Interactive comment on “Depolarization ratio of Polar Stratospheric Clouds in coastal Antarctica: profiling comparison analysis between a ground-based Micro Pulse Lidar and the space-borne CALIOP” by C. Córdoba-Jabonero et al.

Anonymous Referee #1

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Authors strongly appreciate the helpful comments of the referee #1 with valuable suggestions and useful remarks, patently improving this work.

We will try to answer and explain, in any case, all the questions. In particular, the previous lidar datasets and new data have been processed and re-analysed in order to respond the questions of all the referees. Indeed, negative depolarization values as proposed by the referee #3, within given restriction limits (see response to the referee #3's comments), as well as new data profiles corresponding to higher CALIPSO overpass distances from Belgrano II station than those already considered, as requested by both the referees #1 and #3, have been also processed and included in the comparison analyses between lidar datasets. In particular, several changes (three Figures have been added: the new Figures 2, 3 and 6; old Figures 2 and 3 have been modified; old Figure 4 replaced; old Figure 5 splitted into 4 separated figures with other particular cases selected: the new Figures 8, 9, 10 and 11; Table 2 removed, and Tables 3 and 4 have been modified and joined together in one Table, the new Table 2) have been included in the manuscript as a consequence of responding to all the referees' comments. Then, Sections, Figures and Tables have been renumbered. As a result the text has been accordingly modified to contain these changes, including new required calculations, analyses and results. In general, a revised manuscript containing all the necessary modifications is also available.

This paper compares depolarization ratios measured from a ground-based lidar system located in coastal Antarctica with those measured nearby from the spaceborne lidar CALIOP. This is a carefully devised comparison, that is very well written and structured.
Both lidar systems and datasets are well described, and results of the comparison are clearly presented and analyzed. The choices made for the comparison are all well justified. The article is very easy to follow and makes for actually enjoyable reading, which is no small feat. I have a few minor comments outlined below, that I think the authors should take care of before publication in AMT.

General comments:

GC 1)
My first general comment is the regret that no serious attempt is made to explain the discrepancies between both instruments. 54% of all cases with CC>0.5 means that 46% of cases have CC<0.5. Since the paper concludes that these differences are not due to spatial PSC inhomogeneities, I am very curious about what other explanations could exist. Possible hints could have been gained if the paper included cases with particularly bad CC/BIAS results in its Sect. 3.2.3, instead of solely focusing on cases with good correlation (from which there is effectively less to be learned). However, since the authors intend to focus on this question in a future study, as they explicitly state in the conclusion, and given the subject matters of AMT, I don't think it

reasonable to request that more work in that direction should be included in the present article.

<u>Authors</u>: As stated before, all the lidar datasets have been re-processed by including negative values (within restriction limits), in addition to new data profiles from higher CALIPSO overpass distances than those present in the old version of the manuscript. Therefore, calculations, analyses and results have been modified in order to include all this information. A revised manuscript is available.

GC 2)
Related to this comment is the important finding (in the context of this present comparison) that correlation between ground-based and spaceborne depolarization does not change with distance to the MPL4. As I noted above, the authors conclude from this discovery that measurement differences are not due to PSC inhomogeneities. I understand this as saying that geographical changes in PSC structure and composition do not affect the correlation between instruments, which I find worrying since it implies that observations from both instruments are not affected by changes in PSCs! This would suggest that both instruments are actually not very good at describing PSC variability. In a nutshell, I am concerned that the good correlations are due to properties of PSC being statistically similar in average over large volumes. Following this, it would be very interesting to me to see how the inter-instrument correlation evolves considering CALIOP profiles observed at even higher distances away from the MPL4. I would be relieved to see the correlation decrease at some point. I think the paper would better serve its purpose if it was feasible for the authors to include some results of this kind.

<u>Authors</u>: Regarding this referee's comment (see also response to the referee #3's Minor Comment 8), first let's say that the initial lidar datasets used in this work were based on requirements of any inter-comparison: coincidence in time and space. Therefore, analysis comparison between both lidar datasets was restricted, at least, in time to simultaneous measurements carried out at time scales lower than one or two hours from the closer CALIPSO overpass, and in space to profiles observed at spatial scales lower than 55 km separation from the CALIPSO overpass.

