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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.

D. Blessmann et al.

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

General comments

- "Why should we care and what are the significant implications for assessment of stratospheric ozone and its changes?"

  Ozone is the most important trace gas in the stratosphere and plays a large role in stratospheric chemistry, the radiation balance and for the UV radiation on the

  ground. We think it is therefore necessary to understand *all* aspects of the variability of the ozone layer. This is also important for understanding the future development of the system in the context of anthropogenic ozone depletion, global warming and dynamical changes. E.g. it is obvious from our study that a future change in wave driving would also influence ozone mixing ratios in the forming polar vortex.

  It is important in our opinion not only to understand anthropogenic ozone depletion with its direct political and social impacts, but also issues like ozone variability in autumn.

  "If, as the intro states, uncertainties remain in understanding the quantitative effect of dynamical processes on the high latitude ozone layer, then what is the range of the uncertainty, in which processes . . . "

  This is no review paper and it makes no sense in our opinion to discuss dynamical aspects unrelated to the topic of this study. The dynamical processes relevant in the context of the paper are discussed.

- "What have we learned new here?"

  We show for the first time from observational data that the origin of air in autumn is correlated with the ozone amount in the early vortex and with the wave activity in autumn. We are not aware that such a study has been done before. In particular, we are not aware that a conceptual diagram like in Figure 12 has been derived from measured data so far. If you think that this is "pretty common knowledge", please give the references that you have in mind.

  There are only very few papers which have studied the development of the vortex in autumn and ozone mixing ratios in autumn. Kawa et al. (2003) is a case study for the autumn 1999 and Tilmes et al. (2006) is a case study for 2003, while our study comprises 18 years. Kawa et al. (2005) show data on the variability of early winter ozone, but discusses the correlation of November ozone and March columns, which is outside the scope of our paper. The only study showing results related to our paper is Rosenfield and Schoeberl (2001). However, the study only shows results for 10 years, has a completely different focus (forward vs. backward trajectories) and due to its age, uses UKMO analysis data, which is certainly inferior to ERA Interim. We have cited and discussed all these studies in the appropriate places.

We are not aware of any other studies using observed data to study the vortex and ozone development in autumn. If you claim "several places in the text summarize the bottom line of the analyses as already shown by others" it is not very helpful if you don't give the references that you have in mind or don't give arguments that proof your claim. It would be very helpful if you could be more specific here. That would help us to give a substantiated response.

We would like to keep the study short and focussed. We hope that this in your sense, since you state that the paper is "well-written and organized".

Specific comments

- "A good addition to this paper would be to test the assertions regarding the realism of the model simulation in Section 2.2 by tracking observed ozone"

  If we understand you correctly, you propose to replace the initialization of the 117 tracers by the average ozone mixing ratio (in the domain where a particular tracer is 1). Equivalently, one could calculate vortex mean ozone by multiplying the fraction of vortex air originating from a domain marked by a particular tracer by the ozone mixing ratio measured in this domain on 1 September and adding up over all tracers.

  We don't think it makes sense to do these sort of calculations. The effort necessary would not justify the negligible benefit.

The questions that such a calculation could answer already have been answered and discussed in much detail in two papers that are cited in our paper: The model validation paper (Wohltmann and Rex, 2009) and the companion paper on the chemical lifetime of ozone in autumn (Blessmann et al., 2012, which we have now cited a second time in the introduction). The methods used in these papers are much better suited to answer the questions of the quality of the model validation and on the chemical lifetimes, since they were specifically designed to answer these questions.

The model validation paper shows that the combination of the ATLAS model with its transport and mixing algorithm and ERA Interim does a very good job in reproducing observed tracer fields and tracer-tracer correlations. There is no reason to cast doubt on the quality of the model simulation, maybe except for the fact that the model was only validated with data from 1999 and 2000 in the model validation paper, and that the quality of the ERA Interim data could change with time. However, in the meantime, we have also validated the model with data from the winters 2009/2010 and 2010/2011, with very similar results. It is not very probable that the agreement between model and observations is worse for just the additional model runs which are shown in this paper. Further validation with ozone as a non-conserved tracer (compared to the well conserved tracers methane or Halon-1211 used in the validation paper) does not add any additional insight. One could never be sure if deviations to the observations are caused by chemistry or by deficiencies in ERA Interim or the transport algorithm.

You also claim that one could decide if the model "produces a realistic balance between mixing and descent" from transport calculations. We are not of that opinion: In the trace gas fields, you only see the combined effect of descent and mixing, without any chance to disentangle these two effects based only on comparison to observations. E.g., a too strong descent can be cancelled by too strong mixing over the vortex edge in CH_4 and N_2O fields.

The question "if chemistry matters over the time frame of the simulation" is answered in detail in the companion paper (Blessmann et al., 2012) with a much more direct method. If we would see differences between observed and modeled passive ozone in the method you propose, one could not be sure if that is caused by chemistry or by transport and mixing.

