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**Interactive
comment**

Interactive comment on "PBL height estimation based on lidar depolarisation measurements (POLARIS)" *by* Juan Antonio Bravo-Aranda et al.

Anonymous Referee #2

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I have not read the review of anonymous reviewer #1 in detail to avoid any "influence" from his/her criticism, but I fully agree that the quality of the presentation is unacceptable. It seems that none of the 10(!) authors was willing to provide a text fulfilling minimum formal standards of a manuscript. This is downright annoying and disrespectful to their readership. From this point of view I would like to reject the paper.

On the other hand the consideration of lidar depolarization measurements can indeed be a promising approach to improve the retrieval of the mixing layer height. In their paper Bravo-Aranda et al. propose an extension (POLARIS) of an existing method based on the analysis of the range corrected lidar signal. The new approach is compared to the previous one (and it is found that it performs better), and to an independent retrieval utilizing data from a microwave radiometer (and it is found that it does not perform well, cf. doi:10.5194/amt-7-3685-2014). Finally, the mixing layer heights (MLH) derived from

measurements (lidar, MWR) are used to validate WRF-simulations.

The benefit of such an investigation certainly could be high, especially in view of future applications for ceilometers with a depolarization channel, in particular as these instruments are expected to have a lower overlap, can run unattendedly and continuously, and might be available as networks (doi:10.5194/amt-7-1979-2014). This is in my view the only argument not to reject the paper.

The present version of Bravo-Aranda's paper, however, suffers from several shortcomings. First, the small set of measurements is problematic. As a consequence, it cannot be demonstrated in a convincing way that POLARIS is useful when long time series shall be evaluated. Even for the short period of only 3 days (according to doi:10.5194/acp-16-455-2016, the SOP I of ChArMEx 2013 took place from 11. June to 5. July: what about this data-set?) a lot of situations were found when the different methodologies disagree. It is also not clear whether the basic assumption (if I understand it correctly, unfortunately it is not sufficiently explained) "a depolarizing aerosol layer is a transported desert dust layer and does not belong to the mixing layer" can be applied to other sites than Granada (or other Mediterranean countries). So this paper (provided the text has been undergone a substantial revision in grammar and spelling, and has been improved in terms of scientific clarity) can only be considered as a first contribution to a discussion of the benefit of adding depolarization-information to a MLH-retrieval.

I will not list the countless typos, word repetitions, cases of wrong grammar, misuse of capital letters, misspelled units, or undefined symbols. Even one of the affiliations is not correct and the link to the ChArMEx-Website does not work! Only a few specific mostly science-related issues are listed below.

In summary I think the authors should take all comments seriously. If not all issues are fully resolved I will not recommend the publication of a revised version.

Specific comments

- page 2, lines 14ff: There is an extensive discussion on different regimes as the residual layer, the mixing layer, the convective boundary layer and more. In the rest of the paper primarily the "PBL" is mentioned and discussed (see 3/5). A strict terminology is required throughout the paper. When there is a co-existence of the residual layer and the convective layer (e.g. after sunrise) PBL might be confusing.

- 2/23: "*Sunrise and Sunset are characterized by the complexity of the PBL.*": This sentence is really strange!

- 3/18: "*... are feasible and reliable ...*": What is meant with "reliable"? In the paper many example are shown when this is not the case.

- 3/21: "*... include stringent conditions ...*": What is this?

- 4/8: There are a lot of words on the overlap, but the most relevant number, i.e. the minimum range that can be exploited in terms of MLH, is missing. Why is the 90% overlap-range given as a interval? Is it temperature dependent? If it depends on the channel (i.e., wavelength) but only one wavelength is used in this study, it is not adequate to give a range.

  It is strange that the polarization channels which are the most relevant in view of the novelty of POLARIS are not mentioned here – whereas the irrelevant Raman-channels and water vapor channels are mentioned.

- 4/28: The vertical resolution given here does not agree with the statement in line 21.

- 5/24: "*both ... are normalized respectively to the maximum value of RCS and δ in the first kilometer above the surface*". In case of δ no normalization can be seen in any of the corresponding figures. Please clarify.

- section 3: It is not common to use the character "C" for a height.

- 6/4: Fig. 6 is discussed prior to Figs. 4 and 5. Fig. 3 is missing!

- 6/5: What is the reason for selecting RCS at 532 nm?

  Furthermore, "height above mean sea level" should be transformed to "height above ground" (throughout the paper).

- 6/8: "*We do not expect the ...*": Does this mean that an automated POLARIS retrieval is not possible?

- 6/15ff: The following discussion is confusing. A few examples: Under "b.1" it is stated that $C_{CRS} = C_{max}$ (by the way another typo: should be C_{RCS}) whereas in the next line of text the authors describe that $C_{CRS} \neq C_{max}$! It is not clear, what the "lowest layer" is (line 22). It is doubtful that at the top of a lofted layer RCS increases (7/2), the opposite should be the case. It is not clear why there is an increase of δ "before C_{max}" (7/7). It should be clearly outlined what should be understood by "coupled", it seems that it is used in different ways.

  It is difficult to understand a situation when $C_{min} > C_{max}$, whereas the opposite can be identified as e.g. a lofted dust layer.

- In Fig. 5 the differences of the profiles in cases D/E or F/G are hardly visible. Nevertheless the retrieval results in quite different z_{PBL}. This seems to be a weakness of the method and should be discussed in detail. Moreover, case I seems to be critical. The "inhomogeneities" in the shape of the δ-profiles are not much pronounced so it seems questionable if depolarization should be exploited at all, especially when considering measurement errors (error bars are missing in all figures!).

  The labels of the axes and the legend are hardly readable.

- Section 3.3: A discussion of how the different thresholds are found is missing. There are only statements on specific numbers. The rest of the text does not

really fit to the title of the section; it is rather a discussion of the differences of the old and new method.

- 8/16: "C_{RCS} *indicates layeringpoints to a weak edge within the PBL.*" Another example of a "weird" sentence.

- 8/21: Why do the authors switch to "m" instead of "km" as in the rest of the text?

- Section 4: Validation is performed by means of the MWR-retrieval. This implies that the latter is assumed to be the truth (see analysis in doi:10.5194/amt-7-3685-2014). As a consequence the MWR-retrieval and its accuracy has to be explained in more detail.

  In Section 4 the authors demonstrate that there are a lot of differences. Thus, the reader might conclude that the POLARIS-retrieval does not work reliably (in my view the grey and black stars never coincide in Fig. 7).

  In Fig. 7 it is not explicitly explained which parameter is shown in the upper/lower panel.

- 9/24: There are no red triangles in Fig. 9!

- Section 5: Obviously there are very few cases when POLARIS-retrievals agree with the WRF-simulations. What is the conclusion with respect to the usefulness of POLARIS or the accuracy of WRF?