

## ***Interactive comment on “New insights into the vertical structure of the September 2015 dust storm employing 8 ceilometers over Israel” by Leenes Uzan et al.***

### **Anonymous Referee #2**

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#### General

The paper is not in a good shape and needs significant improvement. My comments may help. Please do not feel criticized (as authors). I need to be critical to help you to improve the paper.

As the main goal ceilometer observations, continuously taken during a huge dust storm, are presented and discussed. However, the authors seem to have only minor experience how to handle ceilometer data properly and carefully. This is one of the most serious problems I have with this paper.

Ceilometers are made for cloud detection, and not for aerosol profiling. This is espe-

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cially true for the Vaisala system, which operates at 910nm and is sensitively affected by water vapor absorption. In case of this severe and unique dust storm, the ceilometer obviously could be used to measure the lowest few hundred meters of the dust layers. But the main part, from 1 to about 4-5 km height remained undetectable.

It seems to me that the authors try to avoid to clearly state: The ceilometer is of rather limited use in dust plume tracking. We were not able to see the full dust layer. But such a statement is required! ..and will not disturb the main goal of the paper.

As reviewer I have to say: This is not acceptable and has to be improved! In cases with thick dust layer with optical depth >1 the transmitted (rather weak) radiation pulses of these ceilometers are immediately attenuated in the lowest part of the dust layers.

The other unacceptable issue is that the authors state that they present their ceilometer results in terms of ‘attenuated backscatter coefficient’. However, this is simply wrong, very misleading, and will be confusing for many readers (especially aerosol lidar scientists).

By definition: The attenuated backscatter coefficient is the Rayleigh-calibrated range-corrected backscatter signal! ... Such a calibration is usually impossible (for 910 nm backscattering), even at clear sky conditions.

Thus, the authors show color plots of the ‘basic’ range corrected ceilometer signals! All the plots have to be changed to meet this point. .... as will be explained in detail below.

There are many other points that have to be clarified as explained below.

Major revisions are needed.

Details (P = page, L = line):

The abstract does not just summarize the paper properly. The abstract should be compact, i.e., as short as possible and just cover the contents of the paper: The main goal, the instruments and methods used, and main findings,.... no outlook..., no

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unnecessary (motivating) statements that can be given in the introduction. . .

P3, L78, reference for Meteoinfo or [http...](http://...)

P3, L94, the model better explained. . . . compared to what?

P3, L94, ART, give reference

P4, L100, Please check the finally revised version of the Gasch paper. I asked these authors, the final version should be out soon.

P4, L115, L118, L122: lidar. . . not LIDAR

The introduction could be more straight forward, as follows: There was a huge dust storm in the Middle East, however, the dust forecast models failed. The question was then: Why? This question motivated Solomos et al. (2017) to run a cloud resolving model system. They found the most probable reasons. Please state their findings in the introduction! Afterwards, Gasch et al. (2017) used the new IKON/ART model system with rather high resolution (a global cloud-process-resolving model!!!) and also investigated this dust storm. . . and discussed the storm in even larger detail. . . and concluded. . . . Please read the final version and present their final conclusions. This would be a nice introduction, very informative, so that the reader would learn a lot. And then you could provide the motivation for your own ceilometer study. . . . Because open points remained, and this historical dust storm must be documented for a variety of regions in the Middle East.

P5, L134, Is there no discussion in the literature on dust-radiation-dynamics relationship? I believe, the SAMUM group (Tellus 2011 special issue) investigated the relationship between dust (and smoke) and the radiation field and changes in the air flow (dynamics) as a result of the impact of dust and smoke on the radiative fluxes. Such dense dust layers as in September 2015 certainly had a huge impact on the radiation budget and significantly changed the weather pattern and thus air mass transport. This may explain why the routine dust forecast models failed because the forecasted

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dust concentration was too low to produce a significant radiation effect on the weather pattern (and dynamics) and dust transport in the model.

P5, L138-L142: You must clearly say in the beginning how the ceilometer network can contribute: Ceilometers can detect the dust layer base and provide some information about the lower part of the dust layer and, very important, the downward transport towards the ground. This is a good and valuable contribution to atmospheric science. On the other hand, not more than that! But this is fine! Nevertheless, you need to provide the limits of such systems! Very clearly! At these high AODs, there is no chance to detect most of the dust and the dust layer top.

