Atmos. Chem. Phys. Discuss., 15, C7050–C7055, 2015
www.atmos-chem-phys-discuss.net/15/C7050/2015/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sensitivity of polar stratospheric cloud formation to changes in water vapour and temperature" *by* F. Khosrawi et al.

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Received and published: 21 September 2015

We thank reviewer 1 for the constructive, helpful criticism and the suggestion for revision. We followed the suggestions of reviewer 1 and revised the manuscript accordingly. Additionally to this reply, a supplementary document is provided where we show the results of repeating our sensitivity study on single back trajectories for other Arctic winters. Further, in this supplement it is documented (also on single back trajectories) how the sensitivity study would look like if we take into account a temperature bias of ±2 K. Additionally, due to the comments given by both referees Section 5 and 6 of the manuscript have been thoroughly revised.

This study examines the impact of changes in the concentration of stratospheric water vapour (increases up to 1ppmv) and temperature (decreases of 1K) on the time that air parcels might be below various PSC existence thresholds in the Northern hemisphere. This study also examines a range of satellite datasets to identify trends in the temperature and water vapour concentrations over the period 2000-2014 at high equivalent latitudes. While the central premise of the work is interesting, the amount of analysis shown seems to be too cursory for major conclusions to be drawn with certainty. In particular, Section 5 which examines the trends in the water vapour concentration and temperature over the period 2000 to 2014 lacks sufficient depth in my opinion. Thus, I think this work needs major revision before it is accepted for publication. I identify a number of key points below that concern me about the analysis and suggestions for further analysis which might help the authors tune this work for publication.

Sampling issue and interpretation of track statistics: The authors use trajectories derived from data in the 2010/2011 which they argue is sensible to use because there was a significant amount of PSC in this year. They then argue that this means that the statistics derived are effectively more robust because of the larger number of cases possible to derive back trajectories from. However, I would argue that this selection likely means that this study represents a worst-case scenario. Essentially a year with high PSC occurrence is used as the baseline to examine how even cooler temperatures and more water vapour will impact PSC formation. Whether the resultant statistics of an average year would be similar is not clear to me and not tested. The number of trajectories tracked (738) also seems rather small to me given the nature of the question that the authors wish to examine. Thus, I think this work would greatly benefit from analysis of at least some tracks in another year to identify whether the enhancement in the time below the PSC thresholds is comparable in a relative sense. However, to significantly improve this study, I would suggest doing this type of analysis over a number of years to get a representative set of statistics.

Although the Arctic winter 2010/2011 was quite extreme, our study does not represent a worst case scenario. For the temperature history along the trajectories, it does not matter which Arctic winter is considered. As soon as it gets cold and PSCs are formed, the trajectories resemble each other irrespective which year is considered. We have performed studies on PSC formation previously e.g. for the Arctic winter 2004/2005 (Blum et al., 2006), the Arctic winter 2008/2009 (Achtert et al., 2011) and the Arctic winter 2009/2010 (Khosrawi et al., 2011) and did see the same behaviour in the trajectories applied in these studies. We use the trajectories from our previous Arctic studies to demonstrate that the temperature history along the trajectory does not differ that much from year to year. Especially, we demonstrate that although the total time the temperature is below T_{NAT} or T_{ice} is changing for individual trajectories, that the increase in time temperatures are below T_{NAT} or T_{ice} due to an increase in H_2O mixing ratio or a cooling of stratospheric temperature is comparable for all years (see supplement to this reply). We discuss this in section 6 and based on the comment made by referee 2 we changed the sentence as follow and hope that we get the message through now: *As a consequence the total times where the temperature was below T_{NAT} or T_{ice}, respectively, would have been shorter as for the Arctic winter 2010/11. However, the resulting increase in time due to a decrease in temperature and an increase in water vapour can be expected to be similar, thus as dramatic as for the 2010/11 winter.*

Another issue with the analysis is the use of absolute values of time is somewhat meaningless given the arbitrary number of tracks selected. Thus, I would suggest identifying increases in relative terms (percentage increase relative to the base state). This relative analysis would also allow the trajectories from other years to be directly compared – though obviously with less certainty given the likely fewer number of tracks to be calculated.
We agree and while writing the manuscript, we have already considered using the percentage of time increased relative to the base case, however we decided against it due to the following reasons: (1) for the most extreme changes considered in our study (T$-$1 K and H_2O+1 ppmv) we derive enhancements of 800-1000% which are not very handy numbers. (2) It is much easier to discuss in the text "total hours" than the "percentage time in relation to the total hours".

