

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

by Leenes Uzan et al.

The Eastern Mediterranean region including Israel experienced an extreme dust storm in September 2015. This gave rise to a number of papers investigating the development and characterization of this dust storm by means of numerical models and measurements. Uzan's paper belongs to the second category by considering the spatiotemporal distribution of the dust as measured by a network of 8 ceilometers in Israel. These measurements were supplemented by additional data sets from ground based in-situ measurements and passive remote sensing as well as satellite observation. As the ceilometers – the main focus of the paper – contribute the vertical distribution of dust in the lower part of the atmosphere with very high resolution they do provide valuable information. As a consequence the topic is worth to be published in ACP.

However, before publication can be considered the paper must undergo significant improvements: the discussion of the findings must be much more precise and convincing (and extended), and the organization of the text must be (partly) re-organized. Moreover, many expressions are unclear (the reader can imagine what is meant, but the text in its strict sense is not completely correct) and statements are inconsistent; it is impossible to mention all of them in the framework of a review (only a few examples are mentioned below). Several citations must be added. I strongly encourage all authors to carefully check the text in detail. According to the paper S.E. and P.A. were guiding the research, so it is expected that they can provide significant support.

For me as a reviewer it is really hard to help to improve the paper as many of the mandatory changes interact with changes somewhere else in the paper and many amendments may induce more changes and adaptations. As a consequence my comments below cannot be exhaustive. Moreover I will not comment on typos and linguistic deficiencies – this can be improved during copy editing (if the paper is accepted based on its scientific content).

Major concerns:

1. The description of the instruments and data sets on the one hand, and the presentation and discussion of scientific results on the other hand should be clearly separated. Presently e.g. the "radiosonde section" includes results (lines 211 ff.) that should be moved to Sect. 3. In the "results"-section the

discussion should be more elaborated and linked to the findings from the other data sets (provided in part when the MLH is discussed). Figs. 5a and 6 also belong to the results. A (short) section on the satellite data used in this study is missing and might be included. Note, that the titles of the subsections are inconsistent: "ceilometers" are instruments, but "PM10" is not.

Vice versa the role of the scaling factor (lines 282 ff) should be moved to the description of the instruments and data sets. In particular the ceilometer section must include more relevant information (see below).

2. The discussion of the scientific results must be improved and extended. In the present state the paper is sort of a collection of (useful) pieces of information, but their relationship and their interpretation is not sufficiently elaborated.

First of all the absolute values of the attenuated backscatter must be checked: some of the numbers are unrealistic and the corresponding figures are not clear. For example the labels of Fig. 7c (e.g. 0.000029 in units of $10^{-9} \text{ m}^{-1} \text{ sr}^{-1}$) are confusing. Moreover, there is a lot of information only shown in figures but not explained in words. The authors should be aware that a comparison of brown vs. brown color or brown vs. blue color (Figs. 10–16) is not suitable for a scientific publication. Please use quantitative numbers! Think about plotting coincident attenuated backscatter profiles of the 8 sites (similar to Fig. 7), this may help to see temporal delays in the arrival and decay of the plume.

Make sure that all figures are explained in detail (e.g. missing for Fig. 1 and Fig. 4) and their contribution to the scientific objectives is clear.

Below please find a few more detailed comments:

- Whole text: use a common format for dates: "8. September 2015" instead of "08.09.15" is certainly a better option.
- Whole text: Use "lidar" instead of "LIDAR".
- line 61: I recommend to add a short paragraph here introducing the scientific objectives and the benefit of the paper. Something like "One of the strongest events ever... It was investigated already by several studies... However, ... is missing. Therefore, in our investigation we focus on...". Then the short description of the event and the review of the existing publications can follow as is.

- line 62: Fig. 1 seems to be the justification that it is worthwhile to study the event. A detailed explanation of Fig. 1 is however missing.
- line 68: How does the visibility and the reference to AERONET fit together? Give a citation for AERONET.
- line 71 (figure caption): The AOD derived from MSG (mention the sensor!) is shown. Make clear that Fig. 6 shows the AOD from AERONET for comparison.
- lines 78 ff: Give citations for Meteoinfo, MODIS, EARLINET, ICON-ART.
- line 91: Avoid acronyms in cases when it is only used once or twice, e.g. RST and SBF.
- line 98: When referring to CALIPSO, mention that it was found that the top of the dust layer was at about 3–4 km (though the overpass was several hundreds of kilometers east of Israel). Even better: include a (short?) discussion of CALIPSO measurements (hopefully closer to Israel than in Gasch et al., 2017) into the results-section showing where the upper boundary of the dust layer has been. For this purpose, quicklooks from the CALIPSO-website could be sufficient. Then, the measurement range of the ceilometers can be highlighted (it is doubtful that the ceilometers can fully penetrate the dust layer at all times).
- line 115: Where was the lidar located?
- line 118: Were the model results compared to CALIPSO or EARLINET (both are lidars)?
- line 120: "...but the ability to predict the details were partial..": I don't understand this sentence.
- line 122: Lidars don't provide "aerosol concentration", but backscatter coefficients (or extinction coefficients, depended on the system).
- 124: Give citation to "deep blue algorithm".
- line 130: What is meant by "scattering properties"? Is this relevant for this paper?
- line 139: "So far, no attempt has been done to relate the models findings...". This seems to be in contradiction to the previous review of publications. The benefit of this paper is the provision of vertical profiles of the dust in the

