Referee comments: amt-2017-76

Depolarization measurements using the CANDAC Rayleigh-Mie-Raman Lidar at Eureka, Canada

This work presents in detail a particular methodology to add a depolarization channel to a lidar without change any of the previous optic configuration and apply the Müller matrix theory to track the polarizing effect of the lidar optics. Despite there are many studies which shows the 'best practices' to measure lidar depolarization —in fact, Section 9.2 shows how the hardware could be improved—, I understand that it is not always possible to make the best setup but the optimal one according to the circumstances. Therefore, I consider interesting how the authors face the complexity of the setup and the methodology can help to scientific community to face similar experimental setups. Considering the scopes of the journal, I recommend this paper for publication but the authors should strongly consider the following comments.

I would like to suggest a change of the title of the manuscript. By now, the title highlights the 'depolarization measurements' as a key goal of the manuscript but at the end, the depolarization measurements are only present in Section 8 and not discussed from a scientific point of view. Since the main result of the paper is the depolarization calibration, I would recommend a title fitting the content of the paper and an improved introduction fitting the 'real scope'.

The lidar depolarization technique, and specially the depolarization calibration, is an important research field. However, reading the introduction, I have the feeling that this is the first time that the depolarization is calibrated. I strongly recommend a brief state of the art about the calibration procedures and studies (Alvarez et al., 1999; Snels et al. 2009; Freudenthaler et al., 2010; Bravo-Aranda et al., 2013; and references there in), highlighting why these methods are not applicable to this lidar or how an existing calibration method was adapted. Additionally, authors should include other references of interest such as the general theory based on Stokes-Müller formalism recently presented by Freudenthaler, 2016. In other words, the introduction is too straightforward to me since it is not consider the state of the art of the lidar depolarization technique up to date.

Regarding the structure of the manuscript, I suggest to move the paragraph P2L6-P2L16 to a section called 'Site and lidar description'. Since this work is part of the thesis of the first author, I'll take the opportunity to 'remind' that the introduction should gather the state of the art and the explanations about why the work is necessary and useful.

Bravo-Aranda et al, 2016 quantifies the systematic errors on the depolarization measurements from the non-calibrated parts of the lidar but the polarization effect of the Newtonian telescopes with 90° fold mirrors is not evaluated. The calibration values presented in this study demonstrates that the combination 'window roof + Newtonian telescopes' strongly affect the depolarization measurements (Table 1, ki/(ki-1) = 3.12) and thus, it should be highlighted as an interesting result of this paper.

Minor comments:

P2L12: 1064 nm is not available?

P3L11: The use of a single PMT for the parallel and perpendicular measurement is presented as an advantage. However, in Section 9.2, *'using two depolarization PMTs would allow for different gain settings individually optimized for the parallel and perpendicular channels'* is presented as a hardware change that would improve the perpendicular signal. May the authors clarify which is the best configuration?

P3L23: Does licel report the temporal stability of this device?

P5L1: ‘atop’ -> typo?

P5L15: I suggest to include a comment about the different definitions (Cairo, 1999). The concept of ‘photons polarized perpendicular’ is ‘old-fashion’ and has already demonstrated wrong. Please, revise the paper considering the explanation of Gimmestad 2008.

P17L21: I understand that authors use the depolarization sheet to isolate the polarizing effect since *there is no alternative* but, in any case, I would appreciate the technical specifications of the depolarization sheet (glassine). From the phrase ‘To keep the photon count rates as high as possible during the test, only a single layer of glassine was used, although using two sheets in series ensures more complete depolarization’ (P18L26), I would say that depolarizer is not perfect ($d \neq 1$). Was the depolarization degree of the sheet measured? Any information in this regard would be great for the scientific community (accurate measurements of the polarizing characteristics of the *depolarization sheet* are not so common). Did authors consider use the equations to find the effect of an almost perfect *depolarization sheet?*

P22L15-P23L0: I agree with the authors that the effect of the multiple scattering on depolarization measurements has to be evaluated but is out of the scope of this work. Nevertheless, some references on this regard will be appreciated by the readers.

P25L23: ‘half of the backscattered light reaching the roof window is parallel…’ Parallel to what? This way to understand the depolarization is dangerous. The polarization state of the photons is not binary (parallel/perpendicular). The parallel and perpendicular signal *with respect to the polarizing component of the emitted laser beam* is the way we measured the received light. Please, revise the whole manuscript.