Response to the comments by Referee #3

We greatly appreciate the reviewer for his/her thorough review and constructive comments. We have revised our manuscript as much as possible following his/her comments. Our response to each comment is described as follows:

Response to major comments:

1. The main complaint I have about the paper is how it deals with the question of whether the clouds around 9-11km altitude are tropospheric, stratospheric or something else. This is in the end rather peripheral to the issues that are really explored in the paper, as most of the papers' conclusions can be reached without referencing it, but it is still interesting and worthy of a better treatment. For instance, the authors state at least four times (20008L7, 20010L20, 20013L25, 20021L15) that the typical winter tropopause height is 7-8 km, concluding that the clouds in the 9-11 km range should be considered stratospheric, or at least "tropopausal". However, they do not support their statement about the tropopause height by data or references. The only estimation of tropopause height attempted by the authors is for JJA 2008, when they report a tropopause near 9-10km on average (Sect. 3.2), i.e. higher than 7-8km. Looking into the references provided by the authors, Palm et al. (2005) report for September 29th 2003 a tropopause at ~13km near 71°S, 8°W (based on temperatures from radiosoundings) and ~13.2km at 72°S, 14°W (based on potential vorticity from UKMO meteorological analyses). On September 30th, the same article reports a tropopause at 11.5km near 71°S, 8°W (radiosoundings). These dates are on the verge of the periods considered here (JJAS), but still are considerably above the 7-8 km range. Other references (e.g. Wilcox et al. 2011) suggest that the average south pole tropopause in JJA is at least higher than 8 km. Figures from the Palm et al. paper also show how large tropopause height fluctuations can get along a single CALIOP overpass of the Antarctic. Fig. 5d in the present paper also shows these variations. My point is that comparing average tropopause heights (during a single season) with cloud altitude distributions cannot really tell whether the cloud population in the 9-11km range should be considered tropospheric or stratospheric. To be more convincing in this regard, the authors would need to document cloud fraction as it relates to tropopause height in individual CALIOP profiles (given how the tropopause height can fluctuate above the Antarctic on a given day). If that is not possible, I would suggest to the authors to focus solely on the altitudes of the clouds without trying to address the "troposphere/stratosphere" question in the main body of the article. In that case, this question should certainly not be presented in the abstract as one that is being addressed by or answered in the paper. The question whether these clouds are tropospheric or stratospheric could be raised in the conclusion as one that needs further investigation. Doing this would remove several confusing instances in which the text mentions upper tropospheric clouds being located above the tropopause, which often reads like a contradiction.

First, the tropopause definition (h_b) proposed by Wilcox et al. (2012) is used in the revised manuscript. The average tropopause height is 8.5 km in June through September in the latitude range of 55°S-82°S. The explanation of the tropopause height has been added to the 6th-7th paragraphs of Section 2.

Second, we have newly performed an analysis of the PSCs and UCs based on tropopause-relative altitudes

($z_{TP} = z - h_b$) (Fig. 5). We have found that the PSC/UC frequencies in z_{TP} = 6-13 km (i.e., PSCs) have positive correlation with those in z = 0-2 km in June through August. This point is discussed in the 6th paragraph of Section 3.

20013.6: The authors state that the discontinuity in cloud cover near 20km should be ignored. It is rather hard to do so. Even by masking the data at 20km, one still finds much larger values above 20km than below. How can one reconcile those values? If I'm not mistaken, this discontinuity is due to the change in CALIOP vertical resolution above (180m) and below (60m) 20.2 km, which leads to a better signal-to-noise ratio above 20.2km than below, which leads to an improved ability to detect clouds above 20.2km (assuming cloud properties stay constant throughout the vertical, which they do not). This change in SNR means cloud fractions above and below 20.2 km should not be directly compared, as the instrument's ability to detect clouds is different in the two altitude levels. This fact might very well affect the correlation coefficients reported in Fig. 2 and 3. The right way to address this problem would be to resample CALIOP data below 20.2km to the same resolution as above (180m) before performing cloud detection. If this is not feasible, the authors should at least mention and discuss this problem instead of asking the reader to ignore it.

