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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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We thank reviewer 2 for the constructive, helpful criticism and the suggestion for revision. We followed the suggestions of referee 2 and revised the manuscript accordingly. Especially, section 5 and 6 have been thoroughly revised.

To start with, I would like to state that I have not worked in this field in recent years and only followed the development from a sideline. Therefore, I am not fully updated with the recent literature, and I haven't been able to go through the very comprehensive list of references given in this paper in the frame of such a review. This paper describes investigations of the Arctic stratosphere, with a focus on polar stratospheric cloud (PSC) formation processes

and conditions, both using case studies during the exceptional stratospheric winter of 2010/11 and long-term data sets derived from composed satellite data records.

The paper addresses an issue, which in my opinion is both very interesting and relevant (although interest in stratospheric research has dropped strongly in the recent decade). The two most important parameters influencing the formation of PSCs - stratospheric temperature and water vapour concentration – are hypothesized a lot about, but not settled. In particular, trends of the stratospheric water vapour concentration are highly uncertain, with only one ground-based long-term measurement series from mid-latitudes and a number of satellite-derived data series with considerable uncertainties, as confirmed in this paper.

So the topic is relevant, but I have some critical remarks about the method applied and the conclusions. The most important source of information, the water vapour records from various satellite instruments, should be discussed in much more detail than is done here. The figures 11 and 12 (lower panels) very clearly demonstrate the problems of this issue: the discrepancies between the instruments are almost as large as the inter-annual variability, and then, of course, it becomes very difficult to derive any trends. There seems to be a shorter period, from about 2006 to 2011, where the agreement between the various instruments is good, but before and after this period, the quality is definitely not sufficient to allow trend analyses. I have looked into the very comprehensive paper of Hegglin et al. (2013), but not found any clear statement whether one can combine many satellite instruments to derive a trend in the case of water vapour.

It is possible to combine many satellite instruments and derive a trend for water vapour as shown in the Nature paper by Hegglin et al. published in 2014. Nevertheless, in our study no merging of satellite data has been done! All satellite instruments are considered individually. Irrespective of if the data sets are merged or if the data sets are considered individually it is possible to derive trends in stratospheric water vapour as shown in earlier studies by e.g. Rosenlof et al. 2001. and references therein as well as Scherer et al. (2008). We refer now to these studies at several places in the

manuscript and the text has been revised accordingly as given below in our answer to the comment on P17763, line 19.

Another issue is brought up by the authors themselves: The water vapour concentration anti-correlates to the temperature in the same altitude range, which in turn depends on the dynamical conditions, such as the stability and strength of the polar stratospheric vortex. In the Arctic, these vortex properties are extremely variable (from year to year), so that they introduce a large year-to-year variability in water vapour concentration, even if one uses equivalent latitude to select data. In my opinion, one should investigate water vapour concentrations separately for certain potential vorticity/temperature intervals and look for trends in these sub-sets rather than showing the whole time series without even distinguishing between summer and winter and concluding there is no trend.
So far, trend studies on water vapour were performed using the year-round data sets. Separations were only done concerning the latitude regions. However, we agree that a separation in the polar stratosphere into summer and winter could be worth considering. Attached to this reply is a figure showing the linear trend analyses for winter (DJF). Considering the linear trend analyses for solely the winter months does not change our results. The resulting areas where the changes are positive and significant within the 2 σ uncertainty are quite similar to the changes we found when all seasons are considered.

Concerning instrument-to-instrument comparison, which is shown in Figure 13, why didn't the authors compare the MIPAS and Aura/MLS data for the same period (2004-2012)? That should give a good indication of how well these two satellite instruments agree.
This seems to be a misunderstanding. The purpose and intention of this figure is not to show or state how well MIPAS and MLS agree with each other. Such comparisons have already been performed and published elsewhere. The intention of this figure is to investigate if in the MIPAS or MLS data any linear changes in water vapour are found. We have chosen these two instruments since these are, from the ones

considered in this study, the ones with best spatial and temporal coverage. Further, the trend estimates have been derived for both data sets individually. Therefore, for each trend estimate 12 years could be considered instead of 7 years as in the case when we solely would have used the overlap period. In our study no merging of satellite data has been done. We have slightly changed the text and hope that this becomes more clear now.