However, in particular, following referee #1's suggestion, new data have been analyzed to examine the influence on spatial scales when data from rather higher CALIPSO overpass distances are included in the lidar depolarization comparison. By examining the new results including distances higher than 55 km (note that a re-analysis of all the data has been performed including also negative depolarization values within given restriction limits, as proposed by the referee #3, see response to his/her General Comment 1), no large dissimilarities in the conclusions are found after the new re-analysis. However, in order to clarify this point with the new results, the following sentences have been added in the Conclusions:

"Moreover, the degree of agreement between both lidar δ^V datasets is moderately dependent on the CALIPSO ground-track overpass distance from the Belgrano II station, as shown by the results obtained in each one of the comparison analyses carried out: the vertical correlation (CC), and both the mean (Δ) and percentage ($BIAS$) differences. That is, no large discrepancies are found when CALIPSO ground-track distance is as close as < 10 km far as well as rather far (at 70-90 km long) from the Belgrano II station.

Actually, these results indicate that MPL-4 depolarization observations would reflect relatively well the PSC field that CALIOP can detect at large distances from the ground-based station. As a consequence, PSC properties would be statistically similar in average over large volumes, and hence the present disagreement found between both the lidar δ^V datasets would be likely related to be dominated by small spatial PSC inhomogeneities along the CALIPSO separation from the station. This statement is based on the fact that Belgrano II is a station located well inside the polar vortex (for instance, see Figure 1) during almost all the wintertime period (Parrondo et al., 2007). Indeed, the Antarctic polar vortex is quite stable to allow determined thermodynamic conditions leading to a very low variability of the PSC field, and then of their properties."

GC 3)
Finally, I was surprised to see that the paper totally bypasses the question of PSC-Ib made of Supercooled Ternary Solution (STS) droplets, as if those do not exist. I understand that as these droplets do not depolarize, they arguably fall outside of the scope of the paper, which is concerned about the comparison of non-zero depolarization ratios. However, STS PSC are the most frequent composition class early and late in the Antarctic PSC season (according to Pitts et al. 2009), and can't be totally ignored like the paper currently does. I think the introduction should at least include them in its description of PSC types, and explain why the authors made the choice of not considering them in the study.

Authors: *It is partially true that the paper bypasses the question of PSC-Ib made of Supercooled Ternary Solution (STS) droplets, as if those do not exist. And authors completely agree with the referee that PSC-STS can't be totally ignored, as they represent the most frequent composition class early and late in the Antarctic PSC season (e.g., Pitts et al., 2009). But let's expose a few comments on this, next.*

The scope of this work is not the PSC-type discrimination (this will be done in the future), but the performance of the depolarization measurement capabilities of such a kind of ground-based Micro Pulse Lidars (MPL) for PSC observations. Previously, a comparison against a ground-based lidar devoted to long-term PSC measurements was already performed in the Arctic with the same lidar system (Córdoba-Jabonero et al., 2009), but without a final conclusion on such MPL depolarization capabilities. Therefore, a new MPL depolarization inter-comparison with a reliable lidar located not far from the Belgrano II station was necessary. At polar latitudes such a lidar couldn't be other than the space-borne CALIOP. Therefore, the expecting inter-comparison between both lidar datasets has been presented and analyzed in this work. Authors didn't want to deeply address the PSC classification in this paper, also because backscattering ratio (R) data are not shown regarding the scope of the paper as said before. In fact, a future work will be performed following the current one and addressing this question. However, it is true that more information should be provided on PSCs, at least, in the abstract and introduction. For instance, in the reference provided by the referee #3 (Lowe and MacKenzie, 2008), which has been also included in the manuscript's reference list, a more extensive information can be found on this subject.

In this sense, authors have rewritten both the abstract and introduction including more information on PSC classification, their types and compositions, and their corresponding role in ozone depletion. In particular, both PSC-I (subtype Ia *corresponding to solid particle clouds and subtype Ib to liquid clouds) and PSC-II (ice clouds), as well as mixtures of both of them, have been commented.*

Specific comments:

SC 1)

- Abstract: The second sentence of the abstract is uncharacteristically confusing. It states that "In particular ice clouds, type PSC-II, with respect to the type PSC-I (nitric acid clouds) produce the most significant effects". What does "with respect to the type PSC-I" mean? Does it mean "compared to the type PSC-I"?

SC 2)

- Abstract: In the same sentence, on what exactly do PSC-IIs have the most significant effects? (For instance, this is not true for denitrification...)

