Author's Response:

We wish to thank both referees for their comprehensive and elaborated comments. Despite some mistakes they have revealed in the manuscript, the referees took the time and effort to explain the source of the inaccuracies and offer good advice and practical solutions.

Overall, we have received extensive comments on all sections and figures by both referees. Therefore, we've decided to combine the referees' comments and write our point to point response along the manuscript sections:

- Abstract
- Introduction
- Instruments
- Results
- Conclusions and discussion
- References

We hope this structure is acceptable and easy to follow.

Sincerely,
Leenes Uzan, Dr. Smadar Egert and Prof. Pinhas Alpert

Abstract: lines 39-51

Referees Comments:
Referee #2: *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 unnecessary (motivating) statements that can be given in the introduction....*
Author's response: Comments accepted.
Author's changes in manuscript: The abstract was rewritten as recommended.

Introduction: lines 56-145

Referees Comments:
Referee #1: *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.*
Referee #2: *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.*
Author's response: Comments accepted.
Author's changes in manuscript: Keeping in thought this paper is supplementary and an important insight of the extreme September 2015 dust storm, we significantly enlarged the overview of previous studies describing the dust storm source, the analysis of observations and posteriori model runs. On our behalf, we added information describing the weather conditions and analyzed the dust event evolution over Israel in the lower atmosphere (from ground level to about 1 km). The data we present can support verification of state of the art model simulations (actually already been done by Gasch et al., (2016) which cited our presentation from the EGU conference).

Referees Comments:
Referee #1: *line 68: How does the visibility and the reference to AERONET fit together? Give a citation for AERONET*.
Author's response: Comments accepted.
Author's changes in manuscript: The referee is correct. AOD from AERONET could not be a reference to visibility. The AERONET reference was erased. We added a citation for the AERONET data.

Referees Comments:
Referee #1: *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 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.
Referee #2: *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.*
Author's response: Comments accepted.
Author's changes in manuscript: Fig 1. was moved to the " Results and discussion" section where it was discussed. The caption was updated with the MSG SEVIRI sensor and source of the MSG data. The uncertainty of AOD measurements from MSG SEVIRI can rise up to 15%. The Sede-Boker AERONET AOD values at 12 UTC were added to the figure above the spot indicating the Sede-Boker site.

Referees Comments:
Referee #1: *lines 78 ff: Give citations for Meteoinfo, MODIS, EARLINET, ICON-ART.*
Referee #2: *P3, L78, reference for Meteoinfo or http.*
Author's response: Comments accepted.
Author's changes in manuscript: Citations were added.

Referees Comments:
Referee #1: *line 91: Avoid acronyms in cases when it is only used once or twice, e.g. RST and SBF*.
Author's response: Comment accepted.
Author's changes in manuscript: Acronyms used up to twice were deleted.

Referees Comments:

Referee #1: *Use a common format for dates: "8. September 2015" instead of "08.09.15" is certainly a better option.*

Author's response: Comment accepted.

Author's changes in manuscript: The format of all dates was changed to the suggested format.

