Referee 2

L. Thölix and co-authors discuss in their study “Variability of water vapour in the Arctic stratosphere” sources, variability, and distribution of stratospheric water vapor in the Arctic (70N-90N) and above Sodankylä together with the formation of polar stratospheric clouds (PSCs). The chemistry climate model FinROSE has been used and results have been compared against balloon-borne and satellite-borne observations for the period from 1994 until 2013. The authors focus in particular on the Arctic winter 2009/2010. The manuscript is generally well structured and easy to read. However, I have some major comments, which I am going to specify below.

First of all, I have difficulties to extract the main conclusions of this study. Unfortunately,I cannot spot any novel concepts, ideas, or tools. Please rewrite the Introduction and Conclusions of the manuscript in a way that the goal of this study and any highlights related to your methods become evident. If the main goal of your study is to prove already discussed trends in stratospheric water vapor, I would like to see a more profound trend analysis. Highlighting the model’s availability of PSC formation, the reader needs much more details about how PSCs are treated within the model and how nitric acid concentrations compare to observations. This leads me to my second major concern, which refers to the analysis. Some methodical details are missing, which could be helpful to interpret the results and to judge the quality of the simulations. Sometimes, results are presented in a very coarse way, which might cover differences between simulations and observations. The corresponding description of the analysis is often weak, too. Examples are “measured temperatures” (Measured by which instrument?), “near Sodankylä” (What does “near” mean? 1 km? 100 km? 5 degree?) I will pick up some of these weaknesses again as specific comments in the following part of this review. In summary, I would recommend to publish this study only after major revisions have considerably improved the manuscript.

We thank the reviewer for his/her thorough review. We have substantially revised the manuscript: in particular we provide a more detailed trend analysis, add more details about PSCs in FinROSE model, provided more detailed comparison between observations and simulations, and tightened the language in every occasion spotted. Our point-to-point replies to reviewer's comments are provided below.

P22015/L13ff
Kirner et al. (2015) found that “in high southern latitudes, heterogeneous chemistry on ice particles causes only up to 5 DU of additional ozone depletion in the column”, which would not call “remarkable”. Since your study focuses on the Arctic, it would also be better to cite a study related to the Arctic such as Wohltmann et al. (2013).
ICE PSCs contribute only to a minor part of the chlorine activation. According to a model study by Kirner et al. (2015) 90 % of the ozone depletion in the Antarctic spring is caused by halogen activation on liquid particles. In a model study of the Arctic winter 2009/2010 Wohltmann et al. (2013) showed that chlorine activation on liquid aerosols explained the changes in the ozone column to within 10 %. The additional chlorine activation caused by ICE PSCs is modest. However, when ICE PSC particles sediment to lower altitudes, a reduction of water vapour, i.e. dehydration occurs (Kelly et al., 1989).

P22015/L18ff

Your statement implies that denitrification always occurs due to the sedimentation of ice particles, which is not the case. Please reformulate this paragraph and cite an appropriate paper for denitrification.

In addition, the reaction with atomic chlorine has some significance as a sink for methane, but also as a termination reaction of ozone depleting cycles especially in the Antarctic vortex where denitrification reduces the importance of the reaction between ClO and NO2 (Fahey et al., 1990).

P22015/L26

Tian et al.(2009) predicted that "increasing the stratospheric H2O is likely to accelerate the recovery in the northern high latitudes".

The sentence have rewritten and is now:

'Tian et al.(2009) used a climate model to study the over all effect, the results indicated that the total column of ozone in Arctic would increase in the future despite increased active chlorine in the polar spring regions, while in the Antarctic the ozone recovery would be delayed. The distribution of stratospheric water vapour and its effect on the formation of PSCs are therefore of interest.'

Section 2.1 FinROSE

I am missing a detailed description of how PSCs are simulated within FinROSE. Since PSCs are a main focus of your study, the reader needs to know details about their formation, growth, sedimentation behavior etc. to judge the results presented. Number densities and particle sizes are important to explain dehydration. In my opinion, it is not sufficient to just refer to Damski et al. (2007).

