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Atmospheric Chemistry and Physics Discussions

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

S. B. Andersen and B. M. Knudsen

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We thank the reviewers for the comments, and appreciate their suggestions for how we could clarify our presentation of this work

The most important changes to the paper are that we have extended the discussion of uncertainties and tried to clarify the trend section by including formulas for the models we have used. Finally figure 3 displaying the vortex positions has been changed.

Below answers to specific comments of all three referees may be found. Referee comments are in bold.

Anonymous Referee 3 Received and published: 24 April 2006 General com-

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Interactive Discussion

Discussion Paper

ments: This paper is an extension of paper by Knudsen and Andersen, Nature, 2001. Compared to their previous work, authors added ozone dilution for years 1995, 1996, and 2000 to the statistical model, calculated positions of vortex remnants, and also estimated the effect of dilution on the zonal mean trends. Quantitatively, first of the estimates provided by the authors (391979-2002) seem to be highly uncertain due to several unaccounted factors mentioned by the authors themselves (BD circulation, mid-latitude in-situ ozone depletion). The second ones are probably more correct but also suffer from the ignoring of depletion in other winters. However, the model explains quite a significant fraction of the longitudinal differences in trends that lends more credibility to the calculations. Also, I find information on geographical distribution of dilution (shown in Figure 1) valuable on its own and therefore worth publishing. In summary, I recommend the paper for publication after consideration the minor comments listed below.

Specific comments: Page 1796, line 1 "The ozone depletions in 1993, 1995, 1996, and 2000 were taken from Rex et al. (2004) while the ozone depletions from 1997 were taken from Knudsen and Gross (2000)." Why you do not take all depletions from Rex et al. (2004) for the sake of homogeneity? Does your depletion differ from that by Rex et al.?

In 1997 the longitudinal differences in dilution are largest and therefore it is most important to get the dilution this year correct. The Knudsen et al. (1998b) calculation includes the effect of mixing of air into the bottom of the vortex and is therefore used instead of the Rex et al. (2004) value. The Knudsen et al. (1998b) depletion is 79 DU, whereas the Rex et al. (2004) one is 59 DU.

Page 1796, line 5 "The ozone depletions were regridded every 7th day." Since you have different starting dates do you also have different ending dates or do you run last calculation for less than 7 days?

After each 7 day period the starting (in time) point of the next RDF calculation is given a

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mixing ratio of depleted ozone, which is bilinearly interpolated between the four closest endpoints of the previous calculation. This is added in the paper.

Page 1796, line 15 How do you get averages for April and May? Do you calculate mean values of all regridded depletions? Please, clarify.

Yes we get averages by calculating mean values of all regridded depletions. This is added in the paper.

Page 1797, line 24 "When the average wind speed at the edge of the vortex gets below 15.2ms-1 for at least two consecutive days the vortex is considered broken down." Do break up dates in years 1993,1995,1996,1997 and 2000 are those listed in Table 1?

The break up dates are not the ones listed in Table 1. The calculations were started when the vortex started to diminish in size which is earlier than the breakup date. This has been clarified in the paper

Page 1798, line 12 Figure 3 needs further discussion in regard to the differences between left and right columns. I would also suggest drawing the continents.

The preferred vortex locations are very similar in the winters with the largest ozone depletion compared to all other winters. One difference is that the winters with large ozone depletions generally have larger vortices and therefore the magnitude of the frequency is larger than for the other winters. New figure 3 has been made.

Anonymous Referee 1 Received and published: 27 April 2006 The paper is amending findings from an earlier study published by the two authors in Nature. Even though uncertainties in this study are large the authors attempt to duly discuss the problems of their approach. Nevertheless some aspects of their methodology require further discussion before the paper should be published in ACP.

1) Why are the ozone depletions taken from different sources? Homogeneity in

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the methodology of determining ozone depletion seems mandatory to me.

In 1997 the longitudinal differences in dilution are largest and therefore it is most important to get the dilution this year correct. The Knudsen et al. (1998b) calculation includes the effect of mixing of air into the bottom of the vortex and is therefore used instead of the Rex et al. (2004) value. The Knudsen et al. (1998b) depletion is 79 DU, whereas the Rex et al. (2004) one is 59 DU.

2) The regridding approach requires further explanation. I couldn't quite understand how it was done in this study.

After each 7 day period the starting (in time) point of the next RDF calculation is given a mixing ratio of depleted ozone, which is bilinearly interpolated between the four closest endpoints of the previous calculation. This is added in the paper.

3) Building on the previous point, I feel that an earlier discussion of the uncertainties related to the chosen methodology would be helpful.

The uncertainties resulting from the advection is impossible to quantify, but are generally thought to be small with regridded RDF calculations. The biggest uncertainty probably comes from determination of the end-of-winter ozone depletions. Rex et al. (2006) used an upper limit of 20 DU uncertainty on the column depletion. The uncertainties on individual levels are even relatively larger. Discussion on uncertainty has been added to the paper.

4) Coming back to my point made in the access review: I guess it would be extremely helpful if the authors could be convinced to use only one type of map projection (no preference on my side), so that the reader can appreciate figures 1, 3, 4 and 5 fully. MAP projection

New figure 3 has been made.