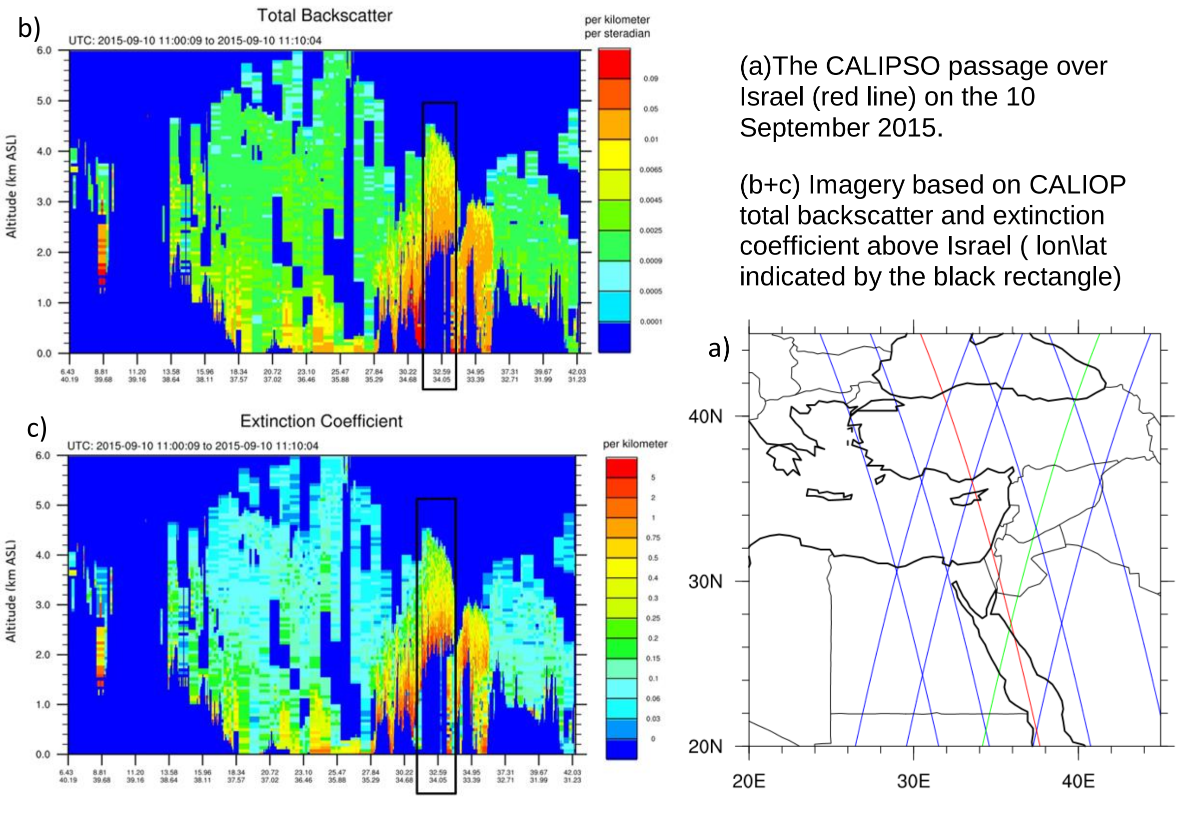
Referees Comments:

Referee #1: *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, quick looks 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).*

Author's response: Comments accepted.

Author's changes in manuscript: We added two images (total backscatter and extinction coefficient) from the only passage of CALIPSO over Israel, on the 10 September 2015. The images reveal a dust layer between 2-4 km ASL (see Fig.X below). A discussion on the CALIPSO measurements was added to the "Results and discussion" section.

Fig. X



(a)The CALIPSO passage over Israel (red line) on the 10 September 2015.

(b+c) Imagery based on CALIOP total backscatter and extinction coefficient above Israel (lon\lat indicated by the black rectangle)

Referees Comments (overview of Gasch et al.):

Referee #2:

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.".

Author's response: Comments accepted.

Author's changes in manuscript: We elaborated the overview of the Gasch et al. (2016) paper, explaining the model set up, ART reference and the concluded process of the dust storm generation. The final version of the Gasch et al (2016) was approved on the 15 November 2017 and will be cited accordingly.

Referees Comments (overview of Stavros et al.):

Referee #1:

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. Is this relevant for this paper?

line 122: Lidars don't provide" aerosol concentration", but backscatter coefficients (or extinction coefficients, depended on the system).

Author's response: Comments accepted.

Author's changes in manuscript: We have rearranged the paragraph written on the Stavros et al., research. We referred to the lidar located (Limassol Cyprus), comparison of the EARLINET lidar to the model results and the estimation of the aerosol concentration based on the lidar ability to reproduce the strength of the dust event.

Referees Comments:

Referee #1: *Use "lidar" instead of "LIDAR".*

Referee #2: *P4, L115, L118, L122: lidar... not LIDAR*

Author's response: Comments accepted.

Author's changes in manuscript: "LIDAR" was changed to "lidar" throughout the text.

Referees Comments:

Referee #1:

line 130: What is meant by" scattering properties"?

Author's response: Comment accepted.

