

Interactive comment on “Interaction of chemical and transport processes during the formation of the Arctic stratospheric polar vortex” by D. Blessmann et al.

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

General comments

- “First, in the discussion in the paper (e.g. conclusions) the paper seems to address the issue of ‘polar ozone’. However, the paper presents only a case study of one particular Arctic winter. It needs to be discussed in the paper in how far the results presented here are relevant to other Arctic winters, e.g., those with less

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polar ozone loss. Alternatively, would the same conclusions have been reached if the Arctic winter 2010/2011 had been analyzed?”:

It is true that the study only addresses one winter and is a case study. That should have been stated more clearly in the abstract and introduction. The restriction on a single winter is due to computational constraints. We agree that some additional discussion on how the results of the case study can be generalized and carried over to other winters is appropriate and helpful for strengthening the validity of our conclusions. We assume here that the basic conclusions from our paper will not be invalidated by the interannual variability in temperature, NO_x levels and transport, since the latitudinal, vertical and temporal gradients in the lifetime of ozone will mainly be determined by the solar insolation. We have now added some discussion of this to the paper. In addition, we performed some simple sensitivity runs to estimate the effect of interannual variability in temperature and NO_x. These runs show that interannual changes in temperature or NO_x are of second importance for the lifetime of the signal.

Since we are looking at the variability and chemistry in autumn here, there is no point in comparing winters with more or less ozone loss, since these are two completely unrelated issues (not taking into account the purely statistical relationship of Kawa and Sinnhuber, which is not the subject of this study). I hope there was no misunderstanding about the time frame we are looking at. This paper deals with ozone in autumn (i.e. from September to December) and not in winter. There may have been some confusion caused about this by the erroneous use of words like “spring” and “mid-winter” in some places (see last general comment).

It is clearly stated in the title, abstract and introduction that the subject of the study is the Arctic polar vortex. Hence, we think it is acceptable that e.g. we talk of the “polar vortex” instead of the “Arctic polar vortex” occasionally to enhance the readability. It is obvious from the context that the Arctic is meant.

- “Second, it should be stated in how far the results of the paper carry over to the

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southern hemisphere polar vortex.”:

We will not extend the discussion to the southern hemisphere. First, we have not performed any model runs for the southern hemisphere. Second, there is no obvious reason why the southern hemisphere should be discussed in this paper. This topic is obviously beyond the scope of our paper, which discusses Arctic ozone. Third, it is to be expected that the basic results would be similar, since the seasonal cycle of solar insolation is the same and mixing ratios of the important species are similar. A minor point is that the southern hemisphere is less dynamically active and there is reason to believe that the results would be of less interest.

- “Lee et al. (2001) have reported...”:

We don't think it makes sense to cite this paper. The paper by Lee et al. addresses the evolution of the vortex in (Antarctic) winter and spring (July to November, which would correspond to January to May in the Northern Hemisphere). Our paper addresses the evolution of the circulation in autumn in the northern hemisphere (September to December). Hence, there is no connection to the results in this paper, even if we would discuss the Antarctic in our paper.

- “Third, my impression is that throughout the paper citations to important, classic papers are missing. Some examples are given below.”:

We hope that we have cited the relevant papers for this study. Additionally, we have now added several new citations, e.g. two papers of Fahey et al. for the summer chemistry. If there are papers that we missed, we will be happy to cite them. Unfortunately, you don't give the promised examples in the following (except for the Farman paper).

- “Finally, I'd suggest...”:

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We have now defined the used terms more precisely. Particularly, the terms “early winter” and “autumn” are defined in the introduction now. Unfortunately, in a number of places in the manuscript, words like “spring” and “mid-winter” were erroneously used, where “early winter” would have been much more appropriate. We apologize for this and this has been corrected in a number of places (e.g. in the last part of the introduction, see also your comment for page 32286, line 16).

The reason for the confusion was in part due to earlier versions of the manuscript focussing also on later periods in the winter.

