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Atmospheric Chemistry and Physics Discussions

Interactive comment on "NAT-rock formation by mother clouds: a microphysical model study" *by* S. Fueglistaler et al.

Anonymous Referee #2

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Review on Fueglistaler et al., NAT-rock Formation by Mother Clouds: A Microphysical Model Study, submitted for publication in Atmos. Chem. Phys.

General comments:

This paper presents 1-D microphysical simulation studies of the growth of NAT particles, falling out of typical PSC type 1a or 1a-enh clouds under supersaturated conditions. The generated large particle properties are in good agreement with observations of NAT-rocks and the model study offers a good explanation for an efficient denitrification mechanism. A number of sensitivity studies are included in the paper to show the robustness of the suggested scenarios. Since denitrification is an essential process for chemical stratospheric ozone depletion, and since the role of NAT-rocks and the formation of these particles could be strongly linked to denitrification, the model study

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and the paper are of interest to the readers of the journal. Therefore I would recommend the paper to be published in ACP after the following remarks have been taken into consideration.

I have two general concerns about the paper. First, I think the model and the initial conditions of the simulations need a more careful description in order for others to repeat the calculations. Secondly, I always hesitate to accept that purely theoretical model simulations prove anything, but there is a tendency in the last section to give some firm conclusion without much discussion of alternative explanations of NAT-rock formation and denitrification.

Specific comments:

p. 30, line 12: What is meant by 'additional atmospheric dilution'?

p. 31, starting line 19, the sentence 'To break down ..': I find it very difficult to understand what you are trying to illustrate by the hypothetical case where the growing particles do not deplete the gas phase. Why is it a steady state case when particles are growing? Why should the particle flux be conserved? I would assume that the particle flux is an integral of number density times velocity, taken over the whole size range. However the second paragraph of this section (p. 32, line 2 onward) is quite clear. I would suggest to leave out the part with the hypothetical case between p. 31, line 19 and p. 32, line 2.

p. 32, lines 14 and 18: I find the wording 'Eulerian' and 'Lagrangian' to be jargon. I would suggest explaining in plain words what you mean.

p. 32, line 21: You should give the assumed radius value of the monodisperse particles in the mother cloud.

Starting p. 33, line 4: I guess the mother cloud is initially located between 22 and 23 km (you should write this). I guess there are initially no solid particles below the mother cloud, only liquid STS droplets in this region?

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p. 33, line 6: I think it would be relevant to show the assumed temperature profile and perhaps the initial nitric acid gas phase profile. If S-NAT=10 then you are certainly below the STS existence temperature. Since the STS droplets are in equilibrium it would be relevant to specify how much of the 8 ppbv HNO3 is contained in the condensed phase, in particular for a comparison with case c in Figure 3.

Paragraph, starting p. 32, line 18, and paragraph, starting p. 33, line 4: You should be more specific about the treatment of the liquid particles below the mother cloud: What are the assumed liquid particle size distribution and sulfate concentrations? How do you calculate the transfer of nitric acid from evaporating STS particles, condensing on the falling NAT particles?

p. 33, line 15: I assume that results in panels b, c, and d in Figure 2 refer to properties of the falling solid particles (you should mention this).

p. 33, line 16: I do not understand this sentence. I guess the FRL is just slightly below the base of the mother cloud, say at 21.5 km. As seen from Figure 2 there is a steady increase in number density (panel c) and particle number flux (paned d) at this level throughout the simulation, not a decrease. At 20 km I can see the reduction in particle number flux but not in number density.

p. 34, line 8: I guess the dependence of NAT-rock formation in your simulations would be both on number density and radius of the monodisperse particles in the mother cloud. As for the base line case you should also give the assumed radius in case b in Figure 3 (both for n=10**(-2) and n=1 cm-3) and explain how much the difference between the three cases could be attributed to different radii.

p. 35, line 18: I think the authors should be a little more modest. A theoretical model study does not prove anything; at most if offers one possible explanation, perhaps out of several other explanations. Using the word 'reveals' clearly makes this sentence too strong a statement. What essentially has been shown by the model is that NAT particles of typical PSC type 1a or 1a-enh sizes can grow to NAT-rock sizes as they

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fall under supersaturated conditions within 3-5 days days. This is really not surprising. To reveal the sources of NAT-rocks would, to my mind, require a much more comprehensive analysis of observational data and not a theoretical model study. The model study also offers an explanation for an efficient denitrification. However it has not been revealed if this is the 'typical' mechanism behind denitrification under Arctic and/or Antarctic conditions.

Technical corrections:

p. 35, line 12: change 'no to 'not'.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 29, 2002.

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