lower part of the troposphere and the continuous measurements at 8 sites. This helps to obtain a more complete picture (with high resolution) of the event over Israel: this is a valuable contribution.

”...spatial, temporal and vertical...”: vertical can be omitted because it is ”included” in spatial.

- line 140: ”display” \longrightarrow ”discuss”
- line 144: ”Section 3 presents the results”: this is very general and does not help the reader. Specify the content of Sect. 3 a little bit!
- line 152: ”... to the atmosphere above its measuring point”. Must be ”top of the atmosphere”. Omit ”above its measuring point”: strictly it is a slanted column.
- line 159: ”...to add the vertical aerosol distribution...”. The distribution is not added, but information on the distribution. Check the whole paper for similar wordings that are not fully correct.
- line 165: To be more precise the first sentence and the corresponding citations should be modified: Start with a general statement on the potential of lidars to observe (dust) aerosols: here you can cite Mona et al. (already mentioned, but missing in the list of references), more papers from the EARLINET community such as Wiegner et al. (2011), Papayannis et al. (2008), Ansmann et al., (2003), all in J. Geophys. Res., or other papers. Then, you can state that with recent improvements of the ceilometers’ hardware these eyesafe single-wavelength systems are getting more and more important in particular when implemented as network (move the citation of Wiegner et al., 2014, to this place). The Weitkamp-paper is also missing in the reference list – so I don’t know why it is included here. Finally, Vaisala’s CL31 can be introduced but note that the cited Munkel-paper from 2004 is on the CT25k-ceilometer, not on the CL31. So replace this citation by e.g. Munkel et al., (2011)¹. Kotthaus et al. (2015) (cited later) is indeed on the CL31.
- Subsection ”ceilometers”: This paragraph needs major revisions. A few points: Make clear what you want to measure/determine: mixing layer heights, general structure of layers, heights of lower and upper boundaries, attenuated backscatter, particle backscatter coefficients or whatever. Depending on the desired output the requirements on the data evaluation are

¹Munkel, C., Schäfer, K., and Emeis, S.: Adding confidence levels and error bars to mixing layer heights detected by ceilometer, Proc. SPIE, 8177, 817708-1817708-9, 2011.

different. Include the discussion of the "scale factor" here: it is given by the manufacturer and it is unknown to the user where it comes from and how accurate it is. What is the purpose of it? Is it just an "mean" conversion factor of counts (no unit) to attenuated backscatter in $\text{m}^{-1} \text{sr}^{-1}$? When you mention the wavelength of 910 nm, you should also mention that the signals must be corrected for water vapor absorption whenever you want to quantitatively derive any aerosol related quantity (e.g. link to AOD); here the citation of Weigner and Gasteiger (already included in the reference list) should be added. As a consequence of Kotthaus et al. you should give the firmware version of your ceilometers. You should mention that the overlap correction is automatically performed by the proprietary software and that it is not disclosed to the user. If the overlap correction creates signal artefacts (at 50–100 m), different for each ceilometer, is this crucial for your scientific objectives (if yes, what are the consequences?)? Be clear with the temporal resolution (it can be selected by the user in the range from ... to ... s; in this paper 16 s was selected, is there a special reason for 15 s for some ceilometers?). 7.7 km applies to all ceilometers, not only that at Beit Dagan. Do you know the pulse energy of the CL31?

- lines 208 ff: Move all results to Sect. 3. "...disclosing the different meteorological conditions...": This is a too short statement: explain, what is shown. Explain the differences that can be seen from Fig. 4. What are the conclusions with respect to the overall topic of the paper?
- line 223: Give the manufacturer (and type) of the instruments.
- line 235: If the availability is given for the PM10-monitors it would be consistent to mention the availability of the other instruments as well (certainly a minor point).
- line 236: "(87% of the monitoring stations)" can be omitted.
- lines 237/38: This is also a result and should be moved to the corresponding section. The same is true for Fig. 5a. Add a xy-grid to facilitate the interpretation of Fig. 5a.
- "...within a 1 min period": maybe it is better to remove this. From this statement the measurement cycle is not clear: 8 s measurements, 22 s break, 8 s measurements, 22 s break, 8 s measurements? This is more than 1 minute.
- line 262: As this is a result it should be moved to Sect. 3.

