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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Do tropospheric clouds influence Polar Stratospheric cloud occurrence in the Arctic?" *by* P. Achtert et al.

Anonymous Referee #2

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General Comments:

This paper examines the possible relationship between the occurrence of polar stratospheric clouds and tropospheric clouds in the Arctic using observations from the spaceborne lidar on the CALIPSO satellite. Several previously published studies have investigated the relationship between PSCs and underlying tropospheric clouds over the Antarctic. In this paper, a statistical approach is used (similar to the previous Antarctic studies) to count how frequently PSCs occur above various kinds of tropospheric cloud systems. So in effect, this study is an extension of these earlier works with a focus on the Arctic instead of the Antarctic. In that sense, this paper would make a new contribution to our knowledge of the relationship between PSCs and tropospheric clouds and would merit publication. However, the discussion of the analysis approach is at times

confusing and lacking in sufficient details to properly judge the overall merits of this study. Another shortcoming is the use of only one year of Arctic data. Given the very high year-to-year variability of conditions in the Arctic and the overall low occurrence of PSCs relative to the Antarctic, statistics based on just one year of data may not be representative of the Arctic in general. The overall statistics and resulting conclusions would be substantially strengthened by including multiple years of data in your analyses (there are at least five Arctic winters available in the CALIPSO data record). Before I can recommend publication in ACP, the authors, at a minimum, need to clean up their discussion of analysis methods, taking into consideration my specific comments below. In addition, I hope the authors consider adding addition years of CALIPSO data to their analyses as this would make their results much more convincing.

One of my major concerns is the specific PSC detection and composition classification approach as described in the paper. On page 3, the authors state that "a PSC is identified if the backscatter ratio R is larger than 1.06." Over what spatial averaging scales is the detection being applied? Pitts et al. (2009) state that their R threshold for PSC detection is about 1.32 for horizontal averaging scales of 135 km (I believe their vertical scale is 180 m). The R value of 1.06 that you use is significantly smaller than the Pitts et al. values, so I assume that you must average the data to much larger spatial scales to achieve this. On page 4, you discuss different choices of 'longitudinal' averaging intervals for the data presented in Figure 1 (1o and 10o longitudinal). What do you mean here by 'longitudinal' averaging? The CALIPSO data are acquired along an orbit track at fixed spatial (and temporal) increments. The longitude spacing between profiles along the orbit will vary depending on the latitude. Would it not make more sense to average along the orbit track with some fixed averaging window? Do you really mean that you average all CALIPSO data that fall within a fixed longitude window? Please clarify this. You also mention on page 4 that 'longitudinal' averaging can affect the mean backscatter ratio and, hence, the composition classification. Therefore, you decided to use 'different longitudinally averaged ranges as described in Pitts et al. (2011). But you don't discuss the specifics of what averaging ranges you

actually use. I believe Pitts et al. (2011) used a successive averaging scheme in steps of 5, 15, 45, and 135 km horizontal by 180 m vertical. For these specific averaging scales, Pitts et al. (2009) list typical threshold values of R for PSC detection as 2.6, 1.82, 1.51, and 1.32, respectively. Obviously, your stated R value for detection of 1.06 is not consistent with the Pitts et al. analyses. So exactly what scales did you use in your analyses and what are your PSC detection thresholds that correspond to these averaging scales?

Specific Comments:

P.2, L.33-34: Why do you say that in particular the formation of type II PSCs is strongly controlled by the detailed structure of the temperature profile? Wouldn't the formation of all PSCs be strongly tied to the temperature profile?

P.2, L.34-36: Clearly, temperature modulations from orographic-induced waves have a large impact on Arctic PSC occurrence, especially over Scandinavia, but is it really true that 'Arctic PSCs are mostly formed' due to these waves? What fraction of Arctic PSCs do you think are formed due to these waves?

P.3, L.76-79: Is R=1.06 really the threshold you used for PSC detection? At what spatial scale?

P.3, L.80-81: The sentence beginning with "The aerosol depolarization. . ." is not correct. Isn't the aerosol depolarization ratio simply defined as the ratio of the perpendicular and parallel backscatter coefficients (not the ratio of R's)?