Small-scale processes and errors in the reanalyses: A number of studies have shown that the reanalyses temperature can be rather biased (e.g. Boccara et al., 2008) and this means that the temperature values derived from NCEP can have uncertainties which might be comparable to the temperature variations considered. In addition, while it is mentioned that several studies have identified the impact of small-scale wave temperature perturbations on PSC occurrence this also builds uncertainty into the impact of the prescribed temperature decrease. Without consideration of these factors the uncertainty on the results from the trajectory analysis is unknown, but I would guess from previous studies might be sizable. Thus, some type of uncertainty analysis – perhaps using Monte-Carlo analysis would add real value to the study in my opinion.
Small-scale processes as well as uncertainties in the reanalyses have the same effect on temperatures. Both cause temperatures to be somewhat higher or lower so that the threshold temperatures may in some cases where temperatures are close to the threshold temperatures may just be reached or just not reached. The temperature cooling of 1 K that we consider in our study shows on the other hand what a 1 K warm bias in e.g meteorological analyses would mean. This would cause an enhancement of the total time temperatures would be below T_{NAT} or T_{ice} and thus cause a prolongation of potential PSC existence. However, the increase in time where temperatures are below T_{NAT} or T_{ice} when H_2O mixing ratios are increased is comparable to our base case. Thus, uncertainties of meteorological analyses or small-scale variability have no influence on our conclusion that increase in H_2O or a cooling of the stratosphere will enhance the potential for PSC formation. To demonstrate this more clearly we picked two trajectories and repeated our sensitivity study on single trajectories with assuming a warm bias and a cold bias of 2 K (see supplement to this reply). In section 6 (P17764, l17ff) we added the following sentence at the end of the paragraph: *However, irrespective of if there is a warm or cold bias in the trajectory temperature or the water vapour mixing ratio in the stratosphere, an increase in water vapour mixing ratios or a cooling of temperature will definitely result in a prolongation of the potential for PSC existence as shown*

in our sensitivity study.

Linear trend analysis: This analysis seems like an afterthought and given the difficulty in intercalibrating the various satellite datasets to the level required to observe a small trend makes me wonder whether this portion of the analysis is an unnecessary distraction. I would advise thinking seriously about whether this analysis really adds value. In particular, I would suggest that a rigorous trend analysis using this many satellite datasets is a large paper in its own right.

The reason why we have these two parts in the paper lies in the motivation of our study which is to investigate if there is a connection between increases in stratospheric water vapour and the widespread severe denitrification that was observed during the recent Arctic winters 2009/2010 and 2010/2011. Therefore, we on one hand investigate the sensitivity of PSC existence on water vapour and temperature changes in the lower stratosphere, and on the other hand we investigate if there is a trend in water vapour observed in the lower stratosphere. Therefore, both parts of the paper are important. The motivation of our study is "not" to investigate a "long-term trend" in stratospheric water vapour. Such studies are currently being performed in the frame of SPARC Water Vapour Assessment (WAVAS) and will be published in the near future. In which order the results are presented seems to be a matter of taste. In the course of writing up our results we swapped the order several times but came in the end to the conclusion that the order as we have it now is the best. To remind the reader why we have these two parts in the paper we added the following sentences at the beginning of section 5: *In the previous sections we demonstrated that a water vapour increase and temperature decrease would increase the potential for PSC formation. More than a decade ago it was already suggested that a cooling of stratospheric temperatures by 1 K or an increase of 1 ppmv of stratospheric water vapour could promote denitrification (Santee et al., 1995; Tabazadeh et al., 2000). During the two Arctic winters 2009/10 and 2010/11, the strongest denitrification in the recent decade was observed (Khosrawi et al., 2011; Khosrawi et al., 2012). Here, we investigate the variability of Arctic water vapour and temperature since the*

new millennium to see if there is a connection to the severe denitrification observed in the past years.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C7050/2015/acpd-15-C7050-2015-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 17743, 2015.