

Interactive comment on “Arctic stratospheric dehydration – Part 2: Microphysical modeling” by I. Engel et al.

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Reply to Anonymous Referee #1

We would like to thank the anonymous reviewer for reading this manuscript and offering suggestions for improvements. In the following, we respond to his/her comments.

General remarks, major concern + P27179/L26 *The paper addresses an important topic, and is generally well written. Most of my comments are only technical comments. I do have, however, one major concern that I wish the authors would seriously consider for the revised version. The model calculation is done with a*

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column-model, and caveats are mentioned. I understand that such a detailed and careful analyses requires some reduction in dimensions to allow the high resolution required for sedimentation, and the microphysical processes while still being computationally feasible. However, I am somewhat concerned that the authors are overly optimistic what scientific conclusions can be derived from comparison of the model with the two observed profiles. I assume that the model is fully mass conserving, and that the dehydration/rehydration seen in the model runs do not change the total water in the column (because the figures show mixing ratios as function of potential temperature, this is difficult to verify for the reader). If we compare the model's de/rehydration signature in mixing ratio shown in Fig. 6 with that of the observations, we can see by eye that the observation is not "mass conserving". (The dehydration "bites" are too large compared to the re-hydration.) Of course there can be many reasons for this (ranging from shear to erroneous assumptions in the initial H₂O profile for the model runs), but the point is that obviously something is missing in the model runs, because all model runs, irrespectively which process is assumed for nucleation etc., would be mass conserving. This then begs the question how much a comparison between observation and different model runs really allows to call one model run better than the other. To be specific: I think that if you would find a model run that perfectly reproduces the dehydration between 480 K - 540 K in Fig. 6 (lower panels), they would be completely off below 480 K (they would produce too much re-hydration), i.e. it is impossible for the model to completely reproduce the observations. It appears to me that this limits the conclusions that can be drawn quite substantially, and I would like to hear the authors' thoughts about this.

We appreciate the reviewer's general remarks and agree that the model's limitations should be emphasized more clearly. Therefore, we moved the individual sentences and paragraphs related to caveats into a new subsection called "Model limitations". We further comment on the robustness of the model results against the background of the observed re- and dehydration layers, which correspond

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to different H₂O masses. The model is indeed fully mass-conserving. Figure 1 shows number densities of water molecules as a function of altitude to demonstrate that the total water stays constant within the vertical column of the model. In contrast, the observations are clearly not mass-conserving in comparison to the climatological mean water vapor profile from Sodankylä. Inspection of Fig. 2 of Khaykin et al. (2013) reveals the day-to-day variability between the individual water vapor profiles. Whereas the agreement between the measurement and the mean values are almost perfect in undisturbed air masses below 450 K on 22 January 2010, there is variability on 17 and 23 January 2010. A different initial profile on these days could be consistent with mass-balanced profiles in the observations. Nevertheless, such profiles remain speculative and we do not intend to change the initial profile for model improvement, while the observed profiles could be affected by wind shear. However, this issue does not affect the conclusions derived from the model results (see below).

A new paragraph added on P27179/L24 is now as follows:

“Whereas ZOMM underestimates the vertical extent of the dehydrated air, it overestimates the dimension of the rehydrated signature. This is related to uncertainties of the H₂O profile used to initialize the model and/or to wind shear (horizontal shear is unimportant due to the constraint by the rotating air in the vortex, but vertical shear is significant). The column model is rigorously mass-conserving. However, independent of temperatures and the nucleation mechanism, ZOMM cannot simultaneously reproduce both, the de- and rehydration signatures observed by CFH relative to the assumed initial H₂O profile. Inspection of Fig. 2 of Khaykin et al. (2013) reveals the day-to-day variability between the individual water vapor profiles. Whereas the agreement between the measurement and the mean values are almost perfect in undisturbed air masses below 450 K on 22 January 2010, there is variability on 17 and 23 January 2010. A different initial profile on these days could be consistent with mass-balanced profiles in the

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observations. Nevertheless, such profiles remain speculative and we refrained from changing the initial profile for model improvement, while the observed profiles could be affected by wind shear. A three-dimensional treatment of the wind fields, which also includes wind shear and mixing of air masses, would be required (but with such a model additional uncertainties would be introduced and the detailed microphysics could hardly be tested). These issues hardly impact the results for S1 during the short (2-day) run-up of the model, whereas they clearly affect S2. Nevertheless, the different scenarios discussed in the subsequent paragraph and presented in Fig. 6 and 7 are robust and allow to draw clear conclusions.”

P27167/L15-17 *Perhaps mention that caveats will be discussed later (i.e. around P27171/L10).*

We added this information as suggested.

