

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

Anonymous Referee #1

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Review: The Paper by Bravo-Aranda et al. presents an advanced methodology based on existing work to estimate the height of the PBL top by aerosol lidar. As a novelty, the authors use the profile of the depolarization profile to better identify the mixing layer under conditions of complex aerosol layering. The paper could be suited for ACP after some revisions.

General comments:

-First of all, I am a bit angry because within the manuscript there are a lot of minor spelling mistakes which easily could have been found if a spelling checker would have been used or a co-author would have read the final version. Thus the authors should carefully read their manuscript again and remove these mistakes – I will not mark them separately. E.g., many “spaces” are missing and very often two dots at the end of a

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sentence are found.

-Secondly, I think the statement made for the improvement of the PBL height is too strong. The methodology sounds very interesting, but is in my opinion limited to situations with Saharan dust intrusions as shown in the 2 case studies of Charmex. Therefore, general statements should be avoided. Instead it should be written that for the meteorological conditions like in Granada this methodology could be a significant improvement. As no long-term data set (>12 months is presented), one can only speculate that the new methodology is a significant improvement. Therefore some statements should be weakened. E.g: the statement in the introduction: P3,13: "POLARIS improves the zPBL detection since the computation of . . ." is only valid, IF different aerosol with different depolarization characteristics are existing within and above the PBL. This is certainly not true for many sites not influenced by dust. Or: P5: line 19:" . . .able to detect the PBL height even when advected aerosol layers in the free troposphere are coupled to the PBL." Only if the advected aerosol layer show a different depolarization ratio.

-Also the quality of Figures 6 and 7 must be improved. With the current state the discussion is hard to follow. Symbols should be revised for better readability, time scale should have more tick marks, height scale should be probably revised. Often symbols lay very close to each other and have similar color. Reducing symbol frequency could be also an option to consider. Please do definitely choose a different symbol for the PBL top height with POLARIS. The star is not visible and all other symbols are much more prominent.

-Even so referred to a previous paper I miss a real discussion on the MWR PBL height trustworthiness. In this paper it is always taken as the truth and one wonders why to use the lidar at all. . .

- An lidar instrument discussion is needed: How trustworthy are profiles close to the lidar (overlap issue but also polarization properties).

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- Title: I think the title is not representative for the paper as it not only deals with POLARIS. Probably, the model comparison approaches and MWR should be accounted for in the title.

-Section 5 should be shortened: Please only show up with new information an restrict to WRF. Almost no new information with respect to the other already published results of R. Banks are given. Probably use WRF to check consistency of retrievals (begin of convection etc.,)

Specific comments (no spelling mistakes):

Page 5, line 11. Unit missing after 0.05 and later in the manuscript. Or what do you mean with dilation parameter?

Page 5, line 21: What do you mean: can be applied to lidars not fully characterized? I think this is a very “dangerous” statement

Page 6: line 12: how can you assure, that this increase is not due to instrumental effects? The ratio of two signals very close to the lidar might not cancel out all instrumental effects anymore, especially regarding depolarization. Do you have a case study where it is seen that the depolarization ratio is constant throughout the atmosphere? I always see an increase towards ground below 1 km.

Page 6, line: I did not understand what you mean with lowest layer, please rephrase: “Then, we define two layers: from the full-overlap height up to the lowest candidate, and from the lowest layer up to the highest candidate.”

Page 6, line 26: I do not know what you mean with aerosol stratification in this context, can you explain better?

Page 7, line 4 to 6. Are these thresholds really without dimension?

Page 8, line 11: An offset of 300 m is not too small, how reliable are the MWR measurements during night time? And how reliable is your lowest candidate, as it is very

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close to the lidar. I wonder if the depolarization measurements are reliable (see comment above), please discuss this. Especially as you state that Polaris improves the detection. I cannot follow this discussion, as I do not know the “truth”.

Page 8, line 22. Between 11:20 and 11:30 UTC on which day? Again it is hard to follow, because the symbols are so tiny and especially the Polaris an (star) is almost invisible.

Page 8: Discrepancies between MWR and Polaris on 16 June afternoon should be more intensively discussed. I see a systematic bias of almost 1 km.

Page 9, line 22ff: Does the MWR really watch the same atmospheric column or is scanning included? Then the discussion could be different. Furthermore, also the overlap issue and the near-range depolarization issues could play a role.

Page 11 line 31: These are certainly no long-term measurements, use better continuous measurements

Page 12, line 7ff: POLARIS allows better model validation compared to what?

Fig. 5 please move legend to case B and enlarge - it is not readable even at 150% zoom. Sometimes the blue star changes the color? If the star is overlaid with a dot please make it visible.

Fig. 6 and 7. See above: Poor quality, work on improvement, otherwise discussion cannot be followed. Explanation for WRF missing in caption.

Fig. 8: pink dot is somewhere else in legend.

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