The other major methodological critics I have, concerns the two case studies. My impression is that the results have not been exploited properly. Both cases are based on observations of mixed PSCs, combined with back-trajectory calculations. The two cases resemble each other in the fact that during the six days covered by the calculation there are two periods with T sufficiently low to allow the formation of PSCs. However, in both cases these two periods are separated by 60 and 80 hours, respectively, with T up to 10 degrees above the T_{NAT} threshold. Then I wonder how relevant the first period is for the PSC display. If it is not, they should focus their analysis on the second period.
Both periods where the temperature drops below the threshold temperature are of importance. The back trajectories follow the flow within the polar vortex. Thus, the air masses are once or twice transported around in the polar regions. For the trajectories shown in case 1 and 2 the air masses were transported around twice during the past 6 days and the two time periods reflect this. It simply shows that the air mass passed twice through the cold region in the Arctic which is cold enough to allow PSC formation. So far, we only have mentioned this in the manuscript in section 4.2. We added now the following sentence to section 4.1.1 and 4.1.2, respectively: *During the course of the 6 days the trajectories followed the circular flow within the polar vortex and thus the air masses were transported twice around in the polar regions (see figure in supplement).* Further, we added two figures in the supplement showing the trajectories for case 1 and case 2, respectively.

On the other hand, the conclusions drawn from the two case studies are partially trivial:

In one case, simulating a T decrease and/or [H_2O] leads to very little changes regarding the lifetime of a PSC, while in the other case it is noticeably extended. From the figure, this is very simple to derive: it is a consequence of the T variations along the trajectory. In the second case, there is a longer period with T above, but close to T_{NAT}, so decreasing T or increasing T_{NAT} naturally leads to big changes regarding PSC existence duration, while in case 1 T varies much more rapidly so that the simulated T and [H_2O] shifts do not have a large impact on the PSC lifetime.

What I would have liked to see here, was a study of whether the observed existence of the various types of PSCs agrees with the thermal and [H_2O] conditions observed and used in the calculation. If the authors did a calculation at 22 km altitude in case 1, i.e. where there is ice in the observation, why don't they show that calculation in Figure 3, or, even better, temperature history figures from slightly outside and different altitudes inside the PSC, say at 19, 20, 21, 22, 23 and 25 km? That would give substantially more information about conditions for the existence of the different types of PSCs. Can, for example, a comparison between the back-trajectory from 20 km altitude (STS PSC) and the back-trajectory at 22 km (pure ice PSC) allow conclusions about the water vapour concentration in this case? Are there satellite data supporting the [H_2O] value found?

We find a good agreement between the observed existence of the various PSC types with the thermal conditions derived from the trajectories as well with the H_2O observed by the satellite instruments (for our base case applying 5 ppmv). We did not use the trajectory at 22 km from case 1 where also ice was measured, because this is one of the few examples where trajectory temperatures have been too high compared to the PSC types observed by CALIPSO. This can happen when waves are involved in the formation process. To account for the different compositions of the PSCs at different altitudes we calculate three trajectories for each PSC observed by CALIPSO, corresponding to the top, middle and bottom of the cloud. The altitudes have been selected so that the different PSC types within the cloud were considered. In our earlier Arctic winter studies (Achtert et al., 2011 and Blum et al., 2006) we calculated

trajectories at every km of the observed PSC. In these studies, the development of certain PSCs was investigated in more detail (together with box model simulations). However, also in these studies trajectories at three altitudes corresponding to the top, middle and bottom of the cloud would have been sufficient. Therefore, for a statistic as it is performed here, the three trajectories per observed PSC calculated are more than sufficient. To derive water vapour concentrations from the trajectories is not within the scope of this study.

A few concrete comments and questions:

Throughout the paper, the authors should be more thorough in using the right terminology for concentrations/mixing ratios, e.g., write "H_2O concentration" or "[H_2O]", and not just "H_2O"

At several places in the text we clarify now what we mean and write H_2O mixing ratios instead of just H_2O.

P. 17757, line 3: Are the starting coordinates of the first back-trajectory calculation given wrong? (71N, 61E) is not on the trajectory; from Figure 2 (upper panel) I conclude that it should be (71N, 51E). If the authors used the wrong coordinates in their calculation, this might have important consequences for their calculation.

The starting coordinates for the back trajectory given in the text are correct. However, although we intended to start the trajectory at some point along the CALIPSO track, we accidentally started the trajectory shifted by a few degrees to the east. However, this does not change our results since the trajectory was nevertheless started within the PSC and crosses the CALIPSO track just a few hours later (see Figure in supplement).

P. 17757, lines 12ff: What does the sentence "The temperature history along the trajectory is in agreement with the CALIPSO observations" mean? Are there CALIPSO measurements from the time-space points along the backward trajectory which show such agreement?