<u>Authors</u>*: SC1 and SC2 comments have been addressed together. Indeed, this sentence was confusing. As known, PSCs provide reactive surfaces for the release of active chlorine compounds, which are directly involved in the ozone destruction. In this sense, authors mean that when ice PSCs (PSC-II) do form they can provide aerosol surface areas 100 times greater than those of liquid STS (type Ib) or NAT (type Ia) PSCs (Lowe and MacKenzie, 2008, and references therein), thus favouring an enhancement of the ozone depletion as compared to that of those PSC-I clouds. However, it is true that the occurrence of PSC-II is lower than that for the PSC-I (e.g., Adriani et al., 2004; Maturilli et al., 2005; Pitts et al. 2009), and it must be taken into account. Therefore a more relevant role on ozone depletion is actually linked to liquid STS and NAT PSCs, which are the most important PSCs for chlorine activation and denitrification (one more process involved in ozone reduction), respectively.*

Authors have rewritten both the abstract and introduction in order to avoid this confusing sentence, introducing the information exposed before. The corresponding paragraph in the Abstract has been reworded as follows:

"Polar Stratospheric Clouds (PSCs) play an important role in polar ozone depletion, since they are involved in diverse ozone destruction processes (chlorine activation, denitrification). The degree of that ozone reduction is depending on the type of PSCs, and hence on their occurrence. Therefore PSC characterization, mainly focused on PSC-type discrimination, is widely demanded."

SC 3)

- Sect. 1: The introduction states that "PSC are classified in two groups depending on the temperature formation threshold". In my opinion, the PSC classification is more likely based on the cloud composition (NAT, ice, etc.) and its consequences for the cloud optical properties. It's true that this composition depends on the cloud formation temperature, but not only.

<u>Authors</u>*: As in the SC 1 and 2, this sentence has been also modified in order to reflect a more clear explanation. It has been reworded as follows:*

"PSCs are classified in three main groups depending on their composition, and then on their temperature formation threshold (i.e., see the review on PSC microphysics and chemistry by Lowe and MacKenzie, 2008, and references therein): type Ia (PSC-Ia) are nitric acid trihydrate (NAT) clouds formed above the frost point (T_{NAT}=194 K at 30 hPa), type Ib (PSC-Ib) are super-cooled ternary (H_2SO_4, HNO_3, H_2O) solution (STS, liquid particles) clouds, and type II (PSC-II) are water ice clouds (T_{ice}=185 K at 30 hPa)."

<u>Reference</u>:

Lowe, D., and MacKenzie, A. R.: Polar stratospheric cloud microphysics and chemistry, J. Atmos. Solar-Terr. Phys., 70, 13-40, 2008.

SC 4)

- Sect. 1: The last two sentences of the Introduction first paragraph (8054, lines 18-22) basically make the same point (Antarctic winter stratosphere is colder than the Arctic one, hence PSC are more frequent there). Please find a way to combine those two sentences into one.

<u>Authors</u>: *These sentences have been combined, and the corresponding paragraph has been reworded as follows:*

"Arctic temperatures are close to the threshold of PSC formation, hence both spatial and temporal PSC distributions present a high variability at daily and yearly scales. In contrast, PSC presence in the Antarctica is almost ubiquitous from the beginning of wintertime to early springtime, since Antarctic temperatures can reach rather lower values than those present in the Arctic (Parrondo et al., 2007), leading to a higher occurrence of PSCs over the Antarctic continent."

SC 5)

- Sect. 1: The introduction makes the assumption that all PSCs depolarize (8054, lines 23-25). This assumption is true as long as STS-based PSCs are ignored, which I don't think is correct (cf. second major comment). Please rewrite this section once the introduction mentions the existence of Type Ib PSCs.

<u>Authors</u>: *That is not completely like that. What it is written in this sentence is: "Due to the fact that non-spherical particles change the polarization state of the incident light, unlike spherical particles, PSCs can be detected and identified by using lidar systems with depolarization measurement capabilities." Authors mean that the 'absence' of depolarization (i.e., depolarisations close to zero or lower than the molecular value) also provide information about the type of PSC (together with the backscattering ratio obtained by lidar measurements), including the liquid STS PSCs (PSC-Ib).*

In any case, authors consider indeed this paragraph was ambiguously written in the previous version of the manuscript, and it has been reworded as follows:

"*Lidar measurements have been widely used for PSC classification on the basis of two lidar variables: the backscattering ratio (total backscatter-to-molecular coefficient ratio, R) and the volume linear depolarization ratio (δ^V). Indeed, due to the fact that non-spherical particles change the polarization state of the incident light, unlike* *spherical particles, both PSC-I (subtype Ia corresponding to solid particle NAT clouds and subtype Ib to liquid STS clouds) and PSC-II (ice clouds), as well as their mixtures, can be detected and identified by using lidar systems with depolarization measurement capabilities".*

SC 6)

- Sect. 2.1.1: The authors state that the MPL4 system is able to probe the atmosphere up to 30 km with a sufficient SNR. Is this true for individual 1-minute profiles, or for hourly-averaged profiles? This should be mentioned.

