

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

**Anonymous Referee #2**

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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

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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.

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. Concerning instrument-to-instrument comparison, which is shown in Figure

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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.

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 TNAT 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. 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<sub>2</sub>O] 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 TNAT, so decreasing T or increasing TNAT 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<sub>2</sub>O] 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<sub>2</sub>O] 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

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vapour concentration in this case? Are there satellite data supporting the [H<sub>2</sub>O] value found?

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<sub>2</sub>O concentration" or "[H<sub>2</sub>O]", and not just "H<sub>2</sub>O"

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

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?

P. 17759, lines 6 ff: Instead of speculating about the values of [H<sub>2</sub>O], why don't the authors use measured values of this parameter inside the Arctic polar vortex from the satellite data series?

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.

P. 17764, lines 14ff: Isn't this anti-correlation between stratospheric temperature and [H<sub>2</sub>O] 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.

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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 TNAT, a decrease of  $T_{air}$  by 1 degree might have as dramatic consequences as described here.

P. 17765, last paragraph: The main question that remains unanswered still is whether the H<sub>2</sub>O 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.

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.

Figure 7: It would be better if all panels had the same y axis scales; this would show the differences much better.

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?

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

p. 17747, line 24: "while", instead of "although"

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

p. 17754, line 20: ... according to ...

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

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

P. 17758, line17: ... temperatures reach below TNAT for 15 / 30 h with an increase in ...

p. 17760, line 13: This must be Fig. 9, not Fig. 7.

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.

p. 17764, line 16: enhanced

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

P.17765, line 8: ... to mid-January, and PSCs were ...

Figure caption of Figure 11, 3rd line: remove completely "Shown is is"

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