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Interactive comment on “Variability of water vapour in the Arctic stratosphere” by L. Thölix et al.

Anonymous Referee #2

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General comments

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 (70°N–90°N) 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.

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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.

Specific comments

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 I 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).

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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.

P22015/L26

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

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).

P22020/L27 and P22021/L1

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

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.

P22023/L5

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

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P22023/L8

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

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.

P22024/L24ff and Figure 4

Please explain how you define “anomaly”.

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 30°N-30°S whereas you look at 70°N-90°N.

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?

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.

P22027/L1ff

From 1990 - 1996, 4 out of 7 winters show a significant coverage of ice PSCs, too.

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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.

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.

P22028/L17ff

For the Arctic 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.?

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

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than the areas shown in Figure 5. Why do you compare those to temperatures above Sodankylä?

P22028/L25

What do you mean by CALIPSO temperatures? CALIPSO does not measure temperature.

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.

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).

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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.

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!

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.

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; ...).

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.

P22015/L15

remarcable → remarkable

P22024/L12

Only the years 1994 - 2013 are shown in Figure 4.

e.g. P22027/L10

NATs → NAT particles.

e.g. P22028/L8

“and and”

Figures 2 and 4

Please keep the colors for clarity (e.g. MLS=blue vs. MLS=orange vs. methane oxidation=blue).

Figure 4

Please add the unit of Panel b - e to the y-axis.

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