

Interactive comment on “The influence of polar vortex ozone depletion on NH mid-latitude ozone trends in spring” by S. B. Andersen and B. M. Knudsen

Anonymous Referee #2

Received and published: 5 May 2006

This is an interesting paper based on a previously used method to estimate the influence of Arctic ozone on mid-latitude ozone trends. The method is based on RDF calculations of the dilution of the polar ozone depletion. While previous publications focused on the effect of the ozone loss dilution in specified winters, this paper extends the method in order to calculate the influence of polar ozone depletion in mid-latitude regions and evaluate its impact on ozone long term trends. This is a challenging task, since the method depends on a correct evaluation of polar ozone depletion available only in several winters in the 1990s and on several assumptions (e.g. no dilution prior to the vortex break up date, small effect of photochemistry in April, lack of polar ozone

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loss in some winters, effect of Pinatubo aerosols, Ě). In that respect, the paper is certainly lacking a precise evaluation of the uncertainties due to the various assumptions. This lack of precision is also noticeable in the trend section since the estimate of ozone loss at mid-latitude due to dilution is used in the ozone trend calculation. In fact no error bar is given in the paper, which undermines the presented results.

The trend section is rather confuse, without a clear explanation on how various parameters are used (e.g. the ECMWF 250 hPa geopotential) and why. To my opinion, the use of linear trends is not justified at present and should be replaced by an estimation of the residual effect on ozone over a certain period (1979-1997 or 2002 in this case, once the “known” variability due to e.g. the solar cycle or the QBO is removed. In order for the results to be presented in a scientific way, this section should be rewritten in order to mention the effect of uncertainties in the parameters used as well as the correlation effects.

The paper is written in a relatively fuzzy way, without specifying the assumption and the formula used for the calculations. So it is rather difficult to read and understand. In addition, the structure should be revised in order to provide the necessary information at the right place, e.g. explanation of the longitudinal effect of the dilution by Figure 3 and explanation of the break up time of the vortex.

Detailed comments

Introduction

The authors show a correct knowledge of the recent literature on the subject (although the Millard et al. study published in JGR is lacking), but they should be more specific on the previously estimated effect of ozone loss dilution.

P1794, L. 25: Since change in the supply of ozone in spring due to planetary wave depends mainly on the latitude, the author should be more specific in the longitudinal aspect of circulation changes.

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The authors should give an estimate of the effect of dilution in March, since this will affect the mid-latitude ozone amount in the remaining part of the year. Overall error bars need to be provided on all the dilution estimates.

P1796-L12: "Part of the depletion Ě": explain this sentence.

P1796-L13: The explanation on how the break-up time of the vortex is determined should be given here and not in section 3.

Figure 1: Provide a physical explanation why the dilution is mainly over Scandinavia and Russia. Since the mixing is occurring at all longitudes, the effect of dilution should be zonal. The longitudinal effect may be due to the fact that the vortex remnants are still close together, depending on the mixing time scale. This is shown by Figure 3, which should be introduced here.

P1796-L25: The effect of photochemistry should be estimated in a more systematic way and error bars should be given.

Figure 2: This comparison to individual soundings gives the feeling that the method works but it is not quantitative. Do the authors see the same agreement for other stations? Local effects can also exist in ozone sounding measurements, so a statistical analysis of the bias would be a better tool to evaluate the accuracy of the method.

P1797-L6: The authors completely neglect the effect of the Pinatubo aerosol in this study. In 1991/92, the Arctic ozone loss was one of the largest as shown from Match campaign results (see WMO 2002 ozone assessment). In 1993/94, it was also quite large, so the estimate for these particular winters should be included in the study. In addition, the effect of Arctic ozone loss in the late eighties has to be estimated for the trend study.

Section 3

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P1797-L25: Explain the sentence: "Hereafter the equivalent latitude of the edge Ě". Are equivalent latitudes used in the paper?

P1798-L3: I have problems understanding the fact that vortex remnants are defined by an upper value of PV.

P1798-L7: It is not clear what is calculated here: frequency of the occurrence of the vortex? General remark: Could the frequency of occurrence include real vortex location in March? The authors should provide more explanation on how this frequency is calculated.

Section 4

General remark: the figures in this section are difficult to read. The authors should consider a different layout.

P1798-L25: A short explanation on how the QBO and solar cycle are handled is needed here.

P1799-L1: The trends are given in %. Is it %/decade?

P1799-L6: Explain why the geopotential at 250 hPa is used as a proxy for circulation changes. Is there a correlation between the geopotential height change and the dilution effect? In addition, since the linear ozone trend is not used in the model, it is difficult to understand the units of Figure 4 and 5 (trends in %/decade). Could the authors be more specific?

P1799-L14: Here again the wording is not precise enough. I suppose that 32% correspond to the square of the correlation value? Does the geopotential height change explain 32 % of the trend or 32% of the longitudinal difference in trend? In the latter case, the authors should explain how this result is obtained.

Section 5

P1800-L22: The sentence is not very scientific. What is causing the local ozone deple-

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tion (photochemistry, transport)?

P1800-L25: The authors should be more specific in what they are doing in this section and the rationale: Is the dilution effect evaluated twice (it is embedded in the TOMS measurements from which the dilution is again subtracted)?

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 6, 1793, 2006.

ACPD

6, S770–S774, 2006

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