

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

J. Reichardt et al.

Received and published: 3 May 2004

The authors gratefully acknowledge the reviewers' efforts on carefully reviewing the manuscript. We revised our paper following most of their recommendations. Our reply to the reviewers' comments is given below.

Reviewer 2 (Comments Published: 6 December 2003)

General

1.) *The authors' 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. So far ground-based lidar measurements have been used in PSC studies that are more of a statistical/climatological nature. These studies form the background of our work. The main purpose of our contribution is to show how to*

make ground-based lidar data more valuable for cloud research. We demonstrate that by use of a mesoscale meteorological model one can actually interpret ground-based lidar measurements of PSCs in terms of microphysics and dynamics. Furthermore, our paper concentrates on methodological aspects as well. Substantial improvement in the understanding of PSCs, however, cannot be expected from our investigation.

2.) *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.* We rewrote our discussion without borrowing, so this problem should be resolved.

3.) *The authors must rely on observations at only one wavelength ..., and the lack of microphysical modeling affects their discussion, which remains often vague.* In the case of the NAT/LTA PSC, we demonstrated that lidar ratio and depolarization ratio at 355 nm are sufficient to constrain the microphysical retrieval because of their opposing dependence on particle size. In fact, these two optical properties are much more benign functions of particle size, and thus better suited for the retrieval, than the color ratio (which fluctuates strongly with size). We even bring experimental and theoretical data face to face to show how well they fit (Fig. 8), which is not done in most other similar studies. In the case of the ice particles, an additional, longer observation wavelength would have indeed aided the microphysical retrieval, as we stated in the manuscript. Certainly, it would have been desirable to have a microphysical model available for the analysis. However, we can only advance step by step. The first step was to combine ground-based measurements with mesoscale modeling, the next step will be to add a microphysical model.

Specific Comments

4.) *Abstract, I20:* We removed this speculation.

5.) *I21:* We followed this recommendation.

6.) *General:* *The authors often state that high cooling rates lead to NAT particle for-*

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

mation. ... I assume their reasoning follows the 'NAT on ice' scenario, in which case they should cite Our data are in good agreement with the theoretical relationship between NAT particle number density and cooling rate as derived by Luo et al. (J. Geophys. Res., 108(D15), 2003) assuming NAT forming via deposition nucleation on ice particles, and thus can be considered as additional observational evidence of this hypothesis. We rewrote the whole manuscript to make this clear.

7.) *1 Introduction: 5833/18–22:* We fully acknowledge the value of quasi-Lagrangian studies. We apologize, the text was clumsy and misleading. We rewrote the Introduction.

8.) *2 Instrumentation ...: Restructure the discussion of the two lidar systems. Why don't you use 532-nm backscatter ratios?* We followed this recommendation. Use of the U. Bonn lidar observations is restricted to visualizing the development of the macrophysical PSC properties during daytime, it is not considered for the microphysical interpretation of the PSC data. The reason is an inconsistency in the depolarization data. Particle depolarization ratios of the water-ice PSC observed with the U. Bonn lidar in the afternoon of 16 January 1997 are overwhelmingly < 0.04 at 532 nm, whereas δ_{par} values obtained with the GKSS Raman lidar are > 0.5 . This pronounced discrepancy cannot be explained by wavelength-dependent scattering by the presumably rather large ice particles. Because our 355-nm depolarization ratios are calibrated without critical assumptions and, moreover, are in good agreement with theoretical optical properties of micron-size crystalline particles we have to assume that the U. Bonn depolarization data are inaccurate. As a consequence, we had to exclude also 532-nm backscatter ratios from the microphysical retrieval because of their sensitivity to polarization.

9.) *I consider the fact that you use a wider set of particle shapes than previous studies important. However, ... conclusions are not compelling.* Please see our reply to 3.).

10.) *5837/18:* See manuscript.