The description of PSCs in FinROSE have inserted.

'*The composition of LBA and STS are calculated using the method by Carslaw et al. (1995). The STS are not considered below the ICE psc formation temperature. The number density profile for LBA and STS is estimated from McLinden et al (1999) and the sulphuric acid distribution [µm2 cm-3] is based on 2-D model data (Bekki and Pyle 1992). NAT formation is based on the thermodynamic equilibrium equations by Hanson and Mauersberger (1988). The model includes an option to include a supersaturation requirement for NAT and ICE formation, but this option was not used in the simulations reported in this paper. The choice was made due to the relative modest resolution of the model. Co-extistence of NAT and STS is allowed. A scheme for growth of NAT particles is included based on Fahey et al. (2001). The number density of NAT particles is initially assumed to be 1 cm-3 (Krämer at al 2003), the number density is reduced for large NAT particles. The temperature threshold for ICE particle formation is based on expressions by Marti and Mauersberger (1993). The equilibrium pressure of nitric acid above ICE is calculated according to Hanson and Mauersberger (1988). The ICE number density is assumed to be 0.04 cm-3, estimated from synoptic scale PSCs (Dye et al. 1992).*'

P22020/L27 and P22021/L1

The term "water ice" includes also wave ice. You probably wanted to distinguish wave ice and synoptic-scale ice.

Thanks for focusing. The sentence have now changed to:

'*The CALIPSO PSC algorithm classifies PSCs by composition. Six different classes are defined: supercooled ternary solution (STS), two classes of liquid/NAT mixtures and mix 2 enhanced and water ice (including synoptic-scale ice and wave ice) (Pitts et al., 2011)*'

P22021/L17

There are also significant differences between FinROSE and MLS from January until April in the same altitude range as mentioned for the summer months. FinROSE is too moist compared to MLS, which favors of course PSC formation.

The MLS data is changed to be level3 overpass data and all of the climatologies have been recalculated using years 2004 to 2014. The text is modified for matching the figure.

'The largest differences are between 10 and 1 hPa, in winter/spring the concentration in FinROSE is about 1 ppm higher compared to MLS but in summer MLS is about 0.3 hPa moister than FinROSE. The too moist air in the spring in FinROSE can lead for effective PSC formation.'

P22023/L5

The model is about 0.7 ppm drier at 20 hPa (not at 30 hPa).

Thanks for mention the error. The MLS data have been changed to level3 MLS overpass data. Now the biggest difference is at 3 hPa. It has been corrected to the text.

P22023/L8

ERA-Interim is also at 30 hPa drier than MLS and the soundings!?

Thank you – corrected.

P22023/L18ff

It would be possible to compare sounding to model data only at those times and locations at which balloon soundings are available instead of calculating a multi year average.

The differences have been calculated first and after that the averages. For FinROSE and ECMWF all the MLS profiles could have been used, but for sounding the number of soundings were the restrictive thing. Text have been modified:

'The differences have been calculated using all the available datapairs during Januaries and Februaries between 2004 and 2014 and after that the differences have averaged. For FinROSE and ECMWF all the MLS profiles could have been used, but for sounding the number of soundings were the restricted thing.'

P22024/L24ff and Figure 4

Please explain how you define "anomaly".

We defined the anomaly as a departure from the mean value for the period 1994-2013. This has been stated in the original version of the manuscript. Now the MLS anomaly have been inserted to the Figure and MLS anomaly have calculated using years 2004-2014 and all the other anomalies have recalculated using years 1990-2014. To make it clearer we rewrite the statement in the revised version as follows:

'*All the anomalies are calculated with respect to the mean values for the years period 1990-2014 for FinROSE and ERA-Interim and for the period 2004-2014 for MLS anomalies.'*

P22025/L17

If at all, the anomalies seen in FinROSE agree with Dessler et al. (2013) but not with Solomon et al. (2010), who see decreasing water vapor concentrations after the year 2000. The wording "also" is therefore misleading. However, Dessler et al. (2013) focuses on latitudes 30N-30S whereas you look at 70N-90N.