P27171/L17 *If I understand correctly, the initialisation of the H₂O profile does not have small scale structures. This could be problematic - see my concerns above.*

The climatological mean water vapor profile, which we used as model initialization, adds of course additional uncertainties to the results. An underestimation of the water vapor mixing ratio by 1 ppm would increase the frost by temperature by roughly 1 K. We could have tried to optimize the initialization to the particular winter, and thereby change the ratio of dehydrating and rehydrating layers, possibly such that they appear to be mass-balanced. However, such a balance might be fortuitous as the presence of wind shear cannot be ignored. Therefore, the multi-year climatological mean remains the best estimate available (and since the initialization is done at temperatures above T_{NAT} and before the occurrence of synoptic-scale ice PSCs within the vortex, we do not expect large deviations in the actual values from the mean profile).

Figure 4(a) *What's happening shortly before Day 18 before the nucleation event - mix-C12215*

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ing ratios on isentropes should remain constant unless there is mixing and/or condensation. Is this simply an artefact from the “contour” algorithm that generates the plot?

Figure 4a and d show time series of gas phase water vapor mixing ratios. Total mixing ratios (including liquid water and ice) stay constant on isentropes until sedimentation of ice particles sets in. The observed decrease in the gas phase at temperatures above the frost point is due to the growth of STS droplets, which take up water and also nitric acid (compare Fig. 7).

P27172/L6 *Do you have a hypothesis or concrete indications why ERA-Interim has such a large and large-scale temperature error? Do you know at which locations ERA-Interim assimilates radiosondes?*

A list of radiosondes assimilated in ERA-Interim is available online:
<http://www.ecmwf.int/research/era/do/get/index/29/28>

The closest location is certainly Sodankylä, Finland, which is included in this list. However, rubber balloons used for operational radio soundings are known to burst in the winter stratosphere already at lower altitudes due to the lack of solar radiation and the extremely low temperatures. Strong winds further complicate the balloon’s ascent. The nighttime soundings on 17 and 18 January 2010 experienced an early burst and reached only top altitudes of 109 hPa and 119 hPa, respectively. In situ temperature data from higher altitudes were therefore missing in the assimilation. The limitation in horizontal, vertical und temporal resolution of the ERA-Interim data might further contribute to temperature deviations.

P27178/L17 *I did not quite see what the text describes here; I see BSR in Fig. 3b, but no H₂O; H₂O is shown in Fig. 4b, and I can convince myself that the spikes in 3b and 4b are at the same altitude levels. However, this is only a visual impression, and the “perfect anti-correlation” noted in the text is difficult to verify. Perhaps it would be useful to show the measurements of H₂O and BSR in the same plot?*

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This would also show that the enhanced BSR between 440 and 490 K is not associated with a corresponding H₂O signal; consistent with the STS mentioned in the text.

We clearly mentioned that BSR measurements are presented in Fig. 3 and water vapor measurements in Fig. 4. Combining both would complicate the plots, which already consist of several panels with multiple axes. The companion paper focuses on the measurements, explains the profound anti-correlation in detail and presents the measurements of BSR and water vapor in the same plot (compare Fig 1. in Khaykin et al., 2013). We will refer to this in the revised manuscript.

P27179/L26 *The discussion up to this point left me somewhat confused. After reading several times, I think I understand what the authors are saying, but with all the caveats it becomes somewhat unclear which aspects of the model result and conclusions are robust (see also my concern in the general comments).*

See general remark above.

P27180/L15 *In which figure can I see that this scenario fails to explain observations? Is this in Figure 6? If so, perhaps mention it at this point - I was initially confused.*

We clarified this in the manuscript.

P27182/L6 *Dentrification or dehydration?*

Dehydration! Thanks for correcting this typo.

P27182/L6 *While your discussion regarding the ensemble members is certainly correct, the question is what can we learn from it? If the model result is sensitive to the details of the temperature perturbation - would you see the same “scatter” in observations if only you had a large enough number of observations? Any chance to get some statistics from CALIOP?*

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Indeed, a similar “scatter” is also visible in BSRs measured by CALIOP. To demonstrate the variability in the measurements, we looked at all CALIOP observations classified as ice or wave ice according to the PSC classification scheme by Pitts et al. (2011). We focused on two consecutive days, namely the 17 and 18 January 2010. Figure 2 shows a histogram of relative occurrences of CALIOP BSR values. As stated in the manuscript, we expected moderate BSRs for the majority of ice clouds to explain the observed dehydration. The observations confirm this: BSRs smaller 10 are observed in 65 % of the measurements and 89 % of the data have BSRs less than 20. However, a certain scatter of BSRs exists, possibly originating from temperature perturbations. We included this additional information in the revised manuscript.

