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Comment

## ***Interactive comment on “Simple measures of ozone depletion in the polar stratosphere” by R. Müller et al.***

**R. Müller et al.**

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### **Detailed Reply to Reviewer #1**

We have already responded to the central issue raised by reviewer #1. The purpose of this author's comment is give a point-by-point reply and report the changes made in the paper in response to the comments in the review.

The changes made to the paper in response to the reviewer's and the editor's comments, in our view, have led to a much clearer message of the paper. The paper now clearly states under which circumstances misinterpretations of established simple measures might occur. Further, the paper markedly recommends using the minimum of daily average polar ozone in March poleward of 63° equivalent latitude as an alter-

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native and strongly argues that ‘early vortex break-up’ years should not be considered in time-series of simple measures.

### Point-by-point response

In the following we cite the key points made by the reviewer (shown in *italics*) followed by our response. In some cases, we also cite additions to our paper made in response to the comments (shown in *slanted* letters).

*The authors recommend not using minimum of ozone as a measure of ozone losses. They present evidences that the minimum is often located outside the vortex and therefore can not represent chemical ozone losses. The fact that the minimum value can be caused by high-pressure systems and not by ozone depletion has already been pointed out by Knudsen (2002) so I do not see what is new here.* The point that Knudsen (2002) made was that “. . . the minimum ozone in the Arctic is often caused by high-pressure systems and not ozone depletion” [13]. However, this statement is made in a short comment to an ACPD paper without supporting evidence from observations or from references; note that Knudsen (2002) is not a peer reviewed paper. More importantly, our conclusion goes clearly beyond the point of Knudsen (2002) insofar as we quantify based on observations what “often” means; in 12 out of the 29 winters considered here the minimum ozone column occurs *outside* of the vortex where it clearly can have nothing to do with polar ozone loss driven by heterogeneous chemistry. And we investigate in how far these arguments carry over to model results. Further, as the reviewer remarks in his review, even the minimum ozone in the vortex might be influenced by tropospheric high-pressure systems. In view of this discussion, it is remarkable that the Knudsen (2002) comment is on the very paper [1] that *introduced* the use of minimum ozone as a measure of polar (driven by heterogeneous chemistry) ozone loss, a use that has continued since then in a number of important papers and assessments, for example [22, 9, 23, 6]. We believe that it is very important to make

the point that this metric should no longer be used.

Finally, Knudsen (2002) suggests that “maybe the [March] 63-90N average ozone would be a better yardstick” a suggestion that our paper does not support, for example because (as the reviewer points out in his review) there are winters when the vortex breaks up in March. Instead, our paper suggests a different alternative to using minimum ozone (see discussion below).

*Moreover, observing the minimum inside the vortex does not ensure that it was caused by ozone depletion. It can still be caused by high-pressure tropospheric systems. We agree (see also comment above). This is an additional reason why we suggest in our paper that minimum Arctic ozone should no longer be used.*

*On the other hand ozone-depleted air can be transported outside the vortex in filaments and contribute to ozone minima observed in mid-latitudes. We agree with the reviewer that this process occurs. However, when it does occur, in no way does it affect the chemical ozone depletion occurring inside the vortex. These processes are a consequence of the chemical ozone depletion and not a cause. As long as any metric of the severity of polar ozone depletion aims to capture, as best as possible (we recognise and appreciate the issues associated with the use of total column ozone for this task, see also below), the extent of chemical ozone loss, and confines itself to the region poleward of the vortex boundary, it will be unaffected by filamentation at the vortex edge and resultant transport of ozone depleted air to lower latitudes.*

*To prove the cause of ozone minima the authors should have analyzed more meteorological information including information at the tropopause level for each particular case. Such an analysis was done by Hood et al. (2001) for 71 extreme ozone minima and they concluded that “The data are therefore most consistent with a purely dynamical origin for extreme ozone minima in general...” I doubt that the present study adds something new. We agree that it is very important to link our work with studies in the literature on extreme low total ozone events and ozone mini-holes [14, 16, 10, 7, 2, 12].*

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Such a link, the corresponding discussion, and the appropriate references have been added in the revised version of our paper (see also below). However, the focus of our paper is on the *consequences* of low-ozone events for the design of metrics employed to compare CCM model results with observations; this aspect is not addressed in reference [7] or in any of the papers focusing on ozone mini-holes.

*Further the authors consider minimum of daily averages as a better simple measure because this quantity "both obviates relying on one single data point and reduces the impact of year-to-year variability in the Arctic vortex breakup on ozone loss measures". This is a very good point.* We thank the reviewer for this comment. Indeed we have, in the revised version of the paper, made a strong effort to get this point across more clearly. We believe that changes made in response to the reviewer's and editor's comments have led to a clear recommendation in the paper of the minimum of daily averages as a better simple measure.