Following this and the related comments of R1 and Wang we have elaborated the discussion of the water vapour variability. We performed regression analysis following Dessler et al. (2014). We used three proxies: qbo index (QBO, equatorial winds at 50 hPa), Brewer Dobson circulation index (BD, residual vertical winds at 70 hPa averaged from 30S to 30N),

and cold point temperature (CPT). Unlike Dessler et al. (2014) we found that the use of tropical temperatures at 500 hPa was not enough to explain the variability of the cold point temperature, and therefore used it as one of proxies. Although, there is some correlation between CPT and QBO (0.36) QBO also affect the transport of the water vapour not directly influenced by CPT; therefore the use of both proxies is justified. We apply multiple regression analysis with all three proxies to water vapour time series averaged north of 70N and at 82 hPa and 56 hPa. Cross-correlation analysis shows bread peaks at lags 6-12 months for the proxies. The maximum of the correlations of QBO and CPT with water vapour at 56 hPa is at about 10 months lag, and with 82 hPa is at 8-9 month lags, suggesting that propagation of the tropical anomalies in the lower stratosphere is faster than that in the middle stratosphere, likely due to more efficient mixing. We use 10 month lag for all proxies for regression at 56 hPa and 9 month lag for the regression at 82 hPa.

The individual correlation coefficients with our proxies are shown in the Table below. The main contribution to the polar water vapour variability is CTP, followed by QBO. We found very weak contribution of BD proxy to the variability of the water vapour. One reason is that the effect of BD contribution is accumulated over time and this is not well represented by the monthly proxy. The multiple regression coefficients are 0.57 and 0.51 at 82 hPa and at 56hPa correspondingly, showing that our models only explain 25-30% of the variability. This is considerably less than that of Dessler et al. suggesting that different processes contribute to the polar water vapour variability in comparison to those in the tropics. Note that the regression somewhat explains the increase of the water vapour from 2005 to 2010, which is more clear at 82hPa. However the peak of the water vapor during 2011-2013 is not explained by these proxies.

Table: Correlation coefficients

	CPT	QBO	BD	Multiple
56 hPa	0.454073	0.315499	-0.209597	0.51
82 hPa	0.518588	0.399678	-0.180337	0.57

Figure 4
I am not able to detect the blue dots in Panel a, which are supposed to show the sounding data according to the figure caption. Do you see a trend in the MLS data? Why don't you show MLS data in Panel b - e of Figure 4?
Sounding observations were mistakenly missing from the figure 4. Now they are added and marked with red dots.

MLS anomalies were not in the panels b-e, because the climatology have calculated from years 1994-2013. We have added the MLS anomalies in the revised manuscript.

There is a positive trend in MLS data also, but it is weaker than in FinROSE. Text about it is added. Also the Figure 4 is now split into two figures: new Figure 4 shows timeseries interpolated to Sodankylä, which allows comparison to sounding, and new Figure 5 shows anomalies from 70-90N.

P22026/L21, P22027/L2
I don't understand the meaning of "an average frequency of 0.4 per winter". The description that 4 out of 10 winters offer conditions, which allow the formation of ice PSCs, is clear to

me.
Thanks! Following this recommendation we have changed the sentence 'Which gives an average frequency of 0.4 per winter' to '*That means that 4 out of 10 winters offer conditions, which allow the formation of ice PSCs*.'

P22027/L1ff
From 1990 - 1996, 4 out of 7 winters show a significant coverage of ice PSCs, too. Unfortunately, your water vapor time series start only in 1994. You mention also cold temperatures as possible reason, but you do not show temperature trends in your publication neither you cite any study, which shows that stratospheric temperatures show a negative trend in recent years.
Thank you for pointing to the cold period during 1990s. We will mention it in the text.
Now the water vapour timeseries in Figure 4 have started from year 1990 (and also year 2014 have been inserted). Additionally we have inserted minimum temperatures north of 50N to the Figure 5.