5) Even though I agree that longitudinal differences in trend are very important and that RDF is an appropriate tool to use to look at those, I am feeling slightly

uneasy about the numbers presented in the conclusion. Again I would request a stronger emphasis on the uncertainties when summarizing the results. People tend to read abstracts and summaries/conclusions; I am slightly worried that the message provided by the conclusion as it stands is not the messages that should be taken away from this paper. I would request a stronger emphasis on the spatial distribution and inter-annual variability and less emphasis an eye catching absolute numbers (the same is true for the abstract).

Point taken. Thanks. The abstract has been changed and more discussion on uncertainties has been added to the paper.

6) On page 1800, line 25: I don't understand the sentence - the authors have 5 years and the EESC is not very well defined in its temporal development. What are the authors trying to say? That additional ozone depletion may influence their results? I believe this is true anyway, but how does this relate to a mean dilution of 9.8 with a substantial error bar?

Equation 3 has been added here to clarify the method we are using.

In summary I would request a much more thorough discussion of uncertainties in this methodology and a stronger emphasize on the spatial patterns before the paper is accepted for publication in ACP(D).

Point taken. Thanks. Uncertainties have been mentioned in abstract and discussion.

Being late with my review I had the advantage of already reading the assessment by reviewer 3 and can only support the different/additional points he is making. Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1793, 2006. S673

Anonymous Referee 2 Received and published: 5 May 2006 This is an interesting paper based on a previously used method to estimate the in- fluence 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 loss in some winters, effect of Pinatubo aerosols, ą E). 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

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be more specific on the previously estimated effect of ozone loss dilution.

Thank you for drawing our attention to the Millard et al study which has been included in the paper. Specific details on the previously estimated effect of ozone loss may be found in section 5.

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.

We replaced 'trends in the circulation' with 'longitudinal differences in circulation trends'. Added hereafter: 'Hood et al. (1999) thus found that 330K PV trends indicative of trends in Rossby wave breaking and quasi-stationary wave variability correlate geographically with trends in total ozone'.

Section 2 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.

As the vortex has not yet broken up the effect of dilution on midlatitudes in March will be small. Also the ozone depletion of March will be included in the April-May dilution. The uncertainties resulting from the advection is impossible to quantify, but are generally thought to be small with regridded RDF calculations. The biggest uncertainty probably comes from determination of the end-of-winter ozone depletions. Rex et al. (2006) used an upper limit of 20 DU uncertainty on the column depletion. The uncertainties on individual levels are even relatively larger. Discussion on uncertainty has been added to the paper.

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

The sentence has been rephrased and moved to the beginning of the section for clairity: "Part of the dilution is irreversible. The remainder is reversible due to for example movements of the vortex and vortex remnants".

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

The break up dates are not the dates mentioned here and listed in Table 1. The calculations were started when the vortex started to diminish in size which is earlier than the breakup date. This has been clarified in the paper

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.

With our definition of dilution, including both the ireversible mixing and the reversible movements of the vortex and remnants, the dilution will not be zonal. The physical explanation of why the dilution is mainly over Scandinavia and Russia is closely linked to the fact that this is the most frequent position of the vortex. This is mentioned in the paper.

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

We do not have a photochemical model and to persue an error of estimated 6

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.

Other stations have not been tried since this station had measurements both inside and outside the polar vortex and we therefore believe it to be fairly representative. A full statistical analysis of the bias would be out of the scope of this paper.

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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.

Arctic ozone loss in 1991/92 was not large due to the effect of Pinatubo aerosols as volcanic aerosols only reached high northern latitudes after the formation of the polar vortex. Ozone depletion in 1991/92 and 1993/1994 was significantly smaller than in the years used. However we have in the paper emphasized the fact that our estimate of the influence of dilution on midlatitude trends is a lower limit. A rough estimate based on numbers from (Rex et al. GRL 2004) shows that we underestimate the influence of dilutions by neglecting other years than the five used in the period after 1991 by about 20

Section 3

1797-L25: Explain the sentence: “Hereafter the equivalent latitude of the edge ą E”. Are equivalent latitudes used in the paper?

’After breakdown the equivalent latitude of the vortex remnants is kept constant, i.e. the area enclosed by the vortex remnants is kept constant. All other latitudes in the paper are geographical.

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

Should be lower value. Thank you.

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

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this frequency is calculated.

It is mentioned several times in the section that what is calculated and shown in figure 3 is the frequency of occurrence of the vortex and it's remnants. Thus frequency of occurrence includes real vortex location.

Section 4 General remark: the figures is in this section are difficult to read. The authors should consider a different layout.

They will be larger in the final version.

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

An equation describing the model used has been included in the paper.

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

Yes, it is %/decade. Thank you.

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 more specific?

There is very little correlation between the geopotential height change and the dilution. The regression coefficient is multiplied by the trend in the explanatory variable.

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

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Section 5 P1800-L22: The sentence is not very scientific. What is causing the

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local ozone deple- tion (photochemistry, transport)?

What was meant by local ozone depletion was homogeneous ozone depletion. Transport would not be local. Phrase has been changed.

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)?

Equation 3 is included in paper to clarify our method.

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