P6, L159-161: These statements are misleading. At these large dust AODs, satisfactory aerosol profiling with the Israeli ceilometers was impossible! Furthermore, I do not find Ansmann et al. and Mona et al. in the references.

P7, L173: Do you think that you would find the true mixing layer height (when applying the wavelet analysis) under such dense dust conditions? I am not sure! Usually you have the polluted mixing layer and the clear free troposphere on top. At these conditions, the wavelet technique works well. Now you have the opposite. And there was almost no solar heating of the ground (almost no convection), just a residual (less dust laden) layer below the dust layers. What you detect and interpret as mixing layer top is to my opinion just the other way around: the dust layer base height. One should state and discuss this point more clearly. At AODs of 2 and more there is no convection left to lead to well mixed conditions. The dust layer is warmer than the lowest, near-surface tropospheric layer and produces the temperature inversion observed with radiosonde.

P7, L178: . . .up to 7.7 km height AGL. Yes the ceilometer may measure up to 7.7 km height, but only for clear sky conditions with AOD of the order of 0.2, at least clearly below 1. One has to state that. In case of the dust storm, all delivered 'counts' above 1 km were more or less just background noise!

P7, L183: Now a critical issue: I checked the Kotthaus paper. According to this pa-

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per, and as it is well known, the ceilometer delivers range-corrected signals in arbitrary units. The measured counts are converted by using a conversion factor to obtain useful signal profiles, when the background is subtracted. We may denote these range corrected signals as level-0 data. Vaisala uses a 'conventional' conversion factor to transfer the background-corrected signals into lidar backscatter signals. But this is NOT the attenuated backscatter coefficient! To obtain the attenuated backscatter coefficient (something like the Rayleigh-calibrated range-corrected signal) you need an actual calibration (to obtain an actual conversion factor). This actual conversion factor however can only be derived under clear sky conditions (so that clear sky backscatter in the free troposphere becomes visible and an accompanying sunphotometer delivers the total OD (aerosol plus water vapor absorption) at around 910 nm, and the aerosol-related AOD at 910nm by using interpolation between the measured 870 and 1020nm AOD). At these favorable conditions, the range-corrected signal may be calibrated to deliver the attenuated (aerosol) backscatter profile, and by using the Klett method and adjustment of the lidar ratio (that has to take care of water vapor absorption in addition in case of the Vaisala ceilometer) even the total particle backscatter coefficient. But all this was not possible under these dust storm conditions. In conclusion, you just present range-correct signals in arbitrary units. All the plots have to be changed accordingly. The paper is unacceptable if this important changes are not done.

P7, L188 ... The Beit Dagan Ceilometer measures up to 7.7 km. Yes, as mentioned, under clear sky conditions. At dust storm conditions with AODs from 2 to probably 5 and more, the ceilometer measures just noise from 1 to 7.7 km height. So this is an unacceptable statement. Please change.

P7, L198: Please remove the trivial statement about the radiosonde ascents. Please provide just information on radiosonde type and company, and meteorological parameters measured.

P8, Figure 4: I would suggest to use very contrasting colors, orange and blue, or such nice colors as in Figure 6, for the different sites. We need x-axis description (RH (%)

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and Temperature (deg C)) and also y-axis text (Height (m.a.s.l.)). I am wondering. ...: Do we need to show the Cyprus profiles? Further suggestion: To my opinion it would make sense to show the Israel radiosonde profiles up to 6-7 km height, to have a chance to identify the dust top height because above the dust layer the RH should increase again, at least should show changes, and there should be a temperature gradient change as well or even a temperature inversion at dust top height.

In the radiosonde plots there is always written: ... launching sites... please improve!

P9, Figure 5 caption: Please clearly state what the yellow curve shows. It took me some time, to see that it belongs to the southern region. The curves show the mean of all sites of a given region? Hard to see the ceilometer stations. Give them a yellow full circle and plot them last (after plotting everything else). Then one should clearly see the sites.

P10, L264. The Rehovot station did not work, the instrument was out of order? ... or did the station not allow useful data analysis because the AOD was too high? Please clarify and state what was the case...