The discontinuity at 20 km of averaged profiles of PSC/UC frequency is hardly seen in the correlation coefficient as shown in the maps of Figs. 3 and 4. We think that this is because the ability to detect temporal variation of cloud fraction is sufficiently high for the algorithm in the altitude ranges above and below 20 km. In Fig. 6, the composite of the PSC/UC frequencies has been already shown with the significance level greater than 95%. In Section 3.3, the altitude range of the PSC/UC frequency is separated into the two ranges (namely 15-20 km and 20-25 km). We have found that both results are quite similar. Thus we think that the present study pays a sufficient attention to the discontinuity of PSC/UC frequency at 20 km.

Response to minor comments:

20008.4: The abstract and the conclusion both state the study uses satellite lidar observations for five austral winters (2007-2011). However, fig. 2 in Sect. 3.1 only considers JJAS 2008, and fig. 4 in Sect. 3.2 only considers JJA 2008 (fig. 4). Could the authors justify these choices, or extend the period considered in these two figures to cover JJAS 2007-2011 (for consistency)?

We have revised Figs. 3 and 6. Figure 3 includes the time period from June through September in 2007-2011. Figure 6 includes the time period from June through August in 2007-2011. The observations in September are excluded from the analyses as the averaged PSC/UC frequencies in 15-25 km are significantly lower in September than those in the other months (see the right panels of Fig. 4). Both results are qualitatively similar to those in the original manuscript. We have also revised explanations of each figure in the 1st sentence of the 1st paragraph of Section 3.1 and in the 2nd and 5th sentences of the 1st paragraph of Section 3.2.

20011.17: The authors explain that they derived daily occurrence frequency of PSC/UC in 10° longitude intervals *for five latitude bands. It is still unclear to me how this frequency was obtained. Did the authors compute the percentage of CALIOP profiles for a given day in which a PSC/UC was detected (i.e. the cloud fraction)? Or did the authors compute daily vertical profiles of cloud fraction in lon-lat boxes? If that's the case, what is the vertical resolution of these profiles? (I assume the full CALIOP resolution was used, but it should be explicitly stated). What was the horizontal averaging used to produce the CALIOP profiles? What is the daily number of CALIOP profiles in a lon-lat box? Are all profiles in a given box from the same orbit? i.e. document the sampling. Please expand.*

First, the PSC/UC frequency is calculated as the percentage of cloud fraction in each grid box. We have altered the vertical resolution of PSC/UC frequency (i.e., the vertical extent of the grid box) from 180 m to 1 km in the revised manuscript, following the reviewer's suggestion (the second comment of *20013.11*).
Second, the CALIOP profiles are averaged over 5 km horizontally and 180 m vertically before applying cloud detection algorithm. Third, the number of the observations for each grid box is 500 in June and 350 in September on average in 70-75°S. We have added these explanations on PSC/UC frequency to the 6th-10th sentences of the 1st paragraph of Section 2.

20013.11: Following the lack of details in the description of the CALIOP dataset, it is not clear how the two panels of Fig. 2 were created. What is the resolution, is the sampling constant throughout the vertical?

As mentioned above, the vertical resolutions of the PSC/UC frequency and correlation coefficient maps has been changed to 1 km in the revised manuscript.

20013.11: I am very unimpressed by the correlation coefficients presented in Fig. 2 and 3. A correlation coefficient of 0.1 is not symptomatic of a correlation, rather the opposite (later in the paper, the authors present correlation coefficients above 50% that actually do report a correlation). For instance, the authors conclude that coefficient correlations above 0.1 means clouds at 15-25km are correlated with clouds at 9-11km, but correlation coefficients below 0.09 means they are not correlated with clouds below 9 km. A 0.01 difference in correlation coefficient does not seem like enough robust evidence to reach these conclusions. The fact that those coefficients are meaningful at a significance level of 99% (how did the authors obtain that number?) only means that the absence of correlation is robust. However, given how the correlation coefficient evolves with altitude in Fig. 2 and 3 suggests that the link between clouds really does change with altitude. I would suggest to the authors that these low values are due to their using cloud fraction profiles at CALIOP full vertical resolution. This is not needed – investigating correlation coefficient considering cloud fraction profiles at 300 or 600 meters resolution would still be representative of cloud spatial extent while giving much better (read: higher) correlation coefficient results.