Figure 6
This figure is from my point of view meaningless. First questions, which arise: What temperature and water values did you take to calculate the CALIPSO crosses? Do you show total or gas phase water values? It is well known that ice formation is related to the frost point temperature. Taking a threshold temperature of 190 K means nothing, instead the frost point at 56 hPa varies from 188.6 K (4.6 ppm H2O) to 189.8 K (5.6 ppm H2O). Showing a vortex mean value of water vapor in the Arctic is also quite useless. In case dehydration occurs, this would be a localized event which evens out by calculating the mean. In summary, I cannot spot any relationship between temperature, water vapor and the area covered by ice PSCs in your figure, almost all colors are spread over the entire space.
Following this comment, and a related comment by R1, Figure 6 has been updated and more discussion has been added. Instead of using area with T<190K we use area with T<188K which is tighter related to ice PSC. We get correlations between temperature and ice PSC of 0.93 for the model and 0.72 for CALIPSO, consistent with expectations. To better illustrate the dependence of ice PSC on water vapour we add panel (b) and also support the dependence by providing mean values of ice PSC area conditioned on water vapour and temperature. In statistical sense the dependence clearly emerges, so that increase of water vapour leads to larger PSC. The correlations between water vapour and ice PSC is 0.21 in FinROSE and 0.41 in CALIPSO. Although we agree that the link between local water vapour and PSC should be clearer, the use of mean vortex water vapour is justified by the obtained correlation. It is important to demonstrate this relation in the context of the paper. In the revised version we use ERA-Interim temperatures in connections with ice PSC from both FinROSE and CALIPSO. Water vapour is from FinROSE, and we show total water content (gas+solid+liquid).

P22028/L17ff
For the Arcitc winter 2009/2010 and with Figure 7, you start a comparison not only of ice but also NAT PSCs. However, you never talk about HNO3 concentrations within FinROSE. Explaining differences between simulations and observations just by the model resolution is therefore not enough. HNO3 concentrations could be compared to MLS. Moreover, it would be nice to have some more details again about the "simplicity of the PSC parameterization". Why do you expect differences here? What are the consequences of fixed NAT number densities, supersaturations etc.?
More details of the PSC parameterization have been added to the model description part. In

order to focus the manuscript on water vapour and ice PSC we have taken NAT PSCs off the Fig. 5 and 7.

Figure 7
You show areas of ice and NAT PSCs above Sodankylä? I assume that the values refer to total areas observed in the vortex, right? At least they are about the same magnitude than the areas shown in Figure 5. Why do you compare those to temperatures above Sodankylä?
You are right, total areas of ICE PSCs and NAT PSCs in the Northern Hemisphere were shown in Figure 7. All the NH CALIPSO observations during winter 2009/2010 were taken into account in the calculation of the areas. In the revised manuscript, instead of Sodankylä temperatures we show total areas with temperatures colder than 188 K. NAT PSCs have been taken off. Now the panels are compared. In particular one can see that area with temperatures colder than 188K is larger than PSC area as one could expect based on frost point calculations.

P22028/L25
What do you mean by CALIPSO temperatures? CALIPSO does not measure temperature.
In the original manuscript temperatures supplied together with CALIPSO PCS-mask-data files were used. In the revised manuscript only ERA-Interim temperatures are used.

Figure 8
It is nearly impossible to see any detailed structures in this figure. It would be for example useful to show temperatures below the frost point in the second and third row instead of the frost point temperature itself, which is in addition plotted with a different colorbar than the temperatures themselves. It would also be nice to see plots of water vapor itself. Since you often explain features by dehydration, it would be nice to see that FinROSE can simulate the observed reduction in water vapor, which is visible in the MLS data (Khaykin et al., 2013). The ice comparison between FinROSE and CALIPSO is also difficult. Looking at Pitts et al. (2011), almost no ice PSCs have been observed after 21 January 2010. Only single measurement points were classified (misclassified?) as ice. From your plot I get the impression that significant areas of the vortex are still covered by ice.
You are right. The panels in the figure were too small and it was difficult to see details. We choose only sounding dates to the picture and now the panels are bigger. Also the frost point temperature map from FinROSE were changed to water vapour mixing ratio maps and MLS frost point temperature have been taken off, because the MLS data is coarse without time averaging. The level of the maps have been changed to 35 hPa where the dehydration features are seen from FinROSE data. ICE PSC maps are also now from the level 35 hPa. CALIPSO PSC areas maybe form too large resulting the gridding of CALIPSO data. If there were any ICE in the gridbox the area of that box have been calculated into the area. We have had to show also small ICE areas.