P10, Figure 6: the time axis ... the day scale (width) is changing from day to day. E.g., the 8 September is very narrow, the 7 September has a factor of 4 more space..., why?

P11, L282: The authors write: the ceilometer profiles are unitless and therefore divided by a scale factor of  $10^{(-9)} \text{ 1 / (m sr)}$  enabling quantitative analysis (Kotthaus et al., 2016).

This is simply wrong and unacceptable. Please improve! The basic ceilometer data are signals (let us say ... in units... counts per second), and if they are then range-corrected... then you get the dimension 'counts per second times  $\text{m}^{**2}$ '. So the values are not unitless, but usually given in arbitrary units. Next, by dividing these data by  $10^{(-9)}$  ... does not change anything. You still have just range-corrected signals. You

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can only obtain a profile of the attenuated backscatter coefficient if you are able to calibrate this range-corrected signal profile in the tropospheric region with pure Rayleigh backscattering or in the way as described above. So, you show range corrected signals!!! And not attenuated backscatter!!! As mentioned already, you must change . . . to range corrected signals in all ceilometer plots!

P12, Figure 7, We need a clear statement, that the range-corrected signals shown in Figure 7 decrease rapidly and is close zero at about 700 to 750m in b,c,d because of the strong laser light attenuation in the dust layer! As long as such a statement is missing the reader may believe to see the full dust layer and the top is at 750-1000m height. To repeat: This is unacceptable.

Then, in Figure 8, the numbers for 'your' attenuated backscatter are suddenly up to 15000, compared to values of about  $10^4$  (-14) in Fig 7 (b,c,d)? Then in Figs 10-12: up to 10000. And in Fig 13, suddenly only up to 800. . ., Fig. 14 up to 15000, and Fig15-16 up to 10000. So all this is rather strange. . .and only reasonable and understandable if we switched to range corrected signals (arbitrary units).

So, please change. . . . to range corrected signals.

P12. Figure 8,x-axis please show data always from 0-24 local time (or UTC). Again we need proper text for the x-axis and y-axis, as it is the case in Figure 7.

P12-15: All the Figures 8,10-16 have no x-axis and y-axis description. This is poor and unacceptable. And again, all these ceilometer color plots suggest that the dust layer was just a few hundred meter thick. This is dangerous! The reason is simply the almost total attenuation of the ceilometer radiation pulses by the rather dense dust layers. This must be made very clear.

P13, L324: again and again: you were able to track the dust layer base only, this must be clearly said.

P13, L330: A visibility of 200m (visual meteorological range is defined by an AOD

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of 3, after Koschmieder for an AOD of 4) according to an AOD of 3 means that the particle extinction coefficient was  $15 \text{ km}^{-1}$  or  $0.015 \text{ m}^{-1}$  and the backscatter coefficient is then  $0.0003 \text{ m}^{-1} \text{ sr}^{-1}$  if the dust lidar ratio is 50sr. All your 'numbers' are far far away from these value. This corroborates: It is impossible and dangerous (and thus not justified) to convert the range-corrected signals into optical properties just by taking 'some' conversion factor!

P13, L334-L337. I would remove this text on the ceilometer and the AOD upper limit. This is useless. The Vaisala ceilometers are not built for aerosol profiling. The wavelength is bad, the signals are corrupted by water vapor absorption.

P13, L347: Plots are given in different scales to highlight the dust features. This is ok, because range corrected signals are shown and the ceilometer performance changes from site to site (from ceilometer to ceilometer). So again, there is a clear need to work with range corrected signals.

P13, Fig. 9 shows the dust base height. To my opinion it is misleading to denote the near-surface layer a 'mixed layer' at these conditions with no vertical exchange.

P13: The text on this page is poor and needs to be significantly improved.

P15, L3834: Please clearly state where the dust layer top was found by Solomos et al. (2017).

P17. . . . Figure 17 indicates that there was dust higher up. The AOD decreased towards 0.5-1 on 12-14 Sep. A perfect mixing layer could develop now up to 750 m, as seen on 13 and 14 Sep. Nice to see, that the aerosol dried in the PBL during the morning hours and thus the color of the range corrected signals changed from red to green and blue (for dry particles producing less backscatter later on).