Authors*: This has been mentioned in the text. The corresponding paragraph has been modified and reworded as follows:*

"These 1-min signals registered in alternative mode for each p- and s-channel are hourly-averaged, providing 30-min averaged p- and s-signal profiles in one hour. These hourly-averaged profiles are usually analyzed to study the spatial and temporal variability of the PSC distribution. This MPL-4 observational configuration allows for probing the atmosphere up to 30 km with a sufficient signal-to-noise ratio (SNR)."

In addition, in order to provide more information, the following text has been added within the Sect. 2.2.1, where an analysis of the 5-min, 10-min and 15-min averaged MPL-4 profiles has been included (see also the response to the referee #1's Specific Comment 9 and the referee #3's Minor Comment 4):

"*Regarding time averaging procedures applied to the MPL-4 measurements, hourly-averaged MPL-4 δ^V profiles, as obtained from those 30-min averaged p- ($P^{\parallel}(z)$) and s- signal ($P^{\perp}(z)$) profiles in one hour (see Sect. 2.1.1 for details), are those used in the comparison with CALIOP data instead of instantaneous 1-min profiles ($\delta^{V}{}_{1-\min}$). As aforementioned (see Sect. 2.1.1), this improves the SNR of the lidar measurements at Belgrano II station. Indeed, the level of noise decreases as the time averaging increases, as shown in Figure 2 (for instance, data on 1 July 2009). δ^V variations depending on the time averaging (5-, 10-, 15- and 30-min averaged profiles are shown in Figure 2) reveal that the vertical δ^V structure presents a clearly enhanced SNR when the time averaging is higher than 15 min. In particular, an additional PSC feature at around 25 km height can be identified with enough SNR only for 15-min and 30-min averaging of the MPL-4 data (see Figure 2).*

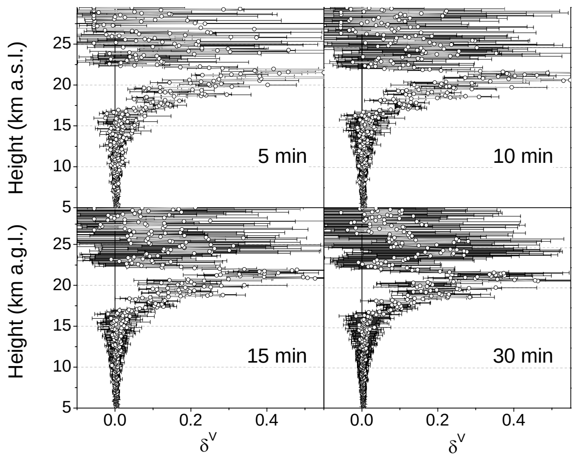

Figure 2. MPL-4 δ^V profiles (grey-lined open circles, in black background of their SD values) on 1 July 2009 depending on the time averaging (see legend inside each panel, from up to down and right to left): 5-, 10-, 15-min and 30-m averaged profiles.

Moreover, MPL-4 δ^V fluctuations along that hour are also studied by examining the differences between instantaneous 1-min ($\delta^V{}_{1-\min}$) and hourly-averaged (δ^V) profiles within the same hour. Mean differences and their RMS values are shown in Figure 3 (for instance, data on 1 July 2009). A height-averaged value of -0.005 ± 0.013 is obtained for these mean differences, and their RMS values show that temporal δ^V fluctuations are lower than 0.05, 0.1 and 0.25 up to altitudes of 18 km, 23 km and 30 km height, respectively.

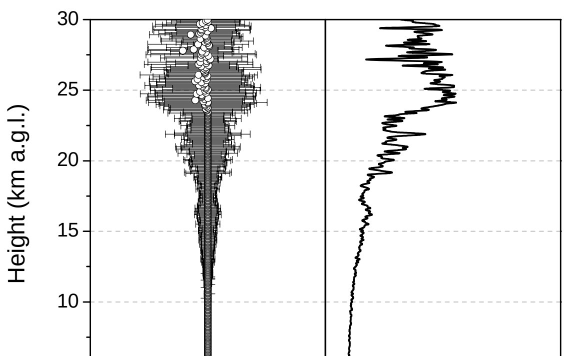

Figure 3. MPL-4 mean ($\delta^V{}_{1-\min} - \delta^V$) differences along an hour and their RMS values (for instance, data on 1 July 2009).