Author's changes in manuscript: " Scattering properties " was corrected to "optical properties".

Referees Comments:

<u>Referee #1:</u>

124: *Give citation to " deep blue algorithm".*

Author's response: Comment accepted.

Author's changes in manuscript: A citation for deep blue algorithm was added (Hsu et al., 2013).

Referees Comments:

<u>Referee #2:</u> *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 dust concentration was too low to produce a significant radiation effect on the weather pattern (and dynamics) and dust transport in the model.*

Author's response: Comments accepted.

Author's changes in manuscript: The cited reference of Pu et al.(2016) did not discuss the effect of dust particles on the thermal radiation. Following the referees' remark, an elaborated discussion based on ground level radiation was added to "Results" section.

Referees Comments:

<u>Referee #1:</u> *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" - "discuss".

<u>Referee #2:</u> *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.*

Author's response: Comments accepted.

Author's changes in manuscript: We rephrased the paragraph referring to the ceilometers' contribution (lines 138-142) and listed operation the limitation of the ceilometers in general and under high AOD conditions.

Referees Comments:
Referee #1*:*
line 140: ”display” - ”discuss".
Author's response: Comment accepted.
Author's changes in manuscript: The paragraph was rewritten. Among other changes, "display" was changed to "discuss".

Manuscript lines 148-273 (Sect.2 Instruments):

Referees Comments:
Referee #1: *A (short) section on the satellite data used in this study is missing and might be included.*
Author's response: Comments accepted.
Author's changes in manuscript: A section of satellite data regarding imagery from MSG, MODIS terra, MODIS aqua and CALIPSO satellites was added to the "Instruments" section.

Referees Comments:
Referee #2: *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*
Author's response: Comments accepted.
Author's changes in manuscript: Presentation and discussion of scientific results were moved to the "Results" section.

Referees Comments:
Referee #1: *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.
Author's response: Comments accepted.
Author's changes in manuscript: we rephrased these sentences as recommended.

Referees Comments:

Referee #1: *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.*

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 at al. (2011), Papayannis et al. (2008), Ansmann et al., (2003), all in J. Geophy. 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 Mu¨nkel-paper from 2004 is on the CT25k-ceilometer, not on the CL31. So replace this citation by e.g. Mu¨nkel 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 different.*

Referee #2: *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".*

Author's response: Comments accepted.

Author's changes in manuscript: The ceilometer paragraph was changed. We described the ceilometers limitations including the instrument specific characteristics which affect the quality and availability of the attenuated backscatter profiles, the calibration methods, the rage detection limitations by the overlap function and determination of the sensitivity of the attenuated backscatter signal to relative humidity. Nevertheless, we portrayed the contribution of the ceilometers in the analysis of dust plume development in the lower part of the troposphere (up to 1.5 km AGL).

Instruments - Ceilometers: lines 164-188

Referees Comments:

Referee #1: *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 a" mean" conversion factor of counts (no unit) to attenuated backscatter in m−1 sr−1?*

Author's response: The ceilometer attenuated backscatter profiles are automatically corrected by:

*An internal calibration to convert signal counts to attenuated backscatter multiplying by 10^{-9}. The internal algorithm (resulting in 10^{-9}), unknown to the user, is suitable for al ceilometer types (C. Muenkel, private communication, September 2017).

*A cosmetic shift of the backscatter signal to better visualize the clouds base.

*An obstruction correction when the ceilometers' window is blocked (by an obstacle).

*An overlap correction to the height where the receiver field of view reaches complete overlap with the emitted laser beam.

Author's changes in manuscript: Data referring to the automatic correction of the ceilometer output profiles was added to the "Instruments" section.

Referees Comments:

Referee #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.*

Author's response: Following Wiegner et al., (2014, 2015), the ceilometer wavelength range (given as 905 ±3 nm) is influenced by water vapor absorption. However, in the case of aerosol layer detection, the water vapor distribution has a small effect on the signal change (indicating the MLH or an elevated mixed layer) because the aerosol backscatter itself remains unchanged. Consequently, except for a case of a dry layer in a humid mixed layer height, the water vapor is unlikely to lead misinterpretation of the aerosol stratification.

