

## ***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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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,

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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**

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.

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.

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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.

### Specific comments

- 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"?
- Abstract: In the same sentence, on what exactly do PSC-IIs have the most significant effects? (For instance, this is not true for denitrification...)
- 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.
- 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.
- Sect. 1: The introduction makes the assumption that all PSCs depolarize (8054,

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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.

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

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