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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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Müller et al. investigate applicability of simple measures of stratospheric ozone depletion. The authors discuss scientific questions related to ozone losses which are within the scope of ACP. The paper is written in a good scientific language. However the authors mainly arrive to the conclusions which are not new and therefore the scientific value of the paper is not clear to me. I also disagree with some of authors' interpretations of their results. The main problems are listed below.

The authors argue that sophisticated measures of polar ozone losses should be used where possible instead of simple ones based on total ozone column observations or simulations. I do not see anything new here. Indeed, total ozone column alone does not provide information on chemical losses. In other words, low total ozone does not necessarily result from enhanced chemical destruction but may be caused by dynamical factors. This fact, I believe, is a well known one.

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. 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. On the other hand ozone-depleted air can be transported outside the vortex in filaments and contribute to ozone minima observed in mid-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.

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. However comparing Figs. 10 and 11, I do not really see that this quantity shows substantially better correlation with Vpsc 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.

Also I do not understand why the authors chose to present the correlation of total ozone with PSC potential and not with ozone losses. If the idea of Figs. 10-12 is to demonstrate that measures based on total ozone do not adequately reflect ozone losses then the authors should show the correlation of these measures with ozone

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losses and not with PSC potential.

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. 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. Presently, the main output of the paper, as can be judged from the Abstract, is that measures based on total ozone do not differentiate between contributions from chemistry and dynamics. As I said before this is not new and hardly deserves publication. Therefore, I cannot recommend the manuscript for publication in its current form.

Other comments:

Title: From the title one may get an impression (as I did) that the authors are proposing new measures of ozone depletion. In fact, in the present version they are mainly criticizing the existing ones. I recommend that the authors consider rephrasing the title.

Data: Mention which meteorological analysis is used to calculate equivalent latitudes for Figures 1-6 and 8-9.

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)?

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,

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Peters et al. 1995; and references therein).

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.

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.

P9844, L10 It is not clear to me why PFP should be used instead of Vpsc 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) be larger in the case of larger 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".

Technical comments:

Figure 5: On display, box-diagrams appear as solid-shaded with no quartiles and medians visible.

Figure 6: The figure looks OK on display, but, when printed, shading which denotes vortex is hardly visible.

References:

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McKenna, D., Jones, R. L., Austin, J., Browell, E. V., McCormick, M. P., Krueger, A. J., and Tuck, A. F. (1989), Diagnostic studies of the Antarctic vortex during the 1987 Airbourne Antarctic Ozone Experiment: Ozone mini-holes, J. Geophys. Res., 94, 11 641-11 668.

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