Author's changes in manuscript: The aforementioned explanation was added to the "Ceilometers" subsection. In our study we did not attempt to derive aerosol quantity as our ceilometers were not calibrated. We have rephrased all sentences that could mislead the reader in this manner.

Referees Comments:

Referee #1: *As a consequence of Kotthaus et al. you should give the firmware version of your ceilometers.*

Author's response: Unfortunately, 6 out of the 8 ceilometers belong to a governmental office which does not allow to publicize their firmware data. Yet, we have been confirmed by

the exclusive supplier of the ceilometers in Israel that the combination of firmware and hardware had been done according to Kotthaus recommendations.
Moreover, the title of Table 2 refers to all ceilometers as CL31 type, while ceilometer Weizmann (Rehovot) is a CL51. Therefore, we added Table X as follows:

Table X. Ceilometer configuration

Location	Type	Time resolution(sec)	Height resolution (m)	*Height range (km)
Mount Meron	CL31	16	10	7.7
Ramat David	CL31	16	10	7.7
Hadera	CL31	16	10	7.7
Tel Aviv	CL31	16	10	7.7
Beit Dagan	CL31	15	10	7.7
Rehovot (Weizmann)	CL51	16	10	15.4
Nevatim	CL31	16	10	7.7
Hazerim	CL31	16	10	7.7

*Height range is dependent on sky conditions and is limited as AOD increases.
* In all ceilometers but Beit Dagan site, data acquisition was up to 4.5 km downloaded from BLview firmware.

The firmware and hardware of the two remaining ceilometers:
Weizmann: engine board (CLE321), receiver(CLRE321), transmitter type(CLT521) firm ware version(1.03).
Beit Dagan: engine board (CLE311), receiver(CLR311), transmitter type(CLT311) firm ware version(1.72).
Author's changes in manuscript: We added the information given above.

Referees Comments:
Referee #1: *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?).*
Author's response: In our study we focused on the downward motion of the dust plume and its effect on the creation of the mixed layer height which did not reach below 200 m AGL. Therefore, artefacts up to 100 m AGL were not crucial.
Author's changes in manuscript: An explanation of the automatic overlap correction was added to the text.

Referees Comments:
Referee #1: *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?*

Referee #2: *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, 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.
Author's response: All ceilometers were capable to measure up to 7.7 km with a range gate of 10 m with the limitations of sky condition and a decrease in SNR with height. The data acquisition was limited to 4.5 km by BLview default (except for ceilometer Beit Dagan). Only ceilometer Beit Dagan has temporal resolution of 15 s set by the manufacturer. The pulse energy of the CL31 1.2 µWs ± 20%.
Author's changes in manuscript: We rephrased the sentence and pointed out the limitations of the ceilometers to actually measure up to the maximum height range declared by the manufacturer.

Referees Comments:
Referee #2: *In the radiosonde plots there is always written: ... luanching sites... please improve!*
Author's response: Comment accepted.
Author's changes in manuscript: "luanching" was replaced by "launching".

Referees Comments:
Referee #2: *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.*
Author's response: We recognize the referees' logic on the creation of thermals under high AOD conditions. Ground level measurements of global radiation, ground temperature (actually named " grass temperature" as it measures 10 cm AGL) reveal the process of ground heating on the 9 September was possible as the maximum global radiation measured at 12 (UTC+2) reached 500-700 (W m-2) in 19 out of 23 sites measured (Fig X1). In comparison, on the 8 September, maximum global radiation reached 200-300 (W m-2) in 10 out of 23 sites (Fig. X2).

Fig. X1 Maximum global solar radiation 9 September 2015 at 12 (UTC+2) measured across Israel in 23 sites. Indication of height of each measuring site (ASL) is given upon the map.