Comments in detail

- Page 32286, line 4: It is now stated that this period typically lasts from the middle of September to the middle of November (with some degree of interannual variability).
- Page 32286, line 16: See the last general comment above. This particular comment should be solved by using “early winter” here.
- A general comment to your next comments about the model description: We tried to keep the model description short, since there are two extensive model description papers which can be used as reference. While some of the issues you mention are certainly interesting, a discussion of the performance, validation and initialization of the model is beyond the scope of this paper, has been done elsewhere in detail and is unrelated to the central issue of this study.
- Page 32287, line 6: A good citation is the model description paper cited some lines above, which I have cited again now. Other papers discussing this are several papers from the CLaMS modelling group (e.g. McKenna et al., J. Geophys. Res., 107, D16, 4309, 2002, Konopka et al., Q. J. R. Meteorol. Soc., 131,

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565-579, 2005) or ATTILA modelling group (Reithmeier and Sausen, Tellus, 54B, 278–299, 2002).

- Page 32287, line 10: You are right. We have rewritten the sentence to make clear that this only refers to liquid particles. Since NAT and ice clouds play no role in the time period of the model run, no further details are given for these clouds. A detailed description of the heterogeneous chemistry module can be found in the model description paper.
- Page 32287, line 13: Since the photolysis of the ClO dimer is of minor importance for the model run and the chemistry in autumn, we have deleted the last part of the sentence. The reader can refer to the model description paper here. Added “relevant rate constants” to the sentence to make sure it is still formally correct.
- Page 32287, line 20: The number is chosen based on the observed variability of ozone in early autumn as deduced from ozone sondes. We added a sentence to the manuscript.

Unfortunately, we are not able to do additional runs with the full model with other perturbations due to computational constraints. Since it is the relative change (relative to the initial perturbation) which is of interest here, we assume that the relative change is not too sensitive to the magnitude of the initial perturbation. We have performed some simple box model runs in response to your comment to estimate the sensitivity to the initial conditions. For this we started box model runs on 450, 575 and 750 K with initial conditions for species and temperature taken from the ATLAS run (averaged over equivalent latitudes north of 60 degrees and 10 September to 20 September). The box model runs were run from September to December. The initial perturbation was set to values of 15% and 30%. These runs show that the quantity f is not affected by the absolute magnitude of the perturbation in good approximation. I.e., the loss of signal is proportional to the initial perturbation. We added some discussion of this to the paper.

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- Page 32288, line 9: You are right, the citation is wrong. The correct citation is Grooß et al. (2002), as for the other tracer-tracer relationships.
- Page 32288, line 16: Since the exact bromine mixing ratio is of minor importance for the results of this model run and the chemistry in autumn, we think it makes no sense to discuss this here. The initialization of all species is provided here mainly for completeness and since it is good scientific practice to give all information needed to repeat the model run by someone else.
- Page 32288, line 24: You are right. The mixing time step is now given in the paper. Since this is discussed in detail in the model description paper, which is cited here, we don't think that more discussion is appropriate.
- Page 32288, line 25 and 27: The sentence has been changed to “The vertical motion is driven by diabatic heating rates (clear sky) from ERA Interim.”. Since I cannot figure out what the comment for line 25 is referring to, I assume it is basically the same as the comment for line 27.
- Page 32289, line 20: Added two papers by Fahey et al. as references.
- Page 32289, line 25: Deleted the reference to Toon et al. (1999). Added reference to Farman et al. (1985) and Fahey et al. (1999).
- Page 32291, line 18: The citation does not refer to the isolation of the vortex, but to the increase in lifetime of ozone by the isolation of the vortex. This is discussed in the cited paper and we think the citation is appropriate.
- Page 32292, line 5: We have skipped some of the sentences advertising the model here, since it didn't really seem to be appropriate. A citation showing that the numerical diffusion in Eulerian models is at least an order of magnitude larger than the observed stratospheric diffusion is e.g. P. Konopka et al., Q. J. R. Meteorol. Soc., 131, 565-579, 2005.

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- Page 32292, line 23: That is how the acknowledgment was requested by the ACE team :-). I changed that to something more sensible.

Additional changes by us

- The figures and discussion were restricted to levels below 750 K. Above 750 K, the passive ozone tracer in Figure 1b is not reliable. Since air masses which are above 750 K at the end of the model run were above the upper model boundary at the time of initialization (1 August), the passive ozone tracer cannot be initialized properly there.
- A paragraph discussing the effect of changing the date of the perturbation was added.
- In addition to the changes in grammar, style etc. suggested by you and the other reviewers, some paragraphs, including parts of the introduction and the conclusions and the figure captions were rewritten for more clarity (without changes in content). Some references were added or updated.
- The title and some sentences were changed due to the request of reviewer 2 to avoid confusion about the word "interaction".

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32283, 2011.

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