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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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

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We thank referee 2 for the review. Below we respond to the questions/comments raised by the referee.

Referee: 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,

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Interactive Discussion

Discussion Paper

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.

Response: We agree with the reviewer that a larger data set over more years would be desirable. However, our intention was to perform an analysis that is as precise as possible and also accounts for thin PSCs that might not be detected with an automated retrieval. Hence, expanding the current data set with an identical high quality for several years is time-consuming and beyond the scope of this study. We could not follow the reviewer's suggestion but we think that our high-quality data set is of interest for future studies.

We changed the data analysis and discussion part of the paper with respect to the comments of both reviewers (See answers to the specific comments).

Referee: 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

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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.

The PSC detection threshold used in the study by Pitts et al. (2009) is defined conservatively with fixed horizontal averaging scales and no discrimination between profiles that do and do not contain PSCs. Therefore the PSC area presented in their study likely represents a lower limit of true PSCs coverage. In contrast to Pitts et al. (2009) only profiles that were identified containing PSC signals were considered in our data set. Averaging only over PSC-containing profiles furthermore increases the PSC signal-to-noise ratio. Since we first identify profiles with PSC signals and then average these profiles we can use the detection threshold value of R>1.06 (Blum et al., 2005; Achtert et al., 2011) which is much smaller than the one used in the Pitts et al. (2009) study. PSCs are only identified in a lidar profile if R>1.06 over three height bins as is now stated explicitly in the text. The averaging scale is adapted to the actual extend of certain PSC layers.

Response: For a better understanding we changed the sentence on Page 3 to: "A PSC is identified if the backscatter ratio R is larger than 1.06 over three height bins." We also included the sentence: "To increase the signal-to-noise ratio only profiles including a PSC were considered for averaging. In contrast to our approach Pitts et al. (2009) chose their detection threshold conservatively and depending on the averaging length without excluding PSC-free profiles. They state that their study is likely to represent the lower limit of true PSC coverage."

Referee: 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.

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Response: We used the coordinates given in the CALIPSO files for averaging. We do average along the CALIPSO track. To make this better understandable we now refer to the "term along-track average" rather than "longitudinal average" in the paper.

Referee: 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?

Response: Our intention was to state that averaging intervals have to be decreased if the composition of a PSC varies. To make this clear we changed the corresponding statement to: "Different choices of along-track averaging intervals of the backscatter ratios (black and red lines in Figure 1c) do not change the altitude range of the observed clouds. They, however, affect the resulting backscatter ratio, which might have an impact on the PSC classification. To prevent this, averaging intervals for the cloud classifications were decreased in case of varying properties within one PSC as was suggested by Pitts et al., (2011)."

Referee: 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?

Response: The reviewer is correct. We changed the sentence to: "In the Arctic stratosphere, the formation of PSCs is strongly controlled by the detailed structure of the temperature profile."

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Referee: 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?

Response: In the introduction to the paper we refer to what is reported in the literature. In fact we don't agree and rather believe that orographic waves are of minor importance for PSC formation in the Arctic. We come back to this in the discussion of the findings of this study. To make our intention more evident we rephrased the statement to: "Previous studies (Carslaw et al. 1998, Dörnbrack et al. 2000, Höpfner et al. 2006, Blum et al. 2005, Juarez et al. 2009) concluded that Arctic PSCs are mostly formed due to gravity-wave-induced temperature modifications." In the introduction we also referred to papers which discussed synoptic cooling as a possible PSC formation mechanism (see line 43 to 45).

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

Response: Yes, a value of 1.06 is used on all scales. As mentioned above, note that we only average over profiles that actually contain PSC signals and that the same threshold is also used in studies by Blum et al. (2005) and Achtert et al. (2011). The averaging scale is adapted to the actual extend of certain PSC layers. (see also previous general comments, bottom of page 1)

Referee: 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)?

Response: You are right. We changed the sentence to: "The aerosol depolarization ratio is defined as the ratio of backscatter signal in the plane of polarization perpendicular and parallel to the one of the emitted laser light."

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Referee: 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?

Response: Meteorological information such as cloud top height and cloud top temperature from the underlying tropospheric clouds are taken from the CALIPSO Level 2 cloud and aerosol layer product. We changed the statement to clarify: "To obtain meteorology information about underlying tropospheric clouds (e.g., cloud top height and cloud top temperature) the CALIPSO level 2 cloud and aerosol layer product (CAL_LID_L2_05km_CLay-Prov-V3-01) was used on a 5 km horizontal grid."

Referee: 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.

Response: As stated earlier we average along the CALIPSO track. We now refer to it as along-track average.

Referee: 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?

Response: The threshold is independent from the averaging range. See answers above on pages 1 and 3.

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Referee: 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?

Response: Each cloud is a separated one. PSC are identified if R is larger than 1.06 along one CALIPSO track. The threshold can lead to more than one PSC per track if a gap with R<1.06 is observed. However between two different tracks, which are shifted in time and space, we cannot be sure if the same PSC is counted twice. This fact is also mentioned in the caption of Fig 3: "PSCs with a large spatial extent might be counted multiple times as they show up subsequent CALIPSO tracks."

Referee: 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?