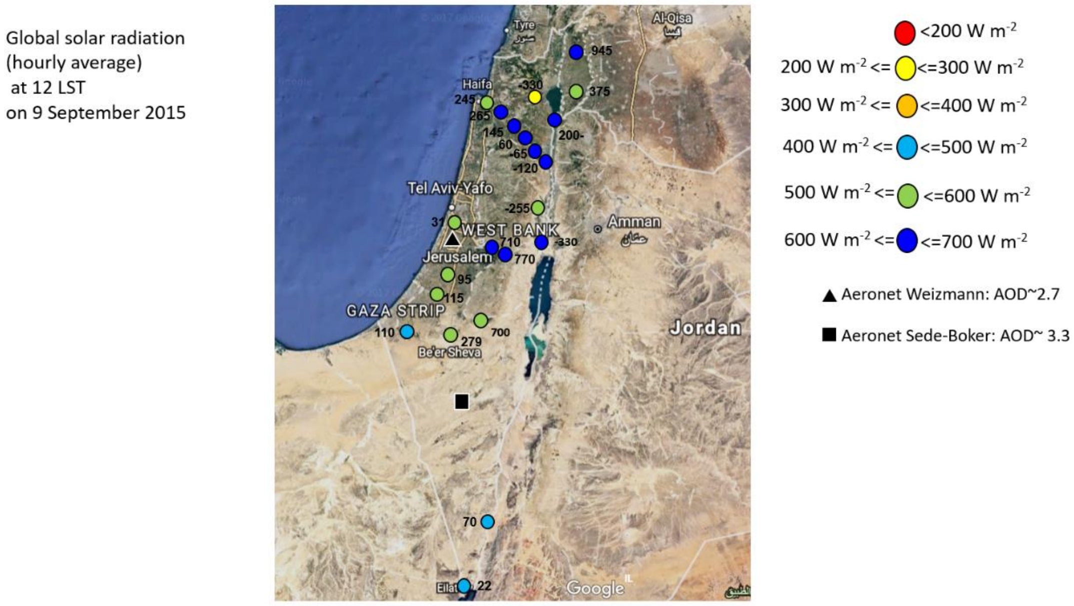
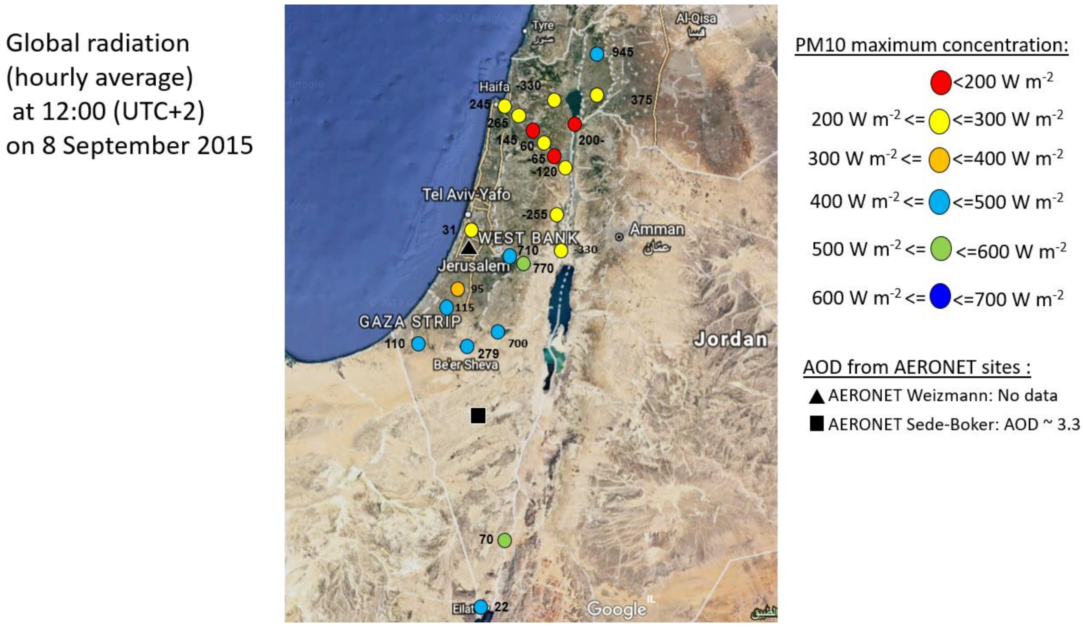