What we actually meant is, that the PSC formation threshold temperatures reached along the trajectory agree with the PSC type observed by CALIPSO. Note: The start point of the trajectory (at t=0) coincides with the CALIPSO measurement. However, as mentioned above, for this trajectory the coincident time with the CALIPSO measurement is somewhat later, at t=-5 h. STS was observed at the altitude where the trajectory was started (20 km). Temperatures along the trajectory dropped sufficiently low below the NAT formation temperature to allow STS formation. The sentence has been changed as follows: *The temperature range T_2 corresponds to the time period when a PSC was measured by CALIPSO on that day. The temperature drops sufficiently low below T_{NAT} to allow STS formation, which is in agreement with the CALIPSO observation at 20 km (Figure 2).*

P. 17759, lines 6 ff: Instead of speculating about the values of [H_2O], why don't the authors use measured values of this parameter inside the Arctic polar vortex from the satellite data series?
Although not stated explicitly, we do not speculate about the H_2O mixing ratios in the Arctic lower stratosphere. The 5 ppmv we use for our base case is the typical water vapour mixing ratio observed in the Arctic polar lower stratosphere by the satellite observations considered in this study as well as by other observations. The 0.5-1 ppmv increase we consider is based on the trends in stratospheric water vapour reported by Rosenlof et al. (2001) and Hurst et al. (2011). To make clear that these values were not arbitrarily chosen, we changed the sentence as follows: *Using the entire trajectory ensemble the total time (sum over all 738 trajectories) where the temperature was below T_{NAT} and T_{ice}, respectively, was estimated applying an H_2O mixing ratio of 5 ppmv, same as in section 4.1.1 and 4.1.2, typical water vapour mixing ratio for the Arctic polar lower stratosphere (Achtert et al. [2011] and references therein, Khosrawi et al. [2011]) and observed by the satellite instruments considered in this study.). This calculation was repeated applying a H_2O increase of 0.25–1 ppmv (ΔH_2O=0.25 ppmv, as in section 4.1.1 and 4.1.2, according to the estimated trends from Rosenlof et al., 2001 and Hurst et al., 2011) as well as a decrease in*

temperature by 0.5 and 1 K.

P. 17763, line 19: "stratospheric water vapour exhibits a strong decadal variability": How can this be stated in light of the following statement "..with the lack of available long-term observations" in line 20? With only one long-term observation series at a mid-latitude station, the first sentence is nothing more than a hypothesis.
We have removed this particular sentence and thoroughly rewritten section 5. Nevertheless, the Boulder time series is the longest "continuous" observed time series available up to now. However, Hegglin et al. (2014) derived by merging of satellite data sets a similar long time series than the Boulder time series. Additionally, several water vapour trend studies have been performed in the past using different in-situ and remote sensing data sets. Although most of these studies do not consider more than two or three decades, they revealed the decadal variability of water vapour as shown in e.g. Fueglistaler and Haynes, 2005; Randel et al., 2006, Fujiwara et al., 2010. We agree that we missed out to mention these earlier studies in our manuscript. We therefore have added these references in the introduction and added the following text: *Long-term balloon-borne measurements at Boulder/Colorado (40 N/105 W) indicate an increase of lower stratospheric water vapour abundances, on average by 1 ppmv, during the last 30 years (1980-2010) (Scherer et al., 2008; Hurst et al., 2011). Recently Hegglin et al. (2014) analysed a merged satellite time series spanning from the late 1980s to 2010, which did not confirm the findings from the Boulder data set, arguing the representativeness of these data on a larger spatial scale. In the lower stratosphere negative changes were dominating, while positive changes were found only in the upper part of the stratosphere. The decrease in the lower stratosphere was attributed to a strengthened lower stratospheric circulation.*

P. 17764, lines 14ff: Isn't this anti-correlation between stratospheric temperature and [H_2O] a consequence of a stronger subsidence of stratospheric air masses when the lower stratosphere is very cold? As the mixing ratio of water vapour increases with altitude (as nicely shown in Fig. 15), increased subsidence would "pull down" wetter air masses from above.