- Page 15091, lines 22–25: We are not completely sure that we understand what you mean. We have rephrased the sentence. It now reads "... higher vortex mean ozone mixing ratios in early winter are typically correlated with a higher abundance of air from lower initial potential temperature levels ... " to remove any ambiguity that not the origin of air masses in potential temperature but the potential temperature itself was meant. We hope that solves the issue.

- Page 15092, Figure 12: "Associations between subsidence and wavedriving and mixing are not precise"

  There was some unnecessary confusion here in regard to the use of the word "subsidence", which was not used consistently. The word is used with two different definitions here. E.g., on page 15085, line 22 (and in most other studies), it is used in the sense of the Transformed Eulerian Mean, i.e. the zonal mean subsidence you would have at a fixed position in latitude and pressure. On page 15086, line 4–6 and on page 15092 it is used in a Lagrangian sense ("the subsidence that the air experienced", "the net subsidence"), in the sense of the net subsidence along the history of an air parcel.

  These two definitions can lead to very different results. E.g., years with a strong Brewer-Dobson circulation (defined by the EP flux here) show strong subsidence in the TEM sense at 550 K, as expected. Figure 1 (of the author comment) shows a positive correlation of EP flux (defined as in the paper) with the mean residual velocity $-w^*$ in Aug–Nov north of 60 degrees N at 550 K. In contrast, EP flux is also moderately positively correlated with the fraction of air originating from 550–

650 K on 1 Sep (Figure 2 of the comment). In fact, there is no very pronounced relationship between the subsidence in the TEM sense and the net Lagrangian subsidence (Figure 3 of the comment).

A strong Brewer-Dobson circulation is correlated with strong mixing and on average, the air will have its origin in more southern latitudes and will have spent more time there. Since there is either upwelling or weaker downwelling in more southern latitudes, the net effect is apparently (from the results of our calculations) that there is less "net subsidence" along the path of an air parcel, even though there may be more subsidence (in the TEM sense) in higher latitudes.

That should have been defined and discussed somewhere in the paper. The manuscript has been changed accordingly, and we are now more precise on this topic. A short explanation has been added and we have changed "subsidence" to "net Lagrangian subsidence" where necessary (e.g. in the caption to Figure 12).

"Preference for one pathway or another is not necessarily associated with lower or higher net subsidence"

We are a little bit confused. The low and high subsidence is part of the definition of the pathways, so that is obviously a wrong statement. We assume that you mean "Preference for an origin in more southern or northern equivalent latitudes is not necessarily associated with lower or higher net subsidence". This is certainly true for a statistical relationship as long as you retain the word "necessarily". If you would say "Preference for an origin in more southern or northern equivalent latitudes is not associated with lower or higher net subsidence", we would disagree. Figure 11 shows that at least at 550 K, this is just the opposite of what we observe.

"It is likely due to varying contributions from different wavelengths breaking at different altitudes"

We certainly agree. We don't necessarily see a contradiction here to the first part of this sentence or to anything written in the paper. Lower or higher subsidence is related to the overall amount of wave breaking above a level, so if that is a contradiction or not depends on if we are looking at particular events here or at the overall statistics. Maybe some of the confusion here arises from the fact that while meridional mixing at a particular level is associated with the wave breaking at that particular level (i.e. the EP flux divergence), the strength of the residual circulation (i.e. w^*) at a particular level is associated with the EP flux through this level (i.e. the divergence at all levels above that level). Since both meridional mixing and subsidence play a role in determining trace gas mixing ratios, this probably needs some explanation to avoid confusion. In particular, using the EP flux and not the EP flux divergence here is a compromise between a good proxy for subsidence and a good proxy for mixing. In addition, EP flux divergence is more difficult to calculate from noisy data. We added some discussion of this to section 5. We also changed the sentence on page 15093, line 5, by deleting "and meridional mixing", since this was obviously wrong.

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.

"Overall, higher wave driving leads to more subsidence, which is the nature of the Brewer-Dobson circulation. Resulting vortex ozone is balance between mixing and subsidence."

We agree. Similar statements are made at several places in the paper. Additionally, we have now added "This is in line with the general accepted mechanism that vortex ozone results from a balance of mixing and subsidence and the strength

of both subsidence and mixing are determined by the amount of wave breaking in the stratosphere" to the introduction.

"... (and apparently contradictory to P. 15086, lines 4-6)"

We do not understand why you think that page 15092 is contradictory to page 15086. It is neither stated here how strong or weak the subsidence is, nor it is stated in which way the quantities subsidence and ozone are correlated (e.g. positively or negatively). If there is no statement, there can be no contradiction. Do you mean that the statements in the paragraph on page 15092, line 12–24 are obviously contradictory to the well-known fact that a strong Brewer-Dobson circulation is connected to high subsidence in the TEM sense, and vice versa?

