

Interactive comment on “Quantification of the impact in mid-latitudes of chemical ozone depletion in the 1999/2000 Arctic polar vortex prior to the vortex breakup” by G. Koch et al.

Anonymous Referee #1

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General comments

A method is presented to study cross vortex boundary transport and its effect on mid latitude ozone before vortex break up. Exchange events are identified with a trajectory model. Air parcels transported from the vortex into mid-latitudes are analysed with a chemical box model and their impact on mid-latitude is quantified.

Whereas the basic idea seems to be worth to be carried out in a detailed study, this paper strongly suffers from incomplete or superficial description of the methods used. I therefore recommend to publish the paper only after a complete revision has been applied. This should include a clear presentation of the method and a critical assessment of its limitations.

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Specific comments

Section Introduction

The motivation for the work is not completely clear for the reader. If the intention is to test if the model is capable to simulate and to explain the observed ozone trends at Payerne (and to contribute to the discussion of midlat ozone depletion in general as suggested by the variety of references), then the comparison with one specific winter, which in addition shows relatively low dynamical activity, is doubtful and needs further justification. What are the limitations of the previous studies and what is the new idea of the present work? At least, this has not pointed out clearly.

Section Data and method

The description of the trajectory method is in my opinion not detailed enough. Is there a reference for the model? What is the reason to select the levels given? Especially, why have lower levels not been included in the study? What does it mean when the authors say that isentropic trajectories are used, but diabatic sinking is included? Are just two profiles for the description of vortex descent adequate? Is there a switch for inside/outside vortex airmasses for sinking too?

As the method is based on the identification of exchange events, this part should also be described in more detail. For the side criterion wind speed, the substantial fraction is not given, nor is it justified. There is a 4day persistence criterion for inside transport, but no for outside? And why, if not? I suggest, the method needs some minimum number of air parcels to give meaningful results in a statistical sense for a specific exchange event. This should be discussed in the paper. I would also expect that this necessary number increases with time through the error in the wind field. Has this been evaluated?

What is the relation to filamentation? Decrease of PV during adiabatic processes means mixing. Is it justified to treat the chemistry within airparcels as if no mixing

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had happened or, how long can these air parcels be treated as such?

The initialization of the chemical box model is unclear. Have all air parcels been initialised with the same profile? I would expect even at 1 Dec some differentiation between mid-latitudes and polar air masses. Is there a reference to the data used?

The photolysis rates used are based on 2D climatology of ozone. The fact, that cross vortex boundary transport is more probable in dynamical active periods and in addition, that also upper layers may be affected, means that the ozone profile for these events may differ from climatological data, probably in a systematic way, and therefore the photolysis rates. This may have consequences for the chemistry which can be as relevant as the use of the somewhat outdated reaction rates. For both aspects, use of climatology and the use of older JPL values, some justification is needed.

The denitrification is handled so crudely that microphysical effects and chemistry seem not really to be the focus of the paper.

Section Overview of the exchange events

Generally, the exact procedure to derive the percentage air masses is not clear. Even for a complete stable vortex I would expect the total number of air parcels inside the vortex to decrease. As the air parcels have been initialised only north of 50N there cannot be an exact balance of inside-outside transport. Have the numbers given corrected for this effect?

What is your definition of mid-latitude air?

Section Split-off event

Are the 65 trajectories selected for the estimation of the ozone loss chemically representative for all air parcels leaving the vortex at this event?

Section Impact of polar ozone

The sections 5.2 and 5.3 do not give really new results related to the current study and

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often repeats well-known facts. In addition, I find the discussion of long-term trends to be outside the scope of this paper.

Section Conclusions

In view of other publications and the different method used, I'missing an edaquate discussions of the results. If the five-times higher percentage of out-of-vortex transport can be attributed to the different definition of the vortex edge, the results of such studies would be highly questionable. The main result of mid-lat early winter ozone loss should be thoroughly compared with other publications and observations.

Minor comments

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|------------|--|
| 1912.18 | two processes are given |
| 1914.04 | change: "when the vortex is stable" to "when the vortex was stable" |
| 1914.18 | SOLVE/THESEO is not explained at this stage; the term SOLVE/THESEO winter 1999/2000 is not a nice expression |
| 1915.08 | change maxima to maximum |
| 1916.16 | in the Fig., PV* is given as 68.7 |
| 1917.24 | in the description section, tracectories are started > 55N |
| 1926.33 | change Achen to Aachen |
| 1929 | Fig.1, what is PV* (give at least reference to section) |
| caption | |
| 1932, 1933 | placement and denotation of figures in caption should be made consistent, orientation should be the same |
| Fig 4+5 | |

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