Response: We changed the discussion of Figure 4 with respect to the publication of Adhikari et al. 2010: "Cooling associated with the presence of tropospheric clouds has an impact on the microphysical properties of PSCs as is discussed by Adhikari et al. (2010). Their finding show that high and deep-tropospheric cloud systems have an significant effect on the relative occurrence of different PSC types, especially on ice PSCs. PSCs of the type ice and mix associated with cirrus and deep tropospheric clouds showed larger backscatter ratios compared to PSCs associated with no cirrus and deep tropospheric clouds. (Adhikari et al., 2010) concluded that this is due to an increase of the nucleation efficiency, providing higher particle number concentrations. Our observations are in agreement with the findings by Adhikari et al. (2010). Figure

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4d, e, f (associated with deep tropospheric clouds) shows larger number of observations with higher backscatter ratios compared to Figure 4c which is associated with PSCs observation without underlying tropospheric clouds. In contrast to Adhikari et al. (2010) our study revealed no increased backscatter ratio within PSCs observed above cirrus clouds."

Referee: 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? Internal gravity waves. . .

Response: This is exactly what our analysis is showing. Our study is in agreement with Teitelbaum et al., (2001).

We sharpened the respective part of the discussion to make it clearer for the readers: "Our analysis shows that the relative occurrence of different PSC types is related to the occurrence of deep tropospheric cloud systems. The deep tropospheric cloud systems may affect the PSC formation because of their ability to cause both radiative and adiabatic cooling in the lower stratosphere (Teitelbaum et al., 2001; Wang et al., 2008; Adhikari et al., 2010). According to Adhikari et al. (2010) the effect of additional cooling might be more important for ice particle formation than for STS formation due to the fact that lower temperature are required to form stratospheric ice particles. Fromm et al. (2003) showed that the average tropopause height in the Arctic associated with PSC observation is significant higher than that of clear sky observations. They conclude that PSC formation in the Arctic is dominated by synoptic-scale forcing. Simmonds and Keay , (2000) and Carrasco et al., (2003) observed a larger number of PSCs in the Antarctic in the downwind regions of intense mesoscale cyclogenesis. Deep tro-

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pospheric clouds are an indicator for the presence of large scale lifting and cyclonic activities (Teitelbaum et al., 2001). Teitelbaum et al. (2001) also reported a linkage between tropospheric synoptic-scale baroclinic waves and PSCs formation. Mountain wave generated PSCs can cover large areas and persist for long time, but they are quasi-stationary relative to the mean stratospheric flow (Cariolle et al., 1989) and therefore cannot explain monthly and interannual spatial variability of PSCs. Their findings suggest that the occurrence of PSCs is highly dependent on tropospheric dynamics rather than mountain generated small-scale waves. This is in agreement with our observations which show that 81.5% of the observed PSCs occurred over tropospheric clouds and thereof 72% over deep-tropospheric clouds. During the winter 2007/2008 deep tropospheric clouds without PSCs occurred only during 28% of CALIPSO overpasses. PSCs investigated in this study were distributed over the entire Arctic (not shown). Most PSCs, were observed by CALIPSO south of Svalbard and in the Barents Sea. This suggests that the orographic influence (mountain generated small-scale waves) is rather small." We also added a corresponding statement to the summary: "Our findings corroborate the study of Teitelbaum et al. (2001)."

Referee: 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?

Response: We already addressed this in the answer to the second specific comment by the reviewer. In the introduction we give a brief review of the current literature. In the discussion we come back to this and conclude that from our observations we don't see a dominating effect of the orographic influence. The reviewer stated himself that orographic effects are relevant over Scandinavia but of smaller influence on the scale of the entire Arctic. Note that the different studies are focused on the entire Arctic and on Scandinavia, respectively, and thus are not in contradiction with each other.

Referee: 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"

Response: We changed phrasing the according to your suggestion.

Referee: 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.

Response: It is the very purpose of Figure 5 to investigate this question. We include cloud tops at all altitudes in order to compare a wide range of cloud top temperatures. Since colder and warmer cloud tops correspond to higher and lower clouds, respectively, this figure gives an impression of the relationship between PSC occurrence and the kind of an underlying cloud system. As the reviewer suggests, the radiative effect of low cloud tops should indeed be very much smaller than the radiative effect of high cloud tops. We do not see such a relationship in Figure 5 and, hence we conclude that radiation is not a dominant mechanism for the linkage between tropospheric and stratospheric clouds.

Referee: 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,

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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?

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

Response: Deep tropospheric clouds indicate the presence of large-scale lifting and cyclonic activities. We added one more reference (Dethloff et al., 2004) to make the connection between changing storm tracks and increased ozone depletion more clear. Recent Antarctic studies from Wang et al. 2008 and Adhikari et al. 2010 suggested that the link between tropospheric and stratospheric clouds is due to a dynamic effect or a radiative effect. The results of our study show that radiative effects do not contribute to the reported linkage between tropospheric and stratospheric clouds. Thus, we contributed to a improved understanding of the link between these two cloud types.

We changed the last papagraph to: ”In a changing climate, storm tracks might get shifted polewards (Dethloff , 2004; Yin , 2005) and can therefore provide favorable conditions for the formation of ice PSCs. Dethloff (2004) reported a decrease of the geopotential height and an enhanced winter polar vortex as an influence of Greenland's deglaciation. Subsequently, ozone depletion and the possibility for an Arctic ozone hole would be increased. Simmonds et al. (2008) investigated the cyclonic behavior in the Arctic region. They conclude that the highest density of cyclones is found between Norway and Svalbard and further eastward in the Barents and Kara Seas. Furthermore, the highest rates of cyclogenesis is south of Svalbard and in the Barents Sea, were CALIPSO observed most PSCs.”

Dethloff K.,Dorn W.,Rinke A.,Fraedrich K.,Junge M., Roeckner E., Gayler V., Cubasch U., and Christensen J. H.: The impact of Greenlands deglaciation on the Arctic circulation, Geophys. Res. Lett., 31, doi:10.1029/2004GL020714, 2004.

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Referee: Technical P.5, L.129: “occured” should be spelled “occurred.”

Response: We corrected the spelling.

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

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