*However comparing Figs. 10 and 11, I do not really see that this quantity shows substantially better correlation with  $V_{psc}$  than the March average. The only noticeable improvement in Fig. 11 in compare to Fig.10 is that winter 2004/2005 is better fitted to the relation. I believe it is impossible to make a solid conclusion based on one point only. My impression is that the March averages provide essentially the same information as minimum of March daily averages.* We have removed the comparison of the quality of a possible fit in Figs. 10 and 11 from the paper. Instead, as recommended by the reviewer in another comment (see below) we discuss now the reason for outliers due to early vortex breakup and the observed ozone column in the coldest winters. Moreover, the minimum of March daily averages is less affected by the year-to-year variability in the Arctic vortex breakup and temporal development of chemical ozone loss. This point has been made more clearly in the revised version of the paper.

*Also I do not understand why the authors chose to present the correlation of total ozone with PSC potential and not with ozone losses.* This is a good point. In the revised version of our paper we have now added a plot where we compare the two most com-

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monly used simple measures with chemical ozone loss deduced from ozone-tracer relations and contrast these relations with the correlation between chemical ozone loss and the new simple measure we recommend in our paper. We therefore add the following text: “. . . we compare chemical ozone loss between 380-550 K potential temperature in March deduced for the Arctic winters 1991–2005 based on the vortex average method [3, 18] with the two most commonly used simple measures based on total ozone, namely the average polar ozone poleward of geometric latitude 63° N in March and the minimum of all total ozone measurements in March poleward of geometric latitude 60° N [15, 1, 23]. [. . .] That means, the new simple measure suggested here shows the closest correlation of all simple measures with chemical ozone loss.”. This addition strengthens our recommendation of a new simple measure (see also comment below).

Anyway, the suggestion to use the equivalent latitude frame for calculation of simple diagnostics of ozone loss may be useful. The authors should decide if they want to propose such a measure. Presently the abstract is written so that I'm not sure if they recommend using it or not. Indeed our intention was to propose such a measure. The additions made to the paper in response to the reviewers and the editors comments, in particularly the two new plots (the last two plots in the revised version), have certainly strengthened our argument here. Further, we made an effort to get this point across clearly in the abstract and the conclusions now.

They also should point out, as was promised in the Introduction, under which circumstances the misinterpretation of such measures might occur. My impression from Figs. 10-11 is that in the Northern Hemisphere the largest deviations from linear relationship are observed during winters with exceptionally large ozone losses (1996, 2000, 2005) or when the vortex broke up early (1999, 2001, 2006). I recommend that the authors concentrate on these findings. Regarding the winters with exceptionally large ozone losses we have included the following statement in the paper now: “Obviously, the minimum ozone column in the three coldest winters (1996, 2000, and 2005) is not much

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lower than the minimum in moderately cold winters such as 1993 or 2003. This effect is mostly responsible for the nonlinear relation between  $V_{\text{PSC}}$  and the minimum of March column ozone". However, the point regarding winters with early vortex breakup is very important. We thank the reviewer very much for this suggestion! The issue of Arctic vortex breakup before March is now discussed much more thoroughly in the paper as suggested. It is mentioned in the introduction, the discussion, the conclusions, and in the abstract. For example, the following text has been inserted in the discussion of Figure 1: "Note that in some winters [1987, 1999, 2001, and 2006] [23, 19], the polar vortex broke up in February, so that the March mean total column ozone hardly provides any information on polar ozone loss".

*...is that measures based on total ozone do not differentiate between contributions from chemistry and dynamics.* We agree that measures based on total ozone do not, in themselves, differentiate between contributions from chemistry and dynamics. However, metrics, based on total column ozone, if constructed carefully enough will be more likely to be representative of the chemical ozone loss than others which might be more predisposed to dynamical influence. This is an underlying fundamental point of our paper.

*Title..* In view of the substantial changes made to the manuscript in response to the comments on the paper, we feel that the title now reflects rather well the content of the paper.

*Data: Mention which meteorological analysis is used to calculate equivalent latitudes for Figures 1-6 and 8-9.* In the ACPD version of the paper, we have used the ERA-40 analysis to calculate equivalent latitudes and the PV gradient at the vortex edge, whereas ECMWF operational analyses were used to calculate  $V_{\text{PSC}}$ . In the revised version we will use ECMWF operational analyses consistently. We will therefore add the following sentence: "Throughout the paper we use ECMWF operational analyses for the calculation of meteorological quantities." The only exception is Figure 7 where NCEP/NCAR reanalyses are used. This figure has now been removed from the paper

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(see also below).