In summary, a high SNR is achieved for a time averaging of 30 minutes applied to MPL-4 p- and s- signal profiles (i.e., hourly-averaged δ^V profiles) in our comparison analysis, and temporal δ^V fluctuations per hour are lower than 0.1 up to altitudes where PSC features more frequently appear."

SC 7)

- Sect. 2.1.2: CALIOP is technically not the first space-borne lidar instrument: LITE and GLAS predate it.

Authors: Completely in agreement with this comment. Then, the manuscript has been modified accordingly.

SC 8)

- Sect. 2.2.1: I gather that TPSC in Figure 2 was obtained by computing the temperature for which a water vapor of 5 ppmv reaches its saturation pressure. Did the radiosounding used in this section contain measurements of water vapor? Using the actual water vapor concentration profile would lead to a more robust computation of TPSC.

Authors: The old Figure 2 has been renumbered by the new Figure 4. Calculations have been performed to show the temperature threshold for PSC-I and PSC-II formation by using the radiosounding water vapour profile. T^{NAT} (T^{PSC-I} in the new Fig. 4) was calculated according to the parameterisations of Hanson and Mauersberger (1988) assuming 10 ppbv of nitric acid and using in situ water vapor concentration (radiosounding data); T^{ice} (T^{PSC-II} in the new Fig. 4) was estimated from Marti and Mauersberger (1993) using in situ water vapor concentration (radiosounding data). The corresponding new Figure 4 (old Figure 2) has been accordingly modified, adding this additional information, and the following text has been included:

"*These threshold temperatures for PSC-I (T^{PSC-I}, dashed line in Figure 4) formation have been calculated according to the parameterisations of Hanson and Mauersberger (1988) assuming 5-ppmv H_2O and 10-ppbv HNO_3; and for PSC-II (T^{PSC-II}, solid line in Figure 4) they were estimated from Marti and Mauersberger (1993) with the same H_2O amount. These proportions were obtained from Maturilli et al. (2005), where these values were reported as typical for this month.*

However, T^{PSC-I} and T^{PSC-II} can vary depending on the real amounts of HNO_3 and H_2O. Therefore, additionally, as the local radiosounding water vapour profile is available, the same calculations have been performed by using the in-situ H_2O concentration (radiosonde data) instead of a constant value. These new threshold temperatures for PSC-I and PSC-II formation are also shown in Figure 4 (thick dashed and solid lines, respectively), and differences are clearly found respect to those previously obtained. In particular, no PSC-II region, i.e., where $T < T^{PSC-II}$, would be observed, differing the lidar results found as indicated by both the R and δ^V parameters. These discrepancies can be likely due to the no simultaneous lidar measurements with local available radiosounding data, at least for this day. Hence, a further study must be addressed on the relation between the PSC-type features and the stratospheric temperature variability, regarding the temperature thresholds for PSC formation. Nevertheless, this is out of the scope of this work."

SC 9)

- Sect. 3: The authors use hourly-averaged MPL4 profiles for their comparison with CALIOP. I have two questions regarding this:

1) were the hourly averaged depolarization profiles obtained by averaging 1-minute depolarization profiles, or by computing the ratio of hourly averaged perpendicular and parallel backscatter profiles? This has strong consequences for final averaged profiles, as it tends to decrease the frequency of extreme depolarization ratios, and should be mentioned.

Authors: Hourly-averaged depolarization profiles were obtained by computing the ratio of hourly-averaged perpendicular s- and parallel p-signal channels. See the response to the previous Specific Comment 6.

2) Do the authors have any insight into the MPL4 depolarization variability below the hour time scale? This variability could influence the discrepancies between MPL4 and CALIOP observations.

Authors: First, we would like clarify that the hourly-averaging performed to the MPL-4 δ^V *profiles is based on a SNR enhancement for these lidar measurements, mainly at altitudes > 20 km height, where the noise considerably increases. Temporal fluctuations of these* δ^V *values during that hour are considered included within the error analysis for that parameter. However, this issue has already been addressed in the response to the referee #1's Specific Comment 6. The manuscript has been modified accordingly in order to include this kind of information (new Figures 2 and 3 have been added in the revised version), thus providing a more complete clarification. Evidently, all these modifications will improve the manuscript.*