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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Influence of transport and mixing in autumn on stratospheric ozone variability over the Arctic in early winter” *by* D. Blessmann et al.

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Dear reviewer,
thank you for reviewing our paper and your helpful comments!

General comments

- Point a: “More discussion of interannual variability of Arctic ozone in autumn and early winter” and “In addition to showing the standard deviation only at one level for the four months, it would be interesting to provide these numbers as a function of altitude and for a longer period from summer to winter or even spring.”

In response to your comment, we have calculated the standard deviations not only for 550 K and September to December, but also for 450–800 K and August to March (this is the time period for which ozone data are available in our merged data set). Figure 1 (of the author comment) shows the interannual standard deviation of the monthly and vortex means of August to March as a function of potential temperature. The “vortex” is defined as the area north of 65 degree equivalent latitude to be comparable to Figure 1 (of the paper). The interpretation of this figure is complicated by the fact that the variability subsides with the downwelling in the polar vortex (in altitudes where the chemical lifetime of ozone is long enough).

Figure 2 (of the author comment) shows the same profiles as before, but now corrected for the typical amount of subsidence in the shown months (for simplicity, the mean subsidence in the vortex in the year 1999/2000 has been used). This can only be a first-order correction, since subsidence is different from year to year, and there is no exact point-to-point relationship between two points on different profiles. In addition, only the months from December to March are shown, since there is no easy measure for the subsidence when the vortex is weak and meridional mixing is strong. The standard deviation is plotted here as a function of the “spring equivalent potential temperature”, i.e. the potential temperature a material surface would have in spring (March). The ozone data set tends to get unreliable above 800 K, so a part of the profile is missing for the earlier months, where the material surfaces are higher up in the atmosphere.

The figures show that the variability at the beginning of winter and in autumn is low compared to the winter and spring season, but is in the same order of magnitude. Starting in December, a maximum (in potential temperature) in the standard deviation begins to appear, subsiding from 800 K in December to 500 K in March. The maximum shows standard deviations of about 0.4 ppm. Typical values in autumn are 0.1 ppm to 0.2 ppm. There is no monotonic and steady increase at all potential temperature levels, but in general, values increase from about 0.1 ppm in September to about 0.2 ppm in November and December.

With regard to these results, we changed two things in the paper: We now average the standard deviation over a vertical range of potential temperatures to avoid that the trend in the values is only a result of chance, and we mention the 0.4 ppm standard deviation in winter to put these numbers into context.

The results for the standard deviation are certainly very interesting, but we would prefer not to add additional figures and discussion. In the moment, this is a nicely focussed study and we think there would be too many unrelated topics outside the scope of this study introduced to the manuscript. The figures need some lengthy explanation and open up some additional questions, and we have to stop somewhere.

- Point c: “More discussion on the Eliassen Palm flux during the formation phase of the vortex”

  Please see the answer to your specific comment on page 15084, line 14.

- “How strongly is early winter ozone variability influenced by long-term changes in ozone depleting substances?”

  We agree that this is an important issue. You say that “This is difficult to see from Fig. 1, but principally the information is there”. Actually, one of the time series *is* shown in the paper. The time series corresponding to Figure 1 (at 550 K) is shown in Figure 6 and 7. The figures show that the variability is mainly due to interannual variability and that there is no significant trend. This is also true for other potential temperature levels, since the deviations of the ozone profile from the long-term mean in different altitudes are correlated (i.e. the profile often shifts as a whole from year to year).

  Trends are low according to WMO (2010) in the considered time period since the

  chlorine loading of the stratosphere reaches its peak in this period and starts to decline again. Typical trends are in the range of a few percents per decade.

  In addition, trends in EP flux and the origin tracers are similarly small. Most of the trends are not significant on the 95% level.

  To make sure that the trends have no influence on the correlation coefficients, we calculate all correlation coefficients from detrended data now. Changes are small, and in most cases, the correlations are even a little bit better than before. We changed all figures and the correlation coefficients mentioned in the text accordingly. In addition, we added a note that the trend due to the change in ozone-depleting substances is small.

- “As it is argued that the wind reversal during the vortex formation period is critical for the propagation of planetary waves and subsequent mixing, it would be helpful to provide also more details for this aspect.”

  The wind reversal typically takes place in early September. There is not much interannual variability in the date of the reversal (less than 2 weeks), i.e. the reversal is mainly driven by the seasonal change in radiation. Since there is not much variability in the wind reversal, there is also no correlation to ozone. Taking this into account, we don’t think that a more detailed discussion is needed.

- “There are a number of figures that do not carry much information . . . ”

  Figures 9 and 14 are quite complex figures and Figures 6, 7, 8 and 13 are mainly in the paper to explain in several steps what is shown in these figures. In addition, Figure 6 and 7 give you a feeling how the time series actually look like. To delete these figures and the accompanying text would make it unnecessarily difficult to understand what has been done.

Specific comments

- Page 15084, line 14: You are right. We have changed the introduction to “In the lower stratosphere, this quantity correlates well with the origin of air enclosed in the vortex and reasonably well with the ozone amount in early winter.”

  We think the main factor for the relatively low explained variance is that it is difficult to put all of the complex dynamics that occurs over several months in the complete stratosphere into only one quantity. EP flux is used intentionally here as a single-valued proxy for complicated dynamical processes and to show up the basic relationships. Integrated EP flux can only be a surrogate for looking at the complete dynamics and the same EP flux (in different years) can have very different effects on the dynamics. It is certainly important for the exact transport and mixing pathways at which latitudes and altitudes the waves break and what the wave numbers are, and so on. These are all things which cannot be deduced from the integrated EP flux.