*Figures: In winters 1999, 2001, 2006 the Arctic vortex broke up before March (author's own statement). Does it make sense to show March ozone inside vortex for these years (Figures 1,3,4,8,10,11)?* Arguably, it does not make sense to show March ozone inside vortex for the years 1999, 2001, 2006, and indeed 1987, when the Arctic vortex broke up before March. On the other hand, we follow here only the practice of recent assessments [22, 9, 23] where rather similar plots were shown. However, in response to the reviewer's comments, this issue is now discussed much more thoroughly in the paper (see also discussion above) and it is pointed out in the conclusions and in the abstract that those 'early break-up' years should be excluded from the analysis when using the minimum of daily average ozone in spring, the measure we are proposing here as an alternative to established measures. Indeed, the last figure of the revised version, where the early break-up years have been removed from the time-series presents a rather different picture than the customarily shown plot of March polar average ozone that includes early break-up years.

*Section 3.2.1: To my understanding ozone minima discussing here can be linked to "ozone miniholes". If yes, then the references to literature on ozone mini-holes would be relevant (e.g. McKenna et al. 1989; Hood et al, 2001; James and Peters, 2002; Peters et al. 1995; and references therein). We agree. It was an oversight to not include a discussion on ozone miniholes. In section 3.2 we have now introduced the following discussion: "Spatially localised and transient (several days) reductions in column ozone, so called miniholes, are frequently observed; it has been established that they are of dynamical origin [14, 16, 10, 7, 21]. The dynamics of miniholes involves a lifting of the tropopause above a tropospheric anticyclone and poleward motion of ozone poor subtropical air in the upper troposphere and lower stratosphere. In combination, this causes a reduction in column ozone [17, 7]. The dynamics of mini-holes further results in equatorward excursions of polar air in the mid-stratosphere; in a situation when this air is chemically depleted by halogen chemistry in the polar vortex, particularly*

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low ozone miniholes may occur [10, 21, 12]. Furthermore, the frequency of low-ozone events is changing with time; Broennimann et al. [2] report that in winter, low-ozone events were much more frequent in 1990–2000 than in 1952–1963 over northwestern Europe”.

P9840, L24 Note that Karpetchko et al., (2005) reported the climatological Arctic vortex edge at about 69 for 15 March (as can be judged from their Fig. 3) and not for 1 April. During the second half of March the vortex shrinks and therefore the difference between observed vortex edge and simulated one may be smaller. We have added the information that the vortex edge reported by Karpetchko et al. (2005) is valid for mid-March. However, Fig. 7 in the ACPD version of the paper (that has been removed from the paper as suggested by the reviewer and the editor, see below) shows that the situation regarding the climatological vortex edge does not change substantially in early April. We have added the following sentence: “*The fact that the size of the Arctic vortex in E39/C is smaller than in reality is highlighted in the electronic supplement, where the strength of the barrier to meridional transport is compared for analysed meteorological data and results from E39/C [4] on the 550 K surface (averaged over the years 1990–1999 and for 1–10 April)*”.

Last two paragraphs in Sect.3.2.2: These two paragraphs discussing differences between vortex size in model and in analysis are not linked to the rest of the paper very well. I recommend avoiding them as well as Figure 7. In response to this recommendation we have removed the two paragraphs and the corresponding figure from the paper. We have however retained them in the electronic supplement as we believe that they support our arguments based on the Karpetchko et al., (2005) study [11] (see response to comment on P9840, L24. above).

P9844, L10 It is not clear to me why PFP should be used instead of  $V_{psc}$  for comparison of Arctic and Antarctic. In fact I do not understand the idea of PFP at all. If you have two vortices different in size but having the same PFP why do you expect that ozone losses are the same? Wouldn't the activated volume (and therefore ozone losses)

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be larger in the case of larger vortex? If the absolute activated volume is larger in one particular winter indeed ozone loss in the sense of an ozone mass deficit would be larger. However, depending on actual the vortex size, rather different *mean-vortex* ozone loss values could be calculated. Therefore, mean-vortex ozone loss should be related to the fraction of the vortex that is activated. The fraction of the vortex that potentially can be activated is described by the PFP. We have extended the explanation in the paper. This section of the paper reads now: “*VPSC is an absolute measure and is therefore strictly comparable only with absolute measures of ozone loss like the ozone mass deficit [8]. Vortex mean ozone loss should be related to the fraction of the vortex that is activated. This effect becomes particularly important if Arctic and Antarctic conditions are compared, where the vortices have rather different sizes. Therefore, Tilmes et al., (2006) extended the concept of VPSC introducing the PSC Formation Potential of the polar vortex (PFP), a measure that takes into account the size of the vortex*”.

P9844, L15 The concept of PFP was introduced only last year. It is not really correct to refer to it as “the well-known fact”. We agree. This sentence has been changed to: “The well known fact that winter/spring temperatures are lower in the Antarctic than in the Arctic (leading to a greater PFP [20]) and that polar column ozone is greater in the Arctic than in the Antarctic [5, 23] are reflected in this plot”.

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