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Interactive Comment

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

Anonymous Referee #1

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General:

Engel et al. present a modeling study of ice cloud formation in the Arctic stratosphere. The paper is based on two soundings in the winter 2009/2010, that provide profile measurements of backscatter and water vapor. Further, CALIOP measurements are used. Using a column variant of ZOMM, the modeling tries to address the formation process of the ice PSCs. Specifically, sensitivity to details in the temperature field forcing nucleation, and the nucleation process (homogeneous versus heterogeneous) are discussed. Conclusions regarding these aspects are based on comparison of model results with observations of backscatter ratio and water vapor.

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 column-model, and caveats are mentionend. 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/re-hydration signature in mixing ratio shown in Figure 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 480K-540K in Figure 6 (lower panels), they would be completely off below 480K (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.

Minor comments: (PAGE/LINE)

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

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.

Figure 4(a): What's happening shortly before Day 18 before the nucleation event - mixing 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?

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?

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

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).

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.

P27182/L6: Dentrification or dehydration?

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?

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27163, 2013.

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