11.) *5837/19–10; 5837/113:* We changed the wording.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

- 12.) *3 Meteorological Setting: Well written:* Thank you.
- 13.) *5839/I15:* The initialization date was added.
- 14.) *4 PSC macrophysical properties. 5842/I15; 5842/I17:* We changed the wording.
- 15.) *5843/I3:* We shortened Section 4, suggestion does not apply anymore.
- 16.) *5843/I14; 5843/I25:* We changed the wording.
- 17.) *5844/I1–22:* The reviewer is right. We did not only rewrite this paragraph but the entire Section 4. We hope it is now more succinct and less confusing. All speculative statements have been removed.
- 18.) *5844/I26:* The paragraph was discarded.
- 19.) *5 PSC microphysical properties. 5846/I18:* We followed this recommendation.
- 20.) *5848/I25:* The HNO_3 profile is a standard profile used by the mesoscale modeling group to calculate NAT equilibrium temperature. Since this profile is not related to the measurement case of 16 January 1997 it is inappropriate to use it for the interpretation other than estimating T_{NAT} . Therefore we removed all such interpretative statements.
- 21.) *5849/I14–15:* The statement was discarded.
- 22.) *5849/I23–25:* We followed this recommendation.
- 23.) *5850/I26:* To shorten the manuscript and to increase its readability (as the other reviewer suggested) we omitted this part of the discussion.
- 24.) *5851/I7:* We followed this recommendation.
- 25.) *5853/I9–10:* This is an interesting topic. Freezing experiments of water drops and in situ measurements in cold cirrus clouds show that the ice particles are initially equidimensional. Thus it seems reasonable to assume that this is also the case of the ice particles that nucleated in LTA. Unfortunately, as far as we know no experimental proof exists for this assumption (probably due to the small size of the LTA droplets).

Upon growth, the initially equidimensional particles may evolve into particles with more extreme aspect ratios (as can be seen in cirrus clouds). So PSC II particles may well have aspect ratios $\gg 1$, but only for particle sizes much larger than the ice nuclei. The probable reason why in previous studies rather oblate or prolate particles were retrieved is the assumed spheroidal particle shape. Unlike our polyhedral particles, spheroids with an aspect ratio of 1 do not generate depolarization at all (spheres), and thus in order to match the observed (high) values of depolarization ratio, aspect ratios are over/underestimated. The request of the other reviewer to shorten our manuscript does not allow us to include this discussion. We hope the interested reader will read this comment.

26.) *5853/126*: The reviewer is correct. Referring to Fig. 8 we now emphasize that the retrieval of microphysical properties from lidar data depends on the assumptions made about particle shape, and conclude that one has to be careful with microphysical data that were obtained using only one particular particle model.

27.) *5854/116*: We do not use the acronym 'FDTD' anymore.

28.) *5854/121*: The reviewer is correct, our line of reasoning was too speculative. We rewrote the paragraph.

29.) *5855/120*: We rewrote the paragraph.

30.) *5856/12–5*: Our computations compare well with the results obtained with the refractive index computer code provided by Krieger et al. (Appl. Opt., 39(21), 2000). We included the reference.

31.) *Section 5.3, General*: We restructured Section 5.3. We hope it is now more succinct and less difficult to follow. Now only retrieval results are presented that do not rely on results obtained elsewhere.

32.) *Figures, size*: We cannot influence the size of the figures as they appear in ACPD.

33.) *Fig. 4*: Measurements M1–M6 were marked.

34.) *Figs. 5–7: We included error bars.*

Reviewer 1 (Comments Published: 9 January 2004)

General

35.) *... These observations are therefore much more restricted than those that have previously been analyzed by others.* Please see comments on 1.) and 3.).

36.) *This is an exceptionally dense and difficult to read paper... I don't think this work should be published until the authors have gone through the paper and extracted what they believe to be the main new findings and presented them in a clear and concise manner. ... new science is lost among the excessive complexity of the writing.* Different authors have different styles, but constructive comments are very welcome. Our paper is a methodological one, so we think it is necessary to explain the methods applied in detail. And since our study touches on several different scientific disciplines it is really difficult to write a short and easy-to-read paper. Nevertheless, we completely revised the text. Except Section 3 (see comment on 12.)), all Sections were rewritten, shortened and restructured to increase readability and to better present our findings.