P22029/L28ff
There is an important difference between the 17 and 23 January 2010. On 17 January, ice PSCs have been observed by balloon-borne measurements above Sodankylä. On 23 January, the dehydrated air masses prevent the formation of ice PSCs. Only STS clouds have been observed even though temperatures were as cold as the week before. Therefore, frost point temperatures on these two days were different (Khaykin et al., 2013).
After recalculating the frost points we have corrected in the Figure. The coldest dates are 17th and 22nd January. It is corrected to the text. If FinRose indeed see the same frost point then we say there is difference with MLS/Khaykin.

P22031/L13ff
One of your main conclusions is that a positive trend in stratospheric water vapor and decreasing stratospheric temperatures have led to an increase in Arctic PSC coverage during the last decade. In this case, you cannot totally ignore literature by Markus Rex (e.g. Rex et al. 2006), the recent WMO report (2014) and also Rieder and Polvani (2013) with a controversial trend discussion.
Thank you for pointing us towards this important issue. In the revised manuscript we refer to this discussion. Note however that from our simulations and analysis of sounding/MLS observations, a long-term trend in stratospheric water vapour cannot be deduced. The positive trend that we discuss concerns the period between about 2006 and 2012, which was followed by a reduction of the concentrations.

P22031/L15f
"The area of [temperatures] colder than 190 K is much larger than the area of simulated ICE PSCs in FinROSE or the area of detected ICE with CALIPSO."! As you mentioned several times, water vapor concentrations are also important and ice formation depends on the frost point temperature. This is nothing new!
We have rewritten this statement. First in the revised manuscript, we use the 188 K temperature as a threshold for the cold air mass, which shows much closer agreement with ice PSC extent. Second we specify that a disagreement between areas with t<188 K and ICE PSC should be expected because of considerable influence of water vapour mixing ratio on PSC formation.

P22031/L19ff
De- and rehydration was indeed observed above Sodankylä in January 2010 and published by Khaykin et al.(2013). However, this cannot be part of your Conclusions (and Abstract) because you neither show balloon profiles of H2O nor FinROSE simulations of de- and rehydrated areas.
We have specified that our conclusions concern simulation of these events by FinROSE. In the revised text we write: '*The winter 2009/2010 was extremely cold in the Arctic stratosphere. Simulations by FinROSE reproduce ICE PSCs and associated dehydration and rehydration at lower altitudes in good agreement with observations reported by Khaykin et al. (2013).*'

Technical corrections

I would recommend to carefully check the English grammar again. Without being a native speaker, I realized mistakes (e.g. P22030/L7 and L8: was instead of were and vice versa; missing verb on P22031/L12; ...).
Thanks for these corrections. We have corrected these and other typos.

P22014/L14
The abbreviation for polar stratospheric clouds (PSC) has already been used before (Line 11 and Line 13). In addition, please ensure that every abbreviation has been explained before the abbreviation is used solely.
Abreviations are checked and corrected.

P22015/L15
remarcable → remarkable

Corrected

P22024/L12
Only the years 1994 - 2013 are shown in Figure 4. e.g.
Figure 4 (and new figure 5) have redrawn for years 1990-2014 and text about it have been modified. The water vapour tracer has recalculated using better upper boundary conditions, and the upper levels in this figure are now better. (The upper boundary condition of the tracer is now always 65% of water vapour. The value is the average of the upper level in the long simulation.)

P22027/L10
NATs -> NAT particles.
Corrected

e.g. P22028/L8
“and and”
Corrected

Figures 2 and 4
Please keep the colors for clarity (e.g. MLS =blue vs. MLS =orange vs. methane oxidation = blue).
Corrected

Figure 4
Please add the unit of Panel b - e to the y-axis.
Unit is added

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