P.3, L.88-89: This sentence says that you used the Level 1 attenuated backscatter product to identify the altitude range and the spatial extent of possible underlying tropospheric clouds. But in the previous paragraph, you say that information on underlying tropospheric clouds came from the CALIPSO Level 2 cloud and aerosol layer product. Which was it?

P.4, L.100: Exactly how do you apply this 'longitudinal' averaging? According to Winker

et al. (2007), the CALIPSO data is acquired at a rate of approximately 20 Hz which produces an along-orbit footprint every 333 m. So the profile spacing is a fixed increment along the orbit track (although the exact spacing varies with altitude). So the obvious way to average the data is over some fixed spatial scale along the orbit track, not in longitude. The longitude spacing between profiles along the orbit will vary depending on the latitude. So I'm not sure why or even how you would average the data longitudinally- can you explain this in more detail? Does 'longitudinal' averaging really just mean averaging along the orbit track? Maybe it is just a poor choice of wording then.

P.4, L.102-104: What longitudinal averaging ranges did you actually use? How did changing the averaging range affect the R thresholds for PSC detection?

P.4, L. 112: How do you define a PSC here? Is each of the 211 PSCs a separate cloud distinguishable from another PSC by some spatial separation?

P.5, L. 131-144: You need to discuss Figure 4 in more detail. What are the implications for PSC composition being impacted by tropospheric clouds? Can you comment on what physical mechanisms would allow the presence of underlying tropospheric clouds to affect the microphysical makeup of PSC particles? Is it possible that your large spatial averaging scales could produce composition classifications that aren't representative of the true nature of the clouds (especially if the clouds are inhomogeneous on relatively small scales)? Looking at Figure 4, in general there aren't significant differences in the patterns shown in the different panels (except for panel a) - only the density of the points seems to change. What are the main points you're trying to make with this set of figures?

P.5, L.146-150: I think there is substantial evidence in the literature that tropospheric disturbances can produce adiabatic cooling over extensive vertical scales, well into the lower stratosphere and enhance the formation of PSCs (e.g. Teitelbaum et al., 2001). So clearly there would be a correlation between deep tropospheric cloud systems pro-

duced by these disturbances and PSCs that are formed by the adiabatic cooling the disturbances produce in the lower stratosphere. Is it possible that this is all your analyses are showing? Do you have any insight to what other mechanisms could produce simultaneous PSCs and tropospheric clouds?

P.5., L.156-157: In the introduction you stated that "Arctic PSCs are mostly formed due to gravity-wave-induced temperature modifications." Here you state that "the orographic influence (mountain generated small-scale waves) is rather small." Does this imply that the Arctic winter 2007/08 is anomalous and not representative of typical Arctic PSC seasons?

P.6, L.162: Minor point- since the Adhikari et al. (2010) study preceded your study, I would suggest rewording the sentence "This is in agreement with our observation" to "Our observations are in agreement with the Adhikari et al. (2010) findings"

P.6, L.173-175: It seems surprising that CTT's as high as 273 K are present given the typically cold conditions in the upper troposphere in the Arctic winter. Did you have a minimum altitude requirement for cloud top or did you simply extract the highest cloud in each profile? Does this mean that if only low cloud was present (i.e. low stratus deck), you would extract the CTT for the top of this stratus deck which may only be a few kilometers above the surface? Would you expect such low clouds (or very thin clouds) to have much of a radiative impact on the lower stratosphere? How would your results change if you restricted the analyses to just the deep tropospheric cloud systems or very high clouds? I would expect these to have the biggest radiative impact.

P.7, L.197-198: This last paragraph seems speculative at best. How would changing storm tracks lead to increased ozone depletion? How is this related to your study? The Simmonds et al. (2008) results were based on 40 years of Arctic data, but cyclone properties exhibited high interannual variability. Of more relevance to your study is the location of cyclones during the 2007/08 winter- can you determine this and relate the occurrence of strong cyclones with PSCs and underlying tropospheric cloud systems?

How does your study contribute to a better understanding of the linkage between tropospheric dynamics and PSC occurrence?

Technical

P.5, L.129: "occured" should be spelled "occurred."

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32065, 2011.