37.) *My major criticism ... is the claim that it is the first study to 'demonstrate that ground-based lidar measurements of PSCs can be comprehensively interpreted if combined with mesoscale meteorological data'. It seems to me that very little can be extracted from these data (...) because very little is known about what was happening upstream of the ground station. ... the uncertainties in upstream processes seem to preclude a thorough analysis of processes.* Probably, we have a science background problem. Reading the remarks of the anonymous reviewer we get the impression that he/she never worked with data measured with ground-based lidars but was/is involved in quasi-Lagrangian PSC studies. If this was correct, it would mean that he/she never experienced the difficulties associated with the interpretation of ground-based lidar measurements and could not fully acknowledge the improvements we made in understanding the measurements by employing mesoscale meteorological data. Of course,

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

quasi-Lagrangian studies are superior to studies with ground-based lidars, but not all lidars are on board an aircraft! Our primary goal is to show how ground-based lidar data can be made more valuable for cloud research. Probably, the reviewer expects too much 'science' from our manuscript which is primarily a methodological one. For this reason, we downgraded our language and made sure that terms like 'process', 'evolution', and 'development' are not used in a microphysical context. Furthermore, we give proper credit to quasi-Lagrangian studies in the Introduction.

Technical Comments

38.) *Abstract, Define 'NAT activation', explain 'diminishing growth rates'.* The new Abstract does not contain these expressions anymore.

39.) *The final sentence sounds like you are the first to study PSCs in this way.* No, we do not say so. We say 'that our study demonstrates that ground-based lidar measurements of PSCs can be comprehensively interpreted if combined with mesoscale meteorological data', and that is correct.

40.) *5833/5:* In the revised manuscript we avoid the term 'convolution in time and space' (although we think it is appropriate), and give proper credit to quasi-Lagrangian studies.

41.) *5833/20:* We followed this recommendation.

42.) *5834/20:* The revised manuscript does not contain emotive terms like 'life cycle' anymore. Please see also comment on 37.).

43.) *5834/16–27:* The data were rejected for experimental reasons (and not because we observed different clouds), see comment on 8.).

44.) *5836/1:* The reviewer is correct, the definition for depolarization we apply is the normal one. What we do differently is the way we calibrate our depolarization measurements (by use of the novel three-wavelength method).

45.) *5836/15:* Using S as the symbol for the lidar ratio (the ratio of extinction coefficient

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

to backscatter coefficient, NOT 'extinction to BSR') is common in the lidar community. In the revised manuscript we make sure that the reader does not mistake S for backscatter ratio.

46.) 5836/17–, 5836–7: We restructured and shortened Section 2 to increase its readability and to better emphasize the main points.

47.) Section 3. *The whole of this section is exceptionally dense and fails to get over the main points, ... Please consider shortening it to one brief paragraph... .* It is interesting to see how opinions can differ. See comment on 12.).

48.) Section 3. *... particularly considering that a detailed analysis of this cloud event has appeared elsewhere.* We do not know of such a detailed analysis published elsewhere. Unfortunately, the reviewer does not give a reference.

49.) 5841/24: We followed this recommendation.

50.) 5841/26: Probably, using 'higher' or 'warmer' is a matter of taste. Once a colleague (native English speaker) advised me to use 'warm' instead of 'high'!

51.) 5841/27: We think the statement is appropriate.

52.) Section 4. *...excessively dense. You should consider removing 2/3 of the text... .* We restructured Section 4 and shortened it considerably. However, we failed to reach the suggested quota.

53.) 5845/7; 5845/8; 5845/10–14: We discarded this paragraph.

54.) 5847/22–: We rewrote the discussion to make it more concise.

55.) 5848/26–: Lidar ratio is about the same for measurement intervals M2 and M3. Since the optical properties of the NAT particles remain unchanged (as can be seen in Fig. 6), those of the coexisting LTA droplets must remain unchanged also.

56.) 5849/14–: See comment on 21.).

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper