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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Mountain wave PSC dynamics and microphysics from ground-based lidar measurements and meteorological modeling" *by* J. Reichardt et al.

Anonymous Referee #2

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General

The authors present an analysis of lidar data of PSC obtained over Esrange (Sweden) on 16 January 1997. A comprehensive discussion of the meteorological conditions leading to these mountain wave PSCs provides a frame of reference for the discussion and interpretation of the lidar data. The substantial body of literature on lidar observation of mountain wave PSCs is properly referenced, although sometimes in a rather arbitrary way. Further, the authors must rely on observations at only one wavelength (355nm) for much of their discussion, and the lack of use of microphysical modelling affects their discussion, which remains often vague. The discussion of the microphysical properties of the observed PSCs often borrows from other publications, making it difficult for the reader to separate assumptions from results. Hence, their claim that this study is the first of its kind with a ground-based lidar may be true, but the results do not substantially improve our understanding of polar stratospheric clouds.

Specific Comments

(y/lx: refers to line x on page y).

Abstract:

l20: 'Its optical ... possibly due to a diminishing growth rate.' Do you mean the growth rate of ice particles? Ice particle growth is a relatively fast process, and high particle number density ice PSCs are usually not far from thermodynamic equlibrium with the gas phase. Hence, I suspect that your observed variability in ice particle size mainly reflects variability in temperature over the observation site.

l21: 'Later on, .. wave-processed LTA...' I guess that 'wave-induced' is a better term for what you want to say.

General: The authors often state that high cooling rates lead to NAT particle formation. Why? Since their analysis cannot contribute to this discussion, the authors should state why they think this is so. I assume their reasoning follows the 'NAT on ice' scenario, in which case they should cite the following publications:

Zondlo et al., 'Chemistry and Microphysics of Polar Stratospheric Clouds and Cirrus Clouds', Annu. Rev. Phys. Chem., 51, 473-499, 2000.

and

Luo et al., 'Extreme NAT supersaturations in mountain wave ice PSCs: A clue to NAT formation', JGR, 108 (D15), 2003.

1 Introduction

5833/l18-22: It appears that the authors do not understand the value of quasi-lagrangian observations. It is not that these observations are popular because one can make 'simplifying assumptions', as the authors insinuate, but the fact that these observations constrain the modelling of the cloud microphysics in a way a ground-based measurement never can. The success of quasi-lagrangian observations is based on the fact that they show the entire 'life cycle' of an air parcel (and the evolution of the aerosol within), and the 'simplifying assumptions' are that the flow is essentially stationary. In contrast, a ground-based lidar shows a sequence of basically independent observations. The authors seem to make a big story ('space-time convolution of the lidar data') of this simple fact.

2 Instrumentation ..

The discussion of the two lidar systems is somewhat confusing. I suggest that you first discuss the GKSS lidar and the quantities that you analyze, and then add a paragraph describing the U. Bonn lidar, and why it is not used for the analysis. I do understand your concerns about the depolarization, but can't you still use the backscatter coefficients at 532nm? If possible, please do so.

I consider the fact that you use a wider set of particle shapes than previous studies important. However, if only one wavelength is used, the conclusions are not compelling, see also comments on section 5 (PSC microphysical properties).

5837/l8: How do you arrive at a value of 20% for the uncertainty in $\bar{S}_{\mathrm{par}}$?

5837/l9-10: I suggest to skip 'and hence microphysical' - it is then easier to read.

5837/l13: 'low-R' you do not further use this acronym, so I suggest you write 'low backscatter'.

3 Meteorological setting

This section is well written and provides a good overview.

5839/l15: Could you please add in this description the integration time of the mesoscale model before the output was taken (i.e. what is the initialization date?).

4 PSC macrophysical properties

5842/l15: I assume 'stratospheric anomaly' refers to the temperature, so it may be clearer to write 'stratospheric temperature anomaly'.

5842/l17: Replace 'growth of R' with 'increase of R'.

5843/l3: You should give the assumed water mixing ratio here, I think the first time you mention your assumed value is on page 5846.

5843/l14: I suggest to replace '..the early stages of its life ..' with '.. of a PSC II, where the ice particles nucleated not far upstream of the observation.'