Fig. X2 Maximum global solar radiation 8 September 2015 at 12 (UTC+2) measured across Israel in 23 sites. Indication of height of each measuring site (ASL) is given upon the map



Moreover, 33 sites of ground temperature measurements were separated to 5 regions- north, south, center, coast and mountains:

Fig. X3 Ground temperature (10 cm AGL) from northern Israel

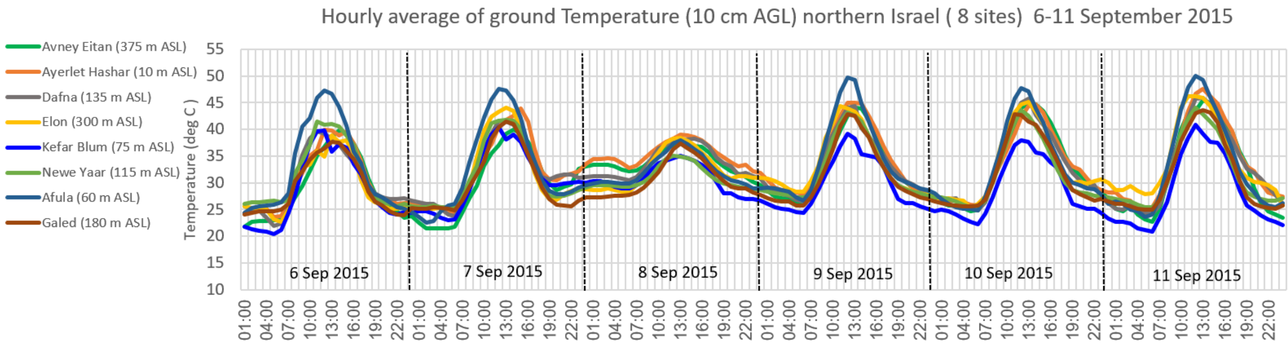


Fig. X4 Ground temperature (10 cm AGL) from mountain sites in Israel

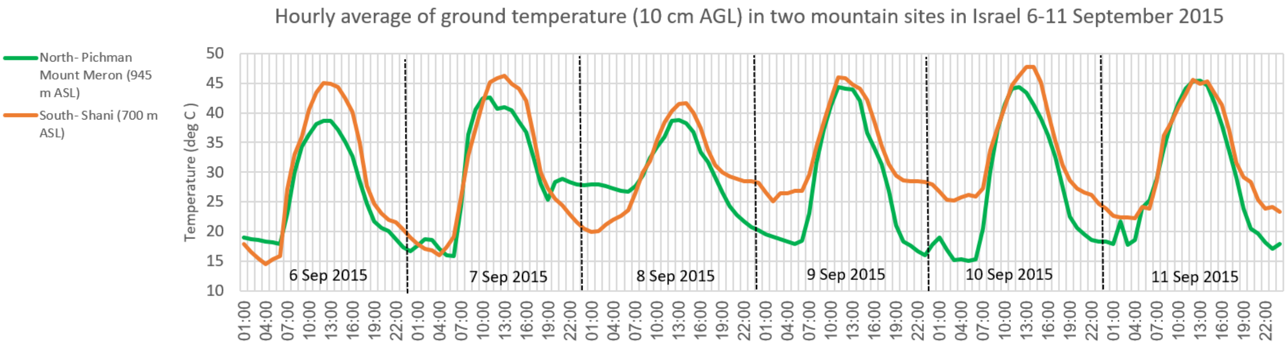


Fig. X5 Ground temperature (10 cm AGL) from valley sites in Israel

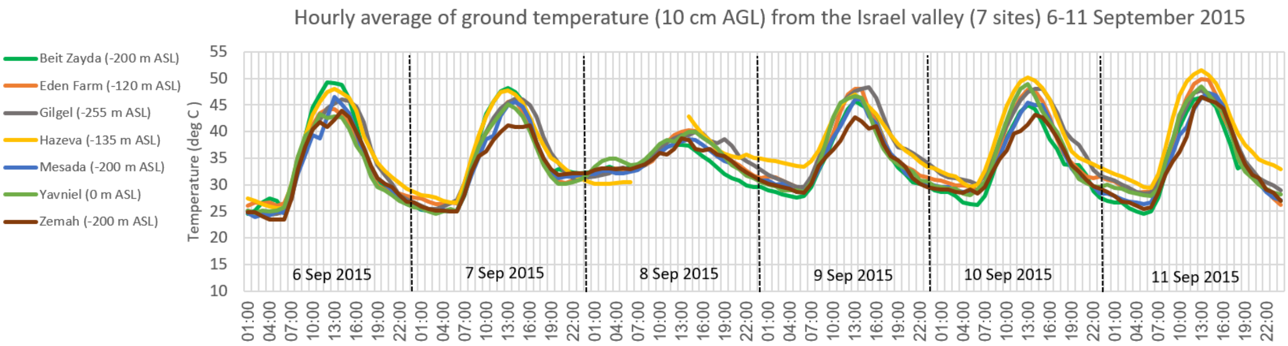


Fig. X6 Ground temperature (10 cm AGL) along the coast of Israel

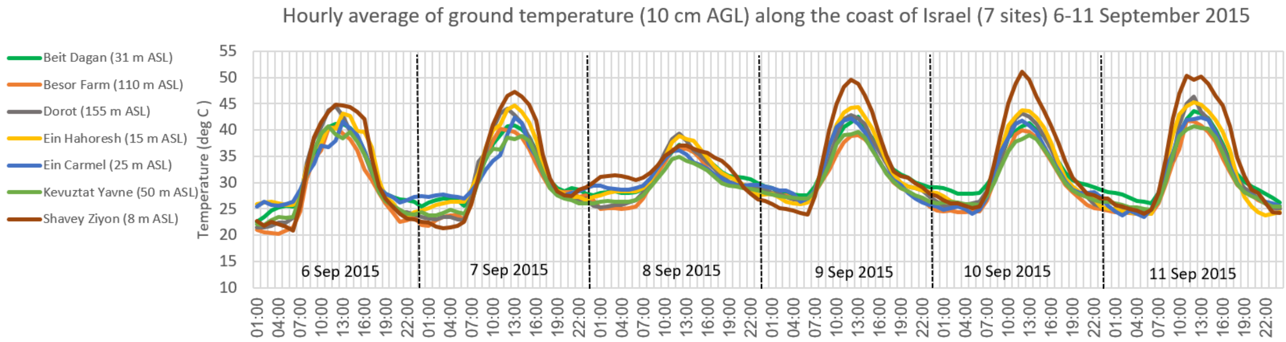
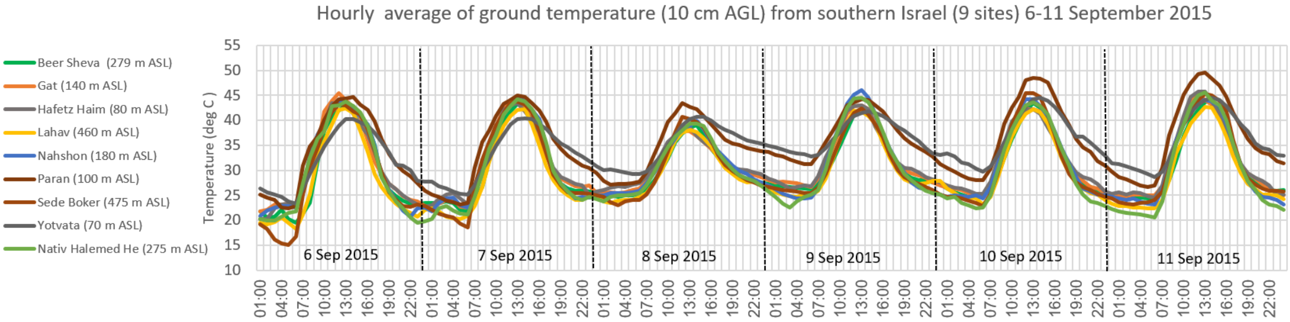


Fig. X7 Ground temperature (10 cm AGL) from southern Israel



The Fig. X3-X7 reveal the ground temperature on 9 September reached the values measured on 6 September. Furthermore, comparing the global radiation (FIG. X1-X2) and ground temperature figures (Fig. X3-X7), we find in southern Israel (the region of the Sede-Boker AERONET site) the maximum global radiation on the 8 September was the highest among all sites (400- 600 W m-2) and barely changed on 9 September. Hence, we do believe the creation of thermals was possible on the 9 September and the thermal creation on the 8 September was rather weak. Thus, the WCT method (seeking the derivative which is the peak value) on the 8 September was probably a result of subsidence of the dust plume as suggested by the referee.