- Figure 13–15: If you state that this is already known from previous work, it would be helpful to cite some of the references you have in mind here. As long as we have no literature to refer to, we will assume that these two figures are a valid contribution to the paper.

  The correlations between ozone and EP flux are less convincing than other correlations shown in this paper. 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 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. We have now added some discussion to section 5, see comment above ("It is likely due to varying contributions from different wavelengths breaking at different altitudes").

- Page 15095, line 5: We have rephrased the sentence. It now reads "The relationships between air mass origin and EP flux or ozone have not been examined in detail so far for the autumn season to our knowledge". The main point here is that we validated the predictions of the theory of the Brewer-Dobson circulation with "observed" data (counting the reanalysis as observed data as well). We don't want to convey the impression that we discovered any new mechanisms or had any new insights with regard to the theory.

Technical corrections

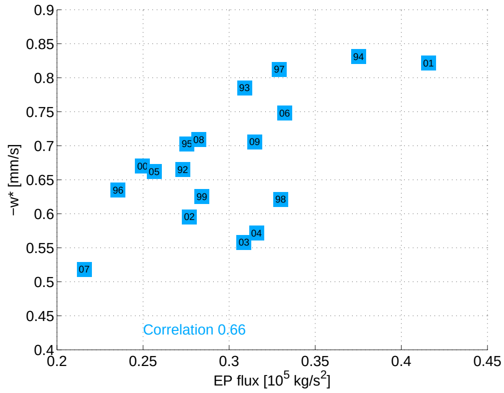
- Abstract, line 2: Done.
- Abstract, line 10: Done.
- Page 15084, line 22: Deleted the reference. However, the discussion in this paper does not only apply to summer but also to autumn, where NOx is still the most important driver of ozone chemistry. This is just not explicitely mentioned in the text of the paper.
- Page 15085, line 1: Done.
- Page 15085, line 9: Done.
- Page 15085, line 28: Done.
- Page 15086, line 23: Done.
- Page 15088, line 22: Done.
- Page 15089, line 1: Done.
- Page 15089, line 4: Done.
- Page 15089, line 9: Done.
- Page 15090, line 10: Either the page number or line number must be wrong. The comment does not seem to refer to this line, which deals with the definition of the vortex.

- Page 15091, line 17: Done.
- Page 15095, line 6: Done.
- Figure 4: Done.
- Figure 6,7: We think that would be more confusing. We would like to leave it as is.
- Figure 9: We don't really understand this comment. The air is not originating from "all" potential temperature levels within the equivalent latitude interval, but only from the potential temperature interval given at the vertical axis. The sentence ends with "... averaged over the potential temperature intervals given at the vertical axis", which seems sufficiently clear to us.

Corrections by us

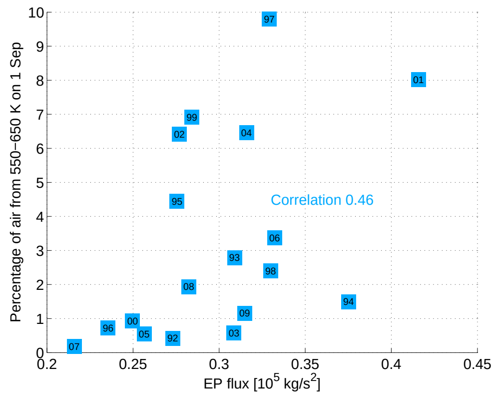
- Figure 11: The axes of the figure were not scaled correctly and some of the data points were outside the visible area. Corrected.
- The citation on page 15085, line 6 should have been Kawa et al. (2005) and not Kawa et al. (2003).

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15083, 2012.



−w* [mm/s]
EP flux [10^5 kg/s^2]
Correlation 0.66

Fig. 1.



Percentage of air from 550−650 K on 1 Sep
EP flux [10^5 kg/s^2]
Correlation 0.46

Fig. 2.

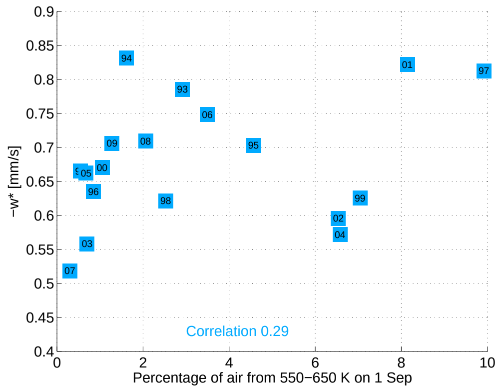

−w* [mm/s]
Percentage of air from 550−650 K on 1 Sep
Correlation 0.29
0.9
0.85
0.8
0.75
0.7
0.65
0.6
0.55
0.5
0.45
0.4
0
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Fig. 3.