

## ***Interactive comment on “Commentary on ”Homogeneous nucleation of NAD and NAT in liquid stratospheric aerosols: insufficient to explain denitrification” by Knopf et al.” by A. Tabazadeh***

### **Anonymous Referee #1**

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This commentary by Tabazadeh et al, on “Homogeneous nucleation of NAD and NAT in liquid stratospheric aerosols: insufficient to explain denitrification of Knopf et al.” is interesting and raises some valid questions about laboratory measurements of atmospheric aerosol nucleation rates. The author correctly points out the need for quantitative experimental data on the freezing process of the stratospheric particles. There are a few points that require further attention.

1) The author correctly points out that a small amount of contamination can result in deactivating the surface from freezing into hydrates. It, however, seems like the data from Knopf et al. is in visual agreement with the data of Salcedo et al (2001) for larger

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supersaturation rates. Would this be consistent with the problems of contamination?

2) There is clearly a problem with extrapolating data beyond the range of the experimental values. This applies both to the extrapolation of data obtained with large droplets (by Knopf et al 2002) to atmospheric conditions as well as the extrapolation of Salcedo et al's work to calculate nucleation rates of NAD and NAT at stratospheric conditions (by Tabazadeh, et al 2001). While the author has highlighted the problems associated with Knopf et al's study, it might be useful to also comment on the validity of the linear extrapolation of Salcedo's data to stratospheric conditions.

3) The statement ``Thus, even if the bulk samples of Knopf et al. had pristine surfaces, which is highly unlikely,...'' should be changed to ``Thus, even if the bulk samples of Knopf et al. had pristine surfaces,...'' as the author does not present any direct evidence of contamination on the bulk samples.

4) Please change ``Finally, for the reasons given above the Knopf et al experimental results are faulty and are not applicable...'', to ``Finally, for the reasons given above, the Knopf et al. experimental results may be faulty and may not be applicable ....''.

In my opinion, the study of Knopf et al and this commentary highlight the need for experiments consistent with conditions of PSC formation in the polar stratosphere. The problems of extrapolating data from bulk samples to sub-micron atmospheric aerosols are correctly pointed out in this commentary. This would lead one to conclude that the conclusions of Tabazadeh et al (2001) using data from Salcedo et al (for 15-85 micron particles) and that of Knopf et al, may not be entirely right (in the concluding paragraph, it may be pertinent to point out that the conclusions of both studies may be subject to revision with better experimental data).

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