We have significantly revised Fig. 3 and 4. In the left panels of Fig. 3, the correlation coefficients of PSC/UC frequency at two different altitudes in the time period of June through September of 2007-2011 are shown. In the left panels of Fig. 4, the correlation coefficients of PSC/UC frequency in each month are shown. We have also revised the explanations of Fig. 3 and 4 in the 2nd-6th paragraphs of Section 3.1 and the figure captions. As mentioned above, the vertical resolution of the correlation coefficient is taken to be 1 km.

20013.11: I do not understand why, if the authors are trying to study the correlation between PSC and tropospheric clouds, they stop at 9 km altitude. In that regard, it would be interesting to see what is going on below. Removing the "troposphere/stratosphere" angle from the introduction would solve this (as suggested in major comment 1).

The altitude ranges in Figs. 3, 4, and 6 have been extended from 9-30 km to 3-30 km. The PSC/UC frequency in the altitude range of 3-9 km is calculated using the CALIPSO lidar Level 2 Vertical Feature Mask (VFM) data. We have found that PSC/UC frequency below 7 km has little correlation with that in 15-25 km (Figs. 3 and 4). We have also added an explanation of the CALIPSO lidar Level 2 VFM data to the 4th paragraph of Section 2.

20013.13: I am not sure how to read "time series for 15-25km are positively correlated with those for 15-25km: : :" I would assume this to be expected and obvious, i.e. along the identity line in Fig. 2 and 3? Please clarify.

As mentioned above, correlation coefficients of the PSC/UC frequencies have been shown in Figs. 3 and 4.

20016.25: Here it looks to me like the paragraph conclusion states the opposite of what it should: right now, the sentence means that "PSCs do not cause anticyclonic PV anomalies". The opening question of the paragraph was "do anticyclonic PV anomalies cause PSCs", not the other way around. The concluding sentence should be rewritten.

We have revised the last sentence of the 5th paragraph of Section 3.2 as follows:
"Thus, the existence of PSCs in the height range of 16-25 km is hardly attributable to the anticyclonic PV anomalies confined to the tropopause."

20017.1: I don't understand how to read a "geographical latitude-pressure section of equivalent latitudes". I hope the other reviewers have more experience with this than I, and I defer to their judgment regarding these results.

To make why equivalent latitude is used (Fig. 8c) clearer, we have added a latitude-pressure section of modified PV (Lait, 1994) (Fig. 8a) and equivalent latitudes as a function of on modified PV on several isentropic surfaces (Fig. 8b). From Fig. 8b, one-to-one correspondence between the modified PV and equivalent latitudes is clear for each isentropic surface. Thus, equivalent latitude is regarded as a normalized potential vorticity (PV) in a specific isentropic surface. Because Ertel's and modified PV are materially conserved in the adiabatic and inviscid flow, the equivalent latitude indicates which latitude the air parcel originally comes from. We have also added an explanation of equivalent latitude and its relation with modified PV in the 7th-8th paragraphs of Section 3.2.

20018.7: The authors define the acronym "AEL" for Average Equivalent Latitudes, but this acronym is only used *once in the rest of the paper, two lines below. I suggest they remove this acronym. Same goes for "CBH", which is used three times in all.*

We have removed the acronyms ‘AEL’, ‘CBH’, and ‘BH’ in the manuscript.