5843/l25: I consider the term 'originate' for LTA droplets inappropriate. You may could say that this cloud could have extended to the western side of the Scandinavian mountain ridge, where a PSC with similar properties was observed.

5844/l1-22: Please rewrite this paragraph - and reference Zondlo et al. and Luo et al. (references given above). Please remove speculations such as on line 13 'The earliest ...' - how do you know? I assume that the first thing to happen is that a type 1b cloud forms (following e.g. the scheme shown in Zondlo et al.), and 'when ice particles are nucleated directly above Esrange' - how do you know? Correct would be to state that the mesoscale simulation suggests that the ice particles formed not far upstream from Esrange.

5844/l26: '... similar features ...' - it is not obvious why an air-borne system should detect laminated vertical structure when a ground-based system does not.

5 PSC microphysical properties

5845/l9: see comment to 'Introduction'

5846/l18: Replace 'solid particle nucleation' with 'ice nucleation'. Also, in the following

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the reader is often confused whether you refer to ice or other solids, such as NAT. So please use 'ice' whenever you mean ice.

5848/l25: I am a bit confused by your HNO3-Profile as shown in Figure 6. In the first version of the manuscript, it was referenced as a MIPAS measurement, now the same profile is 'generated ... by scaling .. in a similar mountain-wave PSC case', with a reference to Larsen et al. 2002. What a reader would like to know here above all other things is *when* this profile was measured, and what portions of the profile were measured, and what portions were derived from 'scaling'. Since you imply that this maximum in HNO3 mixing ratio may lead to the observed enhanced backscatter, it would be interesting to see the vertical temperature profile, and a simple calculation of the lidar backscatter from a liquid droplet PSC assuming thermodynamic equlibrium.

5849/l14-15: You speculate about ice particles surviving in subsaturated air for 30 minutes, with 'heating rates ... very high'. I suggest that you discard this hypothesis also simply because micron-sized ice particles evaporate rapidly.

5849/l23-25: Please re-organize the sentence, you may want to simply keep the part that directly relates to your measurements, and cite only Voigt et al.

5850/l26: Replace 'suppress depolarization' with, if I understand correctly, '.. is sufficient to evaporate the ice.' And on the following line you could add '.. the case if solid particles, i.e. NAT, had ...' to avoid confusion.

5851/l7: Please cite here also Luo et al 2003. You may also want to compare your cooling rates with their Figure 7.

5853/l9-10: If you are sure that such small crystals cannot have aspect ratios of 0.5 or 1.5 then you should explain/reference why. You may discuss here the shapes and aspect ratios used in previous studies (Carslaw et al.,Tsias et al.,Wirth et al., Fueglistaler et al.) and discuss the differences. Again, if you could include S at 532nm, and the irregular shaped particles would still give the best match, then your conclusions would

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be more convincing than what is shown currently in Figure 8.

5853/l26: You compare two different clouds, which likely had different particle sizes. Hence you cannot draw conclusions that the different values are caused by the differences in the assumed particle shape. That different shapes lead to different results can be seen in your Figure 8.

5854/l16: First use of FDTD.

5854/l21: Hu et al. and Fueglistaler et al. look at different clouds, so it is not clear why you use their particle number densities. You state that only the 4.3um particles are consistent with the optical data, but the size range in Figure 8 ends at 2.8microns.

5855/l20: Why is a particle size of 2.2 microns 'somewhat small for ice PSCs'? Your discussion in this paragraph is very vague.

5856/l2-5: Krieger et al. (Applied Optics, 39 (21), 2000) measured the refractive indices of LTA - how do your computations compare with these measurements?

General:

It would help if the profiles 'M1' - 'M6' were marked in Figure 4.

Try to restructure section 5.3 - at present, the reader gets lost. Your discussion relies on results obtained elsewhere, and it is not easy to sort out which parameters are constrained by your lidar analysis and which are constrained by values that you take from other publications.

Figures:

Figs. 2 and 3: Maybe a bit larger.

Fig.4: Mark M1-M6.

Fig. 5: Show standard deviation for averaged profiles, lower part of Figure is hard to read, maybe a bit larger.

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Why is Figure 5 smaller than Figure 7?

Fig. 6: See comment on profile given above.

Fig. 8: Again, a bit larger would improve readability.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5831, 2003.

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