

Reviewer 1:

The authors have answered my questions/criticisms to my satisfaction as far as the present manuscript is concerned. The authors may want to amend/improve the manuscript as follows before submitting their final version:

Line 74: replace “polymerize” by “condense”

This change has been made.

Line 126: The relevant reaction for denitrification is ClO or BrO + NO<sub>2</sub> going to ClONO<sub>2</sub> or BrONO<sub>2</sub>. It is thus NO, and not NO<sub>2</sub>, that brings about the effect of lowering the rate of Cl<sub>2</sub> or BrCl re-formation in competition with Cl<sub>2</sub> or BrCl photolysis followed by rapid ozone destruction.

This has been corrected.

Line 193: “...not be important in the atmosphere”. The authors do not advance an argument that would obviate the presence of amorphous silicon or silicates in aerosol particles. We simply do not know.

This is discussed further in a later Section 4.4 of the paper and reference has been added to that in the methods section. Several atmospheric observations support the presence of solid silica, without metallic counter ions, and are not consistent with the presence of gel in the atmosphere.

Line 330: Dotted line in Figure 6 indicates 0.20 rather than the indicated 0.25 sq micron cm<sup>-3</sup> in the Figure caption.

This has been corrected.

Figure 10: Red curve (broken line) is nowhere mentioned. What is it?

In this figure the value of the surface energy used in the model is represented by the line type, and the assumed atmospheric HNO<sub>3</sub> concentration by line colour. The figure caption has been edited to more clearly describe the data sets shown.

Reviewer 2:

Thanks for your revised manuscript. I can follow your argumentation as a response to my review. However, I have still two minor, technical points:

In the new manuscript, NAX has been introduced. Please check if this is necessary and if you use it properly. Just as an example: Voigt et al. (2005) is talking about NAT and not NAX (line 293). In general, don't switch back and forth if it is just terminology. It makes it more difficult to understand.

The editor's comment regarding  $\alpha$ -NAT demonstrates that there remains interest in the field as to the differing phases of NAX which might form in the polar stratosphere, so we do think that it is important to distinguish when e.g. a previous study has measured nucleation of NAD as well as NAT. That said we had used NAX several times when referring to our atmospheric box model, where in this paper we have specifically made the assumption that NAT is nucleating, so those cases have been amended to specify NAT.

The usage of italic vs. upright fonts for variables is still confusing. I guess that this will be corrected by the journal. However, you need to correct at least your figures:

The overall rule is that symbols representing physical quantities (or variables) are italic, but symbols representing units, or labels, are roman (upright). You should not write whole words like "Temperature" in italic fonts.

We have removed italics from the names of variables where these were written out in figures.

#### Editor Comment:

Please comment on the polymorphism of NAT. Did you observe the nucleation of alpha-NAT or beta-NAT? This is an important question since the nucleation barriers for both phases and between them are rather different, see Weiss et al. 2016 and literature as quoted within.

Specific mention of  $\alpha$ -NAT has been added to the text (line 385) along with the fact that as we only observe the phase which grows and melts, we have only very limited information about the phase which initially nucleates. Our experiments are designed to assess the amount of nitric acid hydrate nucleation in the atmosphere, rather than the phase chemistry. What we can say is that given literature data on the stability of possible phases, we see some nucleation events which could only be NAT, and most could not be water ice. We note that the central assertion of Weiss et al. (2016) is that  $\alpha$ -NAT could nucleate on water ice given their similar crystal structure, however our experiments and analysis are targeted at atmospheric clouds which form in the absence of water ice. Since we see evidence of recrystallisation in some control experiments we are likely sensitive to it to some extent, and since we do not see it in heterogeneous experiments we do not believe that  $\alpha$ -NAT is forming and converting to  $\beta$ -NAT, however we do not feel there is sufficient evidence to include this conjecture in the main text of the paper.

Commented [A(J1)]: Not sure we need this?