20018.17: This sentence is very unclear. I assume the authors want to express that since BHs cover ~15% of the region between 55 and 82°S, there is a non-negligible possibility they might affect PSC/UC formation. I think this statement could be removed altogether, as it is confusing and this hypothesis is quickly explored in the following paragraphs.

As mentioned by the reviewer, this sentence means that blocking highs cover ~15% of the latitudinal area of 55°S-82°S. This value is important as a reference value. This is because if simultaneous occurrence of PSCs and UCs were independent of blocking highs, the coverage of the blocking high should be the same as the percentage of simultaneous occurrence of PSCs and UCs in asscociation with blocking high. We have revised the last two sentences of the 1st paragraph of Section 3.3.

20018.25: "The simultaneous occurrence of PSCs and UCs is defined as the cases when both PSC/UC frequencies in 9-11km and 15-20km are greater than 10%". Over what time step were these frequencies calculated?

The PSC/UC frequency is calculated in each day, as mentioned in the 9th sentence of the 1st paragraph of Section 2.

20018.27: "about a half of: : :" How much exactly? Please present total numbers considering all years in Table 1.

The value is 44%. We have revised the 5th sentence of the 2nd paragraph of Section 3.3.

20019.1: The last sentence of this paragraph relates poorly to the one before it. The previous sentence states that _ half (45% actually) of cases with SVC+UC happen during BHs (3145 cases during BH vs. 3787 cases with no BHs). This value can't be three times higher than for cases without BHs. Table 1 shows that _11% of no-BH situations lead to PSC+UC and 27% of BH situations lead to PSC+UC (i.e. roughly 3 times more). If that is what the authors meant, they should clarify the sentence.

We have added a new table (Table 3), which shows the probability of simultaneous occurrence of PSCs and UCs in the presence or absence of the blocking highs. We have found that the probability of simultaneous occurrence of PSCs and UCs in the presence of the blocking high is 3 times or more as large as that in the absence of the blocking high. We have also added a discussion using Table 3 to the last part of the 2nd paragraph of Section 3.3.

20020.25: This paragraph contains a lot of repeated ideas from the last paragraph of Sect. 3.3. Please try to state them only once.

We have revised the last paragraph of Section 3.4 as follows:
"The dependence of PSC composition on relative location between the anticyclone center and PSCs as observed in Fig. 107 may be understood in the same manner of the ice seeding-process by gravity waves (Carslaw et al., 1998; Höpfner et al., 2006; Eckermann et al., 2009) but by anticyclones in the present case. Note that this inference is consistent with the study by Larsen et al. (2004) showing that solid particles are strongly affected by the synoptic scale temperature histories rather than local temperatures in Arctic early winter."

Response to technical comments

20008.24: "and lead irreversible removal..." There's a "to" missing.

The expression has been revised following the comment.

20016.1: "Little PSC/UCs are observed around 14km: : :" do the authors mean that clouds at this altitude are small, or that there a few of them? If it's the latter, "few" would be better.

We have revised the 3rd sentence of the 4th paragraph of Section 3.2 as follows:
"Few PSC/UCs are observed around 14 km, which corresponds to the cloud gap between PSC and UC regions as statistically seen in Fig. 3a."

20016.16: What is a "e-folding" vertical extent?
We have revised the 5th sentence of the 5th paragraph of Section 3.2 as follows:
"In this case, the vertical e-folding scale of temperature anomaly induced by the PV anomaly is obtained at the Rossby height (Hoskins et al., 1985)."

20022.8: ": : : for providing the CALIPSO data product" should be removed.

We have revised the 1st sentence in Acknowledgements as follows:
"The CALIPSO data product is provided by NASA Langley Atmospheric Science Data Center."

References:

Lait, L. R.: An alternative form for potential vorticity, J. Atmos. Sci., 51, 1754-1759, 1994.
Wilcox, L. J., Hoskins, B. J., and Shine, K. P.: A global blended tropopause based on ERA data. Part I: Climatology, Quart. J. Roy. Meteorol. Soc., 138, 561-575, doi: 10.1002/qj.951, 2012.