P17, L435: At very high optical depth as on 9 Sep, I would assume that convective motions in the PBL as well as a sea breeze winds cannot develop. Are you sure that sea breeze developments were possible at these days with almost no sun and

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differential sea/land heating? Please keep the discussion free of speculation.

P17, L444: You state: The ceilometer reveal total clearance on 10 Sep! But the Weizmann Institute AERONET shows AODs of 2 and more on 10 Sep! What is wrong, what is true? Please clarify?

P18, L454: ..as a dust layer of 250 m thickness (fig 11-13, 15-16) penetrated Israel at a height of 1000-1500m.... How do you know the depth of the dust layer? The ceilometer fails to see higher up.... So, how do you know? I would leave out to mention any dust layer depth.

P18, L461: The AOD was >1.0 all the time on 9 and 10 Sep...until 12 Sep. What do you thus mean with dissipation of dust?

P19: The conclusions have to be rewritten completely after improving all the text before along the lines this review and the other review.

P19, L488: for the first time such an event is vertically analyzed..... this is misleading because Mamouri et al. already used lidar to characterize the dust storm. You probably wanted to say, for the first time .... with a ceilometer network. However, you should mention that there were already lidar studies with Cyprus lidar and CALIOP lidar, and now you come with a ceilometer network study..... Then this would be more clear, and of course this is a new aspect.

P19, L490: Again, you have to mention the limits of a ceilometer. It was too weak to see the layers higher up. No chance to see the main part of the dust layers and dust layer top.

P19, L494: As a result..... of what?

P19, L498-499: When were the AE values high again? They were continuously <0.5 even on 14 Sep (Weizmann AERONET).

P19, L502-504: This is speculation, at least to my opinion. Be more save with your

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statements.

P19, L506-511: Again, dangerous statements. I would remove. Otherwise, you need to check the CALIPSO overflight over Israel to corroborate your speculative suggestions. However, the modeling papers of Solomos et al and Gasch et al. (partly based on model plus CALIOP results) do not leave room for statements like ... who knows to what height the dust plumes reached over Israel. To my opinion, in the Middle East dust layer top was up to 4-5 km height everywhere.

P21, L554: No authors..

P23, L621: TOASJ...?

Some more comments:

I asked colleagues from Israel to help me with... the following..., I am not familiar with Hebrew language... for example: <http://www.svivaqam.net/>.

Technically, you have at <http://www.svivaqam.net/> 'reports from numerous sites'... The dust storm: at September 7, around 22:00 values above 100 are already recorded for two sites (cities): Kiriya Ata and Nesher (next to Haifa).

Further questions and recommendations, they had:

The authors must justify what new information they get using ceilometer more clearly than in Figure 18, what new insights they get about the extreme dust event? And number/summarize all "new insights" about the event that they discover.

What sources of errors do they have when using the ceilometers? They should critically state the limitations, disadvantages and advantages. Including comparison between different ceilometers that authors used for the analyses. Without this comprehensive critical discussion on authors findings, the outcome of the paper is doubtful.

Haifa region (Northern part of Israel) must be included for more comprehensive analyses. The authors stated that: "Unfortunately, there are no PM10 measurements in

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northern Israel".(Line 233) During the dust event there were seven sites that measure PM10 that were available. Figure 5 is not correct therefore, it covers more area, than depicted in Figure 5, including the Haifa Bay area.

Figure 18: The major claim- the dust penetrates from the East. But- combining PM10 from the Haifa Bay area, there is a "jump" towards values of 2500-3000 micrograms/m<sup>3</sup> at 8 of September, similarly to East, which means it has two entrances/sources. Do the authors see the "North region" dust entrance using ceilometer data?

Why did the authors not include PM2.5 sites data.

"The AERONET (Fig. 6) and ceilometer plots (Fig. 8, 11-16) reveal that the first dust plume penetrated Israel at approximately 04:00 UTC". What day? Sept. 7 or Sept 8?

Page 3, Fig.1: The shown AOD range. . . is that the range of trustworthy values? Because the dust AODs were much higher than 2.7. So one could enlarge the color scale. . . How large is the uncertainty in the MSG data, source for the data (http. . .) should be mentioned.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-634>, 2017.