- Fig. 6: to be moved to Sect. 3. I don't understand the x-axis? Why is the length of the days different? Where are the night measurements of AERONET from (do you deploy a lunar photometer?)? Is there a specific reason for not choosing a line plot in Fig. 6a (as in Fig. 6b)?
- line 278: The results-section starts with the list of data sources used: AOD satellite pictures are mentioned but not explained in Sect. 2, whereas radiosondes are explained in Sect. 2 but not mentioned here.
- line 281 ff: Move lines 281–283 to Sect. 2. Check carefully the following numbers of the attenuated backscatter; some of them are unrealistic ($7 \cdot 10^{-1} \text{ m}^{-1} \text{ sr}^{-1}$). See also general remarks at the beginning. Add a xy-grid to Fig. 7.
- Figs. 8–16: What is the unit of the color code? It does not agree with the values given for attenuated backscatter. Are these numbers the "counts"?
- line 320: "the time corrected from local time to" can be removed. The labels of Fig. 8 should be the same as those of Figs. 10–16.
- lines 328 ff: Obviously attenuated backscatter is converted into particle extinction coefficient (with a relatively large uncertainty inherent in all CL31 measurements; see water vapor absorption mentioned above, unknown lidar ratio, unknown accuracy of the scaling factor) with an estimated lidar ratio. Then, the visibility is estimated according to Koschmider. Which altitude was selected for this conversion? The problem is, that if it is done for a large altitude (maybe 100 m or more) it is difficult to compare this visibility to independent ground based measurements. If it is done for the ground, then the overlap problem is critical. Nevertheless, an order of magnitude agreement should be possible, but please extend this paragraph by explaining all aspects of this comparison.
- lines 334 ff: "Ceilometers are not provided with an AOD...": I don't understand this paragraph. AOD and MLH are mentioned but it is unclear what the message is. The retrieval of the MLH from ceilometer data is completely different from a retrieval of the AOD (provided that can be determined at all). So, how is the validation ("verify ceilometer") of the ceilometer's performance achieved? Please rephrase and extend this part.
- line 381 ff: A short paragraph should be included here to prepare the reader: The event is divided into several phases (not necessarily split into single days,

one phase can be shorter, another can last for more than one day; development, main phase, decay can be an alternative) according to certain criteria and to highlight consistencies/inconsistencies of different data sets/models (by doing this "Next, we describe the decrease of aerosols aloft on mid-day" in line 299 can be omitted).

Then, each subsection such as "Entrance of dust into Israel – 7 Sep 2015" should include the full discussion and interpretation of all available data sets for the corresponding period, i.e., parts from Sect. 2 should be included here whenever applicable.

- line 401: What is meant by "decrease"? Concentration or altitude?
- line 405: "clearly shown in Fig. 13-16 between 08-16 UTC)": This is indeed hard to see. Can you explain this in a more quantitative way?
- line 408: Please clarify what "these model findings" are? The vertical distribution of dust (backscatter)?
- line 422: $2000 \mu\text{g}/\text{m}^3$: is this in contradiction to $9800 \mu\text{g}/\text{m}^3$ in line 416?
- line 437: "...limited radiative transmitted...": what does this mean?
- line 443: The figures don't support the "total clearance" statement.
- Fig. 17: This figure is misleading as the range of the color code is different from Fig. 14. This should be pointed out clearly. As long as the intercomparison with the AOD (AERONET) is qualitative only, it would be helpful to show the typical(?) background(?) values of the AOD and attenuated backscatter of the days before the event for comparison. From Fig. 6a it seems that after 11. September 2015 the AOD was still relatively large (by the way: can you give the annual mean AOD of the sites in Israel?). How "typical" are the ceilometer profiles between 11. and 14. September 2015?
- line 454: 250 m: is this the vertical extent or the altitude?
- line 486: The PM10 measurements are considered as in-situ measurements, not remote sensing.
- line 488: "...for the first time, such an event is vertically analyzed using an array of ceilometers...". On the one hand this is true, on the other hand it is slightly misleading as the vertical structure (by other means) has already been investigated. So it might be advisable to use a less strong statement in the next sentence (a note on the limited measurement range).

- lines 492, 497: "plum"!
- line 494: "mainly of mineral dust": where is this information coming from?
- line 507: "The complicated nature of the dust...". This is indeed a benefit of the ceilometer measurements: the temporal changes of the vertical distribution of aerosols can be monitored with very high resolution and thus help to better understand ground based measurements (even though there is the overlap-issue and the beam is likely fully attenuated somewhere in dense dust layers). It can be concluded that it makes no sense to "extrapolate" small scale vertical features in Israel from measurements in Cyprus. This should be emphasized as it is a strong argument for the publication of such a study.
- line 575: This citation must likely be updated.