horizontal resolution of the employed meteorological fields? Taken together, these two effects could lead to the equivalent latitude binning looking worse than necessary in the analysis shown in Figure. 8.

χ as the vertical coordinate

If the assumption in the paper is that "all air parcels ... undergo the same diabatic descent" then I would argue that the actual vertical coordinate employed is potential temperature. Of course for constant diabatic descent and weak horizontal mixing (see p. 16135) constant potential temperature is equivalent to constant χ. But the underlying physical coordinate is potential temperature. This is becoming evident in the discussion of Figure 9, when indeed potential temperature is used as the vertical coordinate instead of χ/N2O. Otherwise the interpretation of ΔN2O as signature of a filament would not have been possible. I do not believe that the suggested analysis is possible without

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explicit reference to Θ. This point needs to be discussed more clearly in the revised version of the paper.

Regular and anomalous mixing

In the discussion about the development of tracer-tracer relationships the terms “regular mixing” and “anomalous mixing” are used. First, these terms should be clearly defined. Second, I have difficulties with some of the arguments about the development of tracer-tracer relationships. Differential descent (but only as long as no mixing occurs during descent) and mixing across the vortex edge both lead eventually to a linearization of the relationship (as stated in the paper), but clearly the impact is the stronger, the stronger the difference in tracer mixing ratios is. Thus, differential descent during the set-up phase of the vortex and mixing across the vortex edge between vortex and mid-latitude air should have the strongest impact, whereas differential descent in an established vortex (when descent rates and thus also differential descent is becoming smaller) should have no major impact of tracer-tracer relationships. Further, I do not understand the argument, why “the outer vortex region . . . [should] remain ‘frozen’ at . . . incomplete state of mixing”. Would one not expect substantial mixing in the equatorward zone of the outer vortex region, equatorward of the maximum PV gradient?

Solitary points and vertical mixing

The observations presented in in section 5 are very interesting and are an important finding of the paper. Nonetheless, I am highly sceptical of the interpretation of the “anomalous, solitary” points. What is suggested here is vertical mixing across isentropes “separated by tens of K”. If the mixing had occurred between points A and B, tens of K means 55 K! This is rivaling the cross isentropic transport that occurs in the strongest tropical convective clouds driven by massive release of latent heat. Is there

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any evidence that breaking gravity waves can be the cause of a 50 K cross isentropic transport? Further, the mechanism proposed here requires that the vertical transport and mixing occurs in a very narrow column of 40 km. Is this compatible with the proposed gravity wave mechanism? Could measurements of other trace species on-board the Geophysica help here? And would one not expect to find signatures of such mixing events in Antarctic ozone sonde vertical profiles?

The authors argue that the "anomalous, solitary" points cannot be caused by isentropic mixing based on their analysis of the data in Fig. 12. However, this argument is strictly true only when it is assumed that the mixing event occurred on 12 October 1999. If the mixing event had occurred much earlier, the potential temperature of the points A-D would be quite different than reported on page 16141.

Water vapor

It is a strength of this paper that the water vapor measurements are taken into account to support the analysis based on the N2O measurements. The major issue here is to clearly demonstrate that the two concepts are really consistent. That is, is the same location of the inner vortex edge deduced from the H2O and the N2O measurements? Data are shown for one example (21 Sept.) but how typical is this example for all the data obtained during APE-GAIA?

Minor issues

- Intro: I would suggest not using the term "containment vessel" any more; in particular, Tuck et al., (1995) do not argue for a "containment vessel".
- page 16126: this is of course true for all atmospheric phenomena and for all species, the variability over say 10 km in the vertical is much larger than over

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10 km in the horizontal. What exactly is the point here?

- page 16129: Which data version of ILAS-II data is used? Discuss the quality of the N2O measurements compared to correlative measurements. I suggest including citations to an ILAS-II paper and a citation to a validation paper of the ILAS-II N2O measurements.
- p. 16135, Fig. 9: it is not really necessary to use modified PV if the analysis is done for more or less constant potential temperature, but if it is used it needs to be defined.
- p. 16136, l. 3,4: Is the point really "absorption" of short-wave radiation by O3, H2O, and CO2? This is confusing. Further diabatic descent is not only "accompanied" by cooling, $\dot{\theta} = Q$, with Q being the cooling rate.
- Fig. 10: The time interval should be written as Δt.
- Page 16137, l. 5: provide references for this statement; does the sea ice edge really impact significantly the diabatic descent rates during polar night?
- page 16138: The idea that different compact tracer-tracer relationships develop in regions isolated by transport barriers is not solely due to Plumb et al., for example the paper by Michelsen et al., 1998 should be cited here. And I do not believe that Plumb et al. 2000 really argue for a strong transport barrier between the polar vortex air and mid-latitude air (see also point below).
- page 16138, l. 9. This paragraph is confusing. Plumb et al. present model calculations that suggest a rather substantial mixing between polar and mid-latitude air, so why is this paper cited to make the point of "isolated physical space" (which in the current context can only mean the polar vortex)? Other papers cited elsewhere in the paper argue more strongly for a strong separation between mid-latitude and vortex air, but are not cited here. On the other hand, the

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latter papers make the argument that, in the polar vortex (considering the ozone N2O relationship), mixing processes do *not* dominate chemical ozone loss (i.e. sinks), a fact that is also obvious in Fig. 8 of the present paper. Finally, of course, diabatic advection alone, in the absence of mixing or chemistry does not change tracer-tracer correlations at all. Please clarify.

- page 16139: The reason for the asymmetry between the NH and the SH should be briefly described, rather than only citing a reference.
- Fig. 11a: Have the mid-latitude measurements been corrected for the known trends in CFC-11 and N2O?
- page 16142, l. 11: What is the basis for the judgment that the events are "sporadic"?
- page 16143, l. 17: Perhaps one should also cite here the classic paper by Jones and Pyle on CH4/H2O.
- page 16143, l. 19: This is confusing: why is the wet troposphere a source of low water vapor mixing ratios?
- Fig, 14: The dynamic range of the color scale should be adjusted; also do not call theta a "compound".
- Fig. 15: The measurements really seem to fall into three classes, but this separation is not clearly borne out by the separation into blue, green, and red measurements.
- Fig. 16: This is a schematic, but it should be demonstrated that the real world resembles the picture suggested by this figure. Data for one flight (but only for one) are shown in Fig. 17.

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- Fig. 17: The Nash vortex edge definitions are shown. Why is the equivalent latitude of the edge dependent on H2O?
- The references should be checked for errors.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16123, 2008.

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