

New insights into the vertical structure of the September 2015 dust storm employing 8 ceilometers over Israel (revised version)

by Leenes Uzan et al.

The authors in fact spent a lot of efforts and improved the manuscript significantly. Due to the large number of changes it is almost impossible to check point by point if and how each individual comment of the reviewers was treated in the revised version (pages 39 ff of `acp-2017-634-author_response-version2.pdf`). As a consequence I focus on how convincingly the replies to the reviewers' comments are (taking into account, that the views of both reviewers have been similar but not identical), and on the revised version as a whole (`acp-2017-634-manuscript-version3.pdf`, "V3").

As a result, a few points remain where I suggest a second iteration step to prepare a final version of the paper suitable for publication. I don't comment on typos etc. – this could be left to the typesetting.

1. Abstract: From the title one would expect that ceilometers provide the main contribution to the study. This is not reflected in the abstract anymore. Please highlight the role of ceilometers more clearly.
2. Introduction: In general the authors followed the recommendations of the reviewers. However: check the "Stavros/Solomos"-problem (throughout the text; Stavros is the first name, so use Solomos!). Include a short section on the radiative forcing because this has now been covered in the results-section (as mentioned by reviewer #2 there are several papers in Tellus B special issues of 2009 and 2011).

In my view the introduction is now sort of long. Certainly, the extension was triggered by trying to fulfill the requirements of review #2. Anyway, check if there is potential to slightly shorten it.

3. Ceilometer section: Include the statement of the limited measurement range more clearly – this was the major criticism of reviewer #2 (and also raised by me) – after (e.g.) line 286. This is indeed mandatory, and is independent on the problems discussed before (overlap, cosmetic shift, etc.): it is an effect of signal attenuation due to reduced atmospheric transmission and happens to all lidar systems (think about a dense cumulus cloud for example – the same effect). Thus, add 1-2 sentences here and refer to them later in the manuscript whenever necessary.

Line 246: Note that the statement in the Vaisala user's guide on the output does not fulfil strict scientific standards: "two-way attenuated backscatter profile with sensitivity normalized units $(100000 \text{ srad km})^{-1}$ " is not a physical quantity. These numbers are not the correct definition of attenuated backscatter (see β^* in one of the cited Wiegner-papers and the explanations of reviewer #2) as it requires a calibration. Just state in the paper, that the range corrected signal (in arbitrary units) is stored (even this is not necessarily true if the h2-parameter is not set – this is fortunately not relevant for the lowest 2.4 km) and replace all cases of "attenuated backscatter". By the way: the authors correctly mentioned in line 264 that the "real" attenuated backscatter cannot be derived. Final comment (to lines 265ff, "Nevertheless..."): what is the purpose of these sentences: Rayleigh calibration is not possible, or Rayleigh requires averaging over 4 hours? Cloud calibration (see O'Connor et al., 2004) should be used? What is really meant with "background correction"?

Please reconsider my suggestions of the use of the (already listed) citations; they were not properly included. Ansmann et al. (2011) and Papayannis et al. (2008) are not covering ceilometers (but the benefit of lidars for dust observations in general), so these citations do not fit to lines 230–232 in V3. The statement on the water vapour absorption should be more precise (lines 276–278 of V3), maybe something like: "... water vapour distribution has a small effect on the pronounced change of the signal shape at the top of the mixed layer or at boundaries of an elevated aerosol layer (Wiegner and Gasteiger, 2015)". The citation of Mona et al. (2012) is missing in the text.

4. Results section: In context of Fig. 17 the authors refer to "attenuated backscatter". Either this should be changed to something like "range corrected signal with the Vaisala's inherent normalization" or better just "range corrected signal (in arbitrary units)". If the ceilometer's sensitivity remains constant during the event (which is likely) and the effect of changing water vapor absorption is neglected, even uncalibrated signals at a given site can be compared, i.e., discussion of the temporal evolution of the dust at that site is feasible and provides a useful contribution to the paper.

In view of the title of the paper I suggest to extend a little bit the discussion of the ceilometer data: As the MLH cannot be derived from the ceilometer data when the (strong) decrease of the transmission overcompensates the large backscatter coefficients of the dust, at least statements like "dust was present up to at least a height of xxx m" can be made. CALIOP data indi-

cate that the top of the dust layer was typically between 2 and 4 km, however quite variable (in time and space) and sometimes multi-layered. Because of lack of co-located measurements (except the example shown in the revised paper, where some, but not all, ceilometer sites are met) no independent measurements of the actual distribution of the dust are available. Thus, the interpretation of the (upper part of the) ceilometer profiles must remain ambiguous. This can be discussed in the paper. However, when describing the lower boundary of the dust layer, the authors can rely on the ceilometer data (as they do in the revised version).

Line 571: What is meant with a "two-layer shape"? An elevated layer? Similar problem at line 618 ("arc shape ascent").

Fig. 17: It seems that the height above sea level is shown, not the height above ground level as indicated by the y-axis. The figure caption seems to be okay.

Fig. 21: Give wavelength of the CALIOP-data. A blow-up is advisable to better see the situation over Israel. Comment on the "blue range" below 2 km: is this total attenuation? What is the elevation of the ground? What is the green line in the right panel showing?

5. Conclusions: A general message to remember is missing: I suggest to clearly point out the contribution, the strengths, and the limitations of the ceilometer network when observing dust storms (not necessarily as strong as this event). In the paper they was demonstrated for one very strong event, but conclusions that are beneficial for future investigations and (maybe) can be used for other regions should be made.
6. Miscellaneous: refer to Fig. 20 instead of Table 6 in line 355. Change "uncelebrated" to "uncalibrated" in line 281, captions of Figs. 6-13 should be changed to 7.-10. September.