

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

Anonymous Referee #1

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The paper describes observations from a single ground-based lidar system downwind of the Scandinavian mountains during a period of strong wave-cloud activity. Much is already known about such wave events from many previous detailed studies in which much more has been known about the PSC properties over a larger area. In this work, the observations are restricted to a single lidar wavelength and observations are available at only one location. These observations are therefore much more restricted than those that have previously been analysed by others.

This is an exceptionally dense and difficult to read paper that goes into great detail on stratospheric wave clouds, PSC development, optical models, microphysical properties, etc. Unfortunately, the scientific message, if there is one, is buried. I don't think

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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.

There are some new aspects in this paper, such as the extension of optical retrievals to irregularly shaped particles but, again, the impact of any new science is lost among the excessive complexity of the writing.

My major criticism, beyond the paper's impenetrability, 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 (other than local PSC properties) because very little is known about what was happening upstream of the ground station. Sure, the wave model provides some input, but the uncertainties in upstream processes seem to preclude a thorough analysis of processes. The observations have been comprehensively analysed, but the various bits fail to tie together to deliver new knowledge.

I would suggest a complete revision and refocussing of this paper to emphasise the new knowledge that has been gained. In so doing, the authors should consider significant cuts in some of the very lengthy descriptions, which currently serve to frustrate rather than guide the reader.

Below are some more technical comments on part of the paper.

Abstract

Define 'NAT activation'. I don't understand the term 'diminishing growth rate'.

The final sentence sounds like you are the first to study PSCs in this way. Others, like Larsen and coworkers, have managed to get a lot out of point measurements by combining with meteorological models (albeit not from lidar), so I don't understand the big hoorah here.

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5833/5. What does 'convoluted in time and space' mean? Perhaps this statement arises because you are thinking about data from a single ground station. The following sentence about lifecycles does not give proper credit to the numerous studies that have already solved the problem you refer to. I am referring here to quasi-Lagrangian studies.

5833/20. The wording 'simplifying assumption' makes it sound like the quasi-Lagrangian studies have been limited. Rather, I think you should say that it has been difficult to use ground-based observations because they lack the additional dimension that is offered by aircraft observations.

5834/20 The sentence "Because of favorable tropospheric weather conditions the two Esrange lidars could monitor the development of the quasi-stationary PSC system over its full life cycle." is very confusing. The lidars don't monitor development of particular cloud elements; they just see bits of cloud floating past that are not connected in any way by processes that can be integrated in time (which is what I understand by development). It is unlikely that the cloud was stationary for the whole day. What is meant by 'full lifecycle'? This term has been applied to quasi-Lagrangian observations made over a short period of time in which the cloud really can be thought of as stationary. I don't think it is appropriate for you to use this term here in the same way.

You go on to say 'For the first time, a mesoscale model simulation is utilized to resolve the time-space ambiguity of ground-based lidar observations'. I think at this point that you are making a meal of what you are doing. Basically you used a model to understand why the PSCs might be changing at a given point in space, and others have done likewise with different data. Dressing the science in this grand language doesn't change this fact. I suggest a downgrading of your description if you are not to mislead readers.

5834/16-27. It is not clear why one set of lidar data were rejected. Why did the two systems measure very different backscatters? Did they observe different clouds?

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5836/1. I think a definition of depolarization ratio at this point would help. Isn't the normal definition of depolarization the same as you give on line 4? I'm confused about what you are doing differently from everyone else in the past.

5835/15. You risk confusing readers by using S as extinction to BSR, where others have used S to mean BSR. A footnote to point out this clash is at least needed.

5836/17-. I'm confused about what you can and can't do. It sounds like you can't measure extinction in the stratosphere, but then you go on to describe how you use optical depth. Is it that you can't measure extinction profile, but you can measure integrated extinction? If so, just say this.

5836-7. These two pages are dense and hard to follow. At the end of it, I'm not really sure what has been used. Please consider revising this text to simplify the language and emphasise the main points.

Section 3. The whole of this section is exceptionally dense and fails to get over the main points, particularly considering that a detailed analysis of this cloud event has appeared elsewhere. Please consider shortening it to one brief paragraph that is relevant to the study and remove the excessive padding.

5841/24. The phrase 'distorted quadrupole field, with the 192-K isothermal lines being the center axes' is impenetrable. Please consider using plain English and try to adhere to the concept that sometimes 'less is more'.

5842/26. Change 'warmer temperatures' to higher temperatures. Otherwise how do you define 'warm'?

5841/27. 'Due to the long-lasting flow past the mountain ridge and the associated dilatation of the horizontal wavelength this pattern reverses over time.'. Again, consider simplifying.

Section 4. Overall this section suffers, in the same way as section 3, from being excessively dense. You should consider removing 2/3 of the text to produce text that someone

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might have the energy to read. I found the summary plenty long enough for me to understand the general situation, which is all that is required here.

5845/7. What 'problem'?

5845/8. What 'approach'?

5845/10-14. The meaning is quite unclear here. What is all this 'space-time convolution'? You have a set of observations from a single site which you wish to interpret as best you can with a 3-D model. There are limitations as in all such measurements, but I have not seen others describe these limitations as a problem with space-time.

5847/22-. The 5 lines here seem to say (after many many additional words) that you really don't know what happened upwind. This is correct, and hence the limitation of single position measurements when trying to understand processes.

5848/Last 2 lines and first 2 lines of next page. I don't follow the arguments here.

5849/14-. These 3-4 lines about ice survival don't make sense. What has heating rate to do with ice survival? How much is the temperature above Tice? I doubt that ice would survive so long in air above Tice.

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

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