Yes, it is correct, that the anti-correlation between stratospheric temperature and H_2O is a consequence of a stronger subsidence during cold Arctic winters. For example, Manney et al. (2008) showed that during cold Arctic winters the subsidence in the vortex is strongly enhanced compared to other years and that thus moister air is "pulled down" from above. Further, during years where the QBO is in its westerly phase the vortex is more stable and colder (Holton and Tan, 1908). The following sentences have been added: *During polar winter vigorous descent occurs within the polar vortex, transporting air masses from the upper stratosphere and mesosphere down to the lower stratosphere (Bacmeister et al., 1995). As water vapour typically exhibits a maximum around the stratopause this descent also transports moister air towards the lower stratosphere. Sonkaew et al. (2013) analysed SCIAMACHY data from 2002-2009 and found that the QBO west phase is associated with larger PSC occurrences and stronger chemical ozone destruction than the QBO east phase. Their findings are in agreement with the Holton-Tan mechanism (Holton and Tan, 1980) which relates the QBO west phase to a colder and more stable vortex. During cold Arctic winters, as 2010/2011, the subsidence within the polar vortex is strongly enhanced as shown e.g. by Manney et al. (2008), causing positive water vapour anomalies.*

P. 17765, lines 9 ff: I do not agree with this conclusion. In case of a relatively warm polar vortex with, say, extended areas of temperatures just above T_{NAT}, a decrease of T_{air} by 1 degree might have as dramatic consequences as described here.
We agree that in a relatively warm polar vortex a decrease by 1 K would have as dramatic consequences as during a cold winter as the 2010/2011 winter as was investigated in our study. This is exactly what we wanted to say with our statement. We changed the text as follows 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.*

P. 17765, last paragraph: The main question that remains unanswered still is whether the H_2O concentration changes in the polar stratosphere. In my opinion, such cold winters with lots of PSCs could be used to shed additional light on that question.
The focus of our study is on the past 15 years, thus we can only give some answers on this time period. In our study we consider the correlation between observed water vapour variability and the recent temperature evolution in the Arctic together with PSC observations to investigate a possible connection between an increase in stratospheric water vapour and the occurrence of cold winters that lead to extreme PSC formation and denitrification. As mentioned before, there is a strong decadial variability found in the observed water vapour time series, and we also found some signifcant positive trends so far. How water vapour and Arctic winter dynamics will change in the future can only be ruled out with taking into account climate model simulations which however is beyond the scope of this study.

P. 17766, lines 20 ff: Here the authors suddenly open a completely new issue that is not discussed before - the sudden drop of lower stratospheric water vapour concentration in 2000-2001 in the tropical tropopause region and its delayed manifestation at higher latitudes. Without a more detailed discussion of it, they should remove it from the conclusions. Besides that, this drop at high latitudes is only seen clearly in one of the satellite records (ODIN), but not in HALOE, MIPAS and SCIAMACHY.
The respective sentences in the conclusion have been removed. Additionally, section 5 and section 6 thoroughly revised. In the introduction the following text has been added: *A decisive role here played a pronounced drop in water vapour in 2000 (also known as the millennium drop)(Randel et al., 2006; Scherer et al., 2008; Solomon et al., 2010; Urban et al., 2012), that first started to recover in 2004 to 2005. This drop was caused by a reduced transport of water vapour from the troposphere into the stratosphere in response to a colder tropical tropopause. The temperature decrease has been due to variations of the QBO (Quasi-biennial Oscillation), ENSO (El Niño Southern Oscillation) and the Brewer-Dobson circulation that collectively acted in the same direction lowering the tropopause temperatures. In 2011 such a*

drop happened again, however more short-lived (Urban et al., 2014). In section 5 we write now: *The signatures of the water vapour drops in 2000 and 2011 are not easy distinguishable in the Arctic. In the altitude range between 475 K to 525 K the decrease throughout 2003 may be attributed to the millennium drop. Arctic observations of POAM III indicated the drop already in early 2001 (Randel et al., 2004). This seems to be consistent with studies by Brinkop et al. (2015) that showed a delay of up to 12 months between the drop occurrence in the tropics and at 50° latitude at these low altitudes. The UARS/HALOE observations employed here do not show a clear sign of a decrease in 2001, however admittedly the measurement coverage of this instrument has not been optimal for these high latitudes. The decrease in the Arctic in 2011 may correspond to the drop observed in the tropics. Yet, the length of the decrease is shorter than observed at the low latitudes. Higher up, between 525 K and 825 K potential temperature, a longer delay to the drop occurrence in the tropics can be expected (Stiller et al., 2012; Brinkop et al., 2015). Thus, the decrease observed here in 2002 and 2003 is more likely attributed to the millennium drop. The decrease in 2011 on the other hand is unlikely to be connected to the tropical event.*

Figure 7: It would be better if all panels had the same y-axis scales; this would show the differences much better.
We have adjusted the y-axis scale, so that it is the same for all panels.