  In addition, while the EP flux through a level is directly associated with the residual circulation at this level (i.e. the subsidence w^* in the TEM sense), meridional mixing is associated with the wave breaking (i.e. the EP flux divergence) at a particular level. Since the EP flux divergence is more prone to noise in the data than the EP flux, and since both subsidence and meridional mixing play a role, we use EP flux as a compromise here.

  We added some additional discussion of this to the paper in Section 5. We don’t want to discuss the dynamics of vortex formation in more detail than currently done. While this is certainly very interesting, we think it is outside the scope of our study. The study is short and focussed, and a closer look at the dynamics would justify a comprehensive study of its own.

- Page 15085, line 8: You are right. Deleted “. . . and then stays constant”.

- Page 15086, line 4: “If the sensitive period lasts from middle of September to middle of November, why is the Eliassen-Palm flux averaged over the period August to November?”

  In fact, averaging the results from September to November does give almost identical results, since there is almost no wave activity in August. It makes more sense to use the period September to November in the paper. We have now changed the text and figures accordingly.

- Page 15086, line 4: “How robust are the results to this particular choice?”

  We repeated the calculations for Figure 13 and 14 using the following time periods: Sep–Oct, Sep–Nov, Sep–Dec. Aug–Nov (the period used in the paper) and Sep–Nov are nearly indistinguishable (see above). If November is left out (Sep–Oct), the correlations get worse (maximum of 0.5), but show the same general pattern. If December is added (Sep–Dec), correlations are very low (maximum 0.25). This is probably due to the fact that the vortex is well established by December and that meridional mixing plays a smaller role. Similar results hold true for Figure 15.

  We added a short discussion to the paper.

- Page 15086, line 4: “Can you provide more details on the timing of the vortex formation period?”

  While we have studied several aspects which are not shown in the paper (e.g. amplitudes of the waves 1, 2 and higher as a function of time and altitude for all years) and there are certainly interesting results, we think a more detailed discussion is outside the scope of the paper (see comment on page 15084, line 14).

- Page 15087, line 7: After 2005, the data set is solely based on ozone sonde data, and before 2006, it is mainly based on satellite data. Since that means that the data after 2005 are more noisy, we wanted to avoid additional complications and explanations and restricted the figure to the years with satellite measurements.

- Page 15088, line 2: Typically, the zonal and monthly mean of the ozone sondes agrees within about 0.4 ppm with the mean of the combined satellite instruments below 800 K. Above 800 K, the satellites show values systematically higher by up to 1.5 ppm, which is probably a problem of the sonde data. Ozone data above 800 K are not used in the paper.

  Changed the sentence to “agree within 0.4 ppm”

- Page 15088, line 15: You are right. Changed as suggested.

- Page 15093, line 5: This is a good point. You are right, this is only true for the residual circulation (i.e. w^*). For mixing, waves breaking above or below the level we are looking at have no direct influence on mixing (they may have by changing the mean flow, but that probably leads too far here). Deleted “and meridional mixing” from the sentence. See also answer to your comment on page 15084, line 14.

- Page 15094, line 22: We would like to keep it as is. The current items in the list follow the structure of the paper and your suggestion would mean not only to rewrite the first item, but also the second, without any significant change in the meaning. We don’t want to imply any causal relationship here, we hope “connected” is neutral enough. We just wanted to avoid writing “correlated” over and over again.

- Figure 15: “Early winter” is used as a fixed definition throughout the paper and is defined at the start of Section 3 (30 Dec to 8 Jan). The formulation is also used in Figs 8, 9, 10, 13, 14 and on numerous occasions in the text to keep the explanations short and clear. If you think that it is more confusing this way,

we would be happy to change that, but we would like to change it consistently throughout the paper.

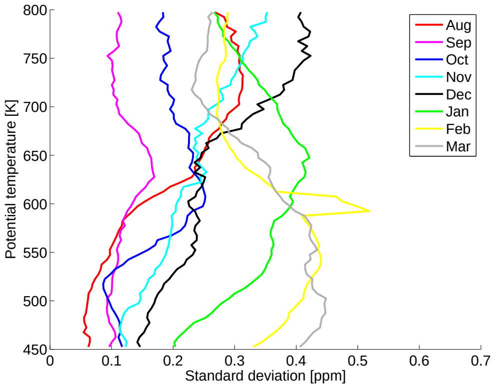
Technical corrections

- Page 15086, line 10 and page 15083, line 4: Changed as suggested.
- Page 15086, line 23: Corrected.

Corrections by us

- Figure 11: The axes of the figure were not scaled correctly and some of the data points were outside the visible area. Corrected.
- The citation on page 15085, line 6 should have been Kawa et al. (2005) and not Kawa et al. (2003).

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15083, 2012.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750
700
650
600
550
500
450
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
Standard deviation [ppm]
Aug
Sep
Oct
Nov
Dec
Jan
Feb
Mar

Fig. 1.

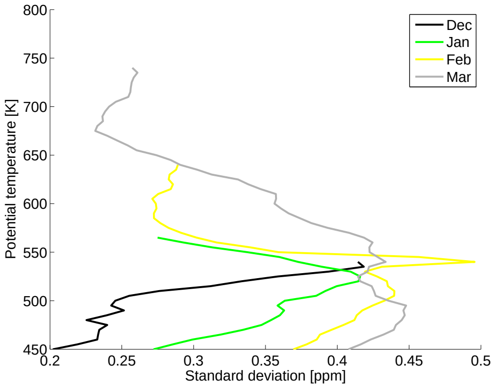
Potential temperature [K]
800
750
700
650
600
550
500
450
0.2
0.25
0.3
0.35
0.4
0.45
0.5
Standard deviation [ppm]
Dec
Jan
Feb
Mar

Fig. 2.