P27182/L10ff *This statement hinges on the small scale temperature fluctuation amplitude being known very well. Is this the case?*

The temperature fluctuations, which we applied to the synoptic-scale trajectories, are resolved on very short timescales with wavelengths less than 400 km. In contrast, the temperature correction stretches over 24 h and about 2000 km. Despite the different resolutions, the amplitude of the correction is 3 times larger than the mean amplitude of the fluctuations. In turn, the small-scale fluctuations have much larger cooling rates. We agree with the reviewer that only little is known about small-scale temperature fluctuations, however, larger fluctuation amplitudes might not be able to compensate/replace the correction. Whereas the correction allows for a slow particle growth over a distinct period of time, fluctuations would induce rapid ice formation resulting in numerous, small ice particles, with too little sedimentation speeds. Large fluctuation amplitudes could also lead to an earlier evaporation of the ice particles.

P27183/L2 *Just to clarify, “This” refers to your work, not Khosrawi et al. (2011)?*

Yes, “This” refers to our work. We clarified this.

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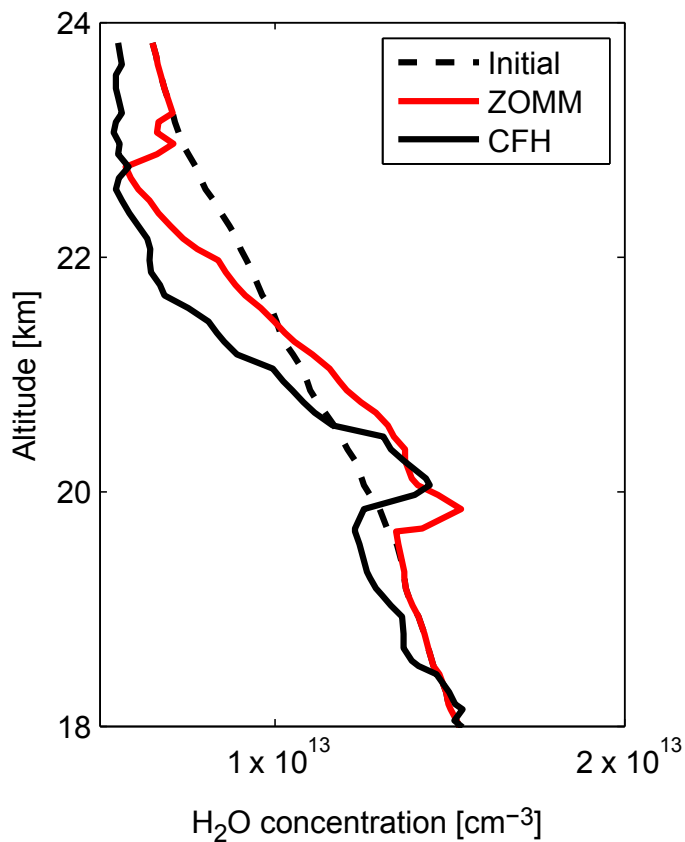


Fig. 1. Number densities of water molecules as a function of altitude. The CFH measurement (black) and the corresponding simulation (red) refer to S2. The climatological mean is included as a dashed line.

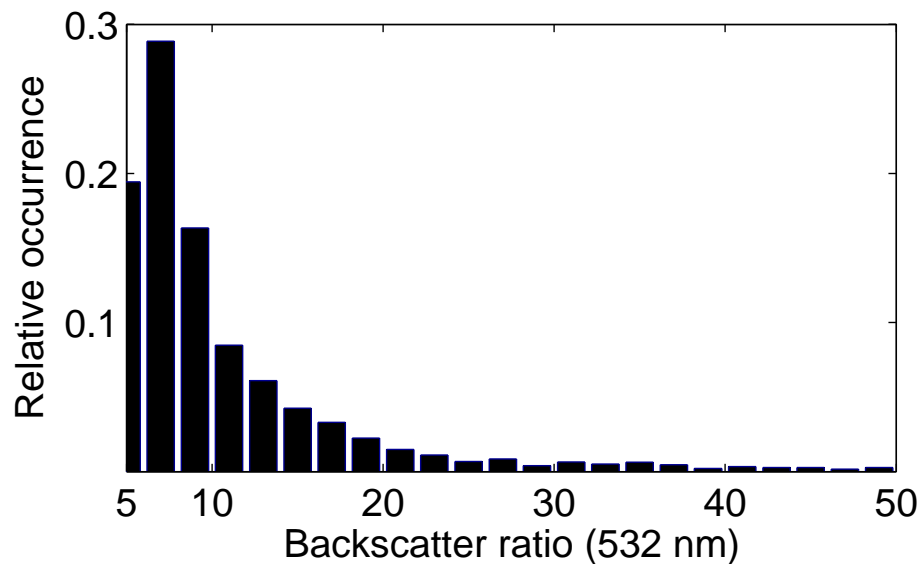
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Fig. 2. Relative occurrences of CALIOP backscatter ratios for all measurements obtained during the 17 and 18 January 2010, which have been classified as ice according to Pitts et al. (2011).

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