Figure 13: Why don't the authors show the trend altitude profiles of both instruments for the same period of time, i.e., 2004-2012? This would give a direct estimate of instrument-to-instrument agreement or discrepancy and to what degree trends from a composed sets of satellite data can be trusted. Adding two more years on either side of the overlap period covered by only one instrument only reduces the strength of the comparison. How does the result of this figure relate to the results of Hegglin et al. (2014) who see no positive trend in the lower stratosphere?
This seems to be a misunderstanding. The purpose and intention of this figure is not to show or state how well MIPAS and MLS agree with each other. Such comparisons

have already been performed and published elsewhere. The intention of this figure is to investigate linear changes (thus trends) in both satellite instruments, MIPAS and MLS. Further, the trend estimates have been derived for both data sets individually. In our study no merging of satellite data has been done. We have slightly changed the text and hope that this becomes more clear now. A comparison to the Hegglin et al. (2014) results is not possible since in this study a different time period is considered. Further, the study by Hegglin et al. (2014) focuses on the mid-latitude and tropics while our study focuses on the polar regions.

Minor corrections:
P 17746, lines 5, 7: PSC existence temperatures (NAT, ice) are altitude dependent; the altitude of the given typical temperature value should be added
The typical value for the existence temperature of NAT and ice are given for 20 km. We altitude is now given in the text.

p. 17747, line 24: "while", instead of "although"
We prefer to start the sentence with "although" rather than "while".

p. 17748, line 14/15: In the latter winter, denitrification also led to severe ozone depletion with a magnitude comparable to the Antarctic ozone hole
The sentence has been corrected.

p. 17754, line 20:....according to.....
This has been corrected.

p.17756, line 9-10: ..... were calculated based on the CALIPSO observations.....
This has been corrected.

P. 17758, line 11: In this case, the temperatures drop.....

The sentence has been corrected.

P. 17758, line17:.....temperatures reach below T_{NAT} for 15 / 30 h with an increase in.....
This has been corrected.

p. 17760, line 13: This must be Fig. 9, not Fig. 7.
Yes, that's correct, we meant Fig. 9 here and not Fig 7. Thanks for pointing this out.

P. 17762, line 28 – p. 17763, line 3:..... period 2002-2012. E.g., the transport.....(e.g., 6 ppmv) reaches much further down.....and 2010/11 than in the other years.
The sentence has been corrected.

p. 17764, line 16: enhanced
In this context "enhance" should be correct.

p. 17764, line 22: (McDonald et al., 2009; Alexander et al., 2011 & 2013). Temperature perturbations that.....
This has been corrected.

P.17765, line 8:.....to mid-January, and PSCs were.....
This has been corrected.

Figure caption of Figure 11, 3rd line: remove completely "Shown is is" This has been corrected.

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

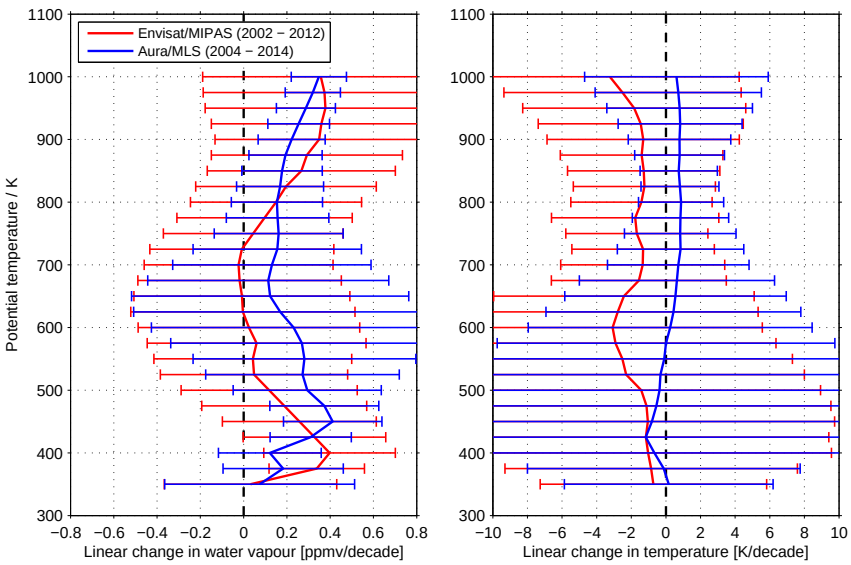

Fig. 1. Linear change in water vapour (left) and temperature (right) vs. potential temperature derived from Envisat/MIPAS (2002–2012) and Aura/MLS (2004–2014) for winter (DJF).