Author's changes in manuscript: If the referees agree with our findings, we will add the aforementioned data to the text and edit the plots according to the authors' submission instructions.

Referees Comments:

Referee #2: *P7, L183: Now a critical issue: I checked the Kotthaus paper. According to this paper 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.
Author's response: Comments accepted.
Author's changes in manuscript: The plots present signal counts. The term "backscatter coefficient" was deleted from the text. We apologize for the mistake. Thank you for the detailed explanation.

Instruments- Radiosonde: lines 197-213

Referees Comments:
Referee #1: *"...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?*
Referee #2: *P7, L198: Please remove the trivial statement about the radiosonde ascents. Please provide just information on radiosonde type and company, and meteorological parameters measured.*
lines 208 ff: Move all results to Sect. 3.
Author's response: Comments accepted.
Author's changes in manuscript: The discussion and figures of the radiosondes profiles were moved to the "Results" section. An elaborated discussion on the radiosondes profiles was added. The radiosonde is a Vaisala type RS41-SG producing profiles of humidity, temperature, pressure, and wind measurement.

Instruments- Particulate matter measurements: lines 221-247:

Referees Comments:
Referee #1: *Note, that the titles of the subsections are inconsistent: "ceilometers" are instruments, but "PM10" is not.*
Author's response: Comment accepted.
Author's changes in manuscript: The title of Sect. 2.3 was changed from "PM10" to " Particulate matter monitoring". In the text, PM10 was mentioned as the type of measurement and not as the name of the instrument itself.

Referees Comments:
Referee #1:
line 223: Give the manufacturer (and type) of the instruments.
Author's response: The particulate matter measurements were taken from the air monitoring network directed by the Israel ministry for environmental protection. Unfortunately, the ministry does not publicize in their internet site the type of instrument

they use. Nevertheless, all monitoring stations were granted ISO17025 meaning the calibration method of measurement is approved by European standards. Based on private communication with the ministry, the manufacturer of the majority of particulate matter instruments is Thermo Fisher Scientific. Two main types are used: FH 62 C14 (beta attenuation method) and 1405 TEOM (tapered element oscillating microbalance method).
Author's changes in manuscript: The instrument type and manufacturer were added to the text.

Referees Comments:

Referee #1:

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.

Author's response: The PM10 continuous measurements are 5 min average based on instantaneous measurements of 1 min. Data is automatically defined "invalid" if among the average based used (e.g.,30 min, 1 hr) more than 25% of the data is missing. Therefore, the availability of the PM10 concentrations we have presented was possible to evaluate. In the other instruments (AERONET, ceilometers), there isn't a defined method to objectively evaluate the availability of the data, thus it was not stated. Overall, the ceilometers data (not referring to the quality of the data along the profile) was 100% but for Weizmann (Rehovot) site with 6 hours missing on the 6 September (shown in the figure) due to a local power failure. The AERONET sites availability is dependent on the daylight hours and the degree of AOD (at high AODs the instruments shuts down). Fig. 6 was an attempt to present the different availability of the AODs as the extent of available data changed from day to day.
Author's changes in manuscript: We added comments referring to the availability of the data from each instrument but avoided presenting absolute values.

Referees Comments:

Referee #1:

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. Figs. 5a … also belong to the results.

Referee #2: *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.*

Author's response: Following the comments we decided to present the data from specific monitoring stations (29) rather than an average of a set stations representing a region. A set of draft figures is given (Fig. X8-X11). If the referees approve, these plots will be organized according to editing instructions.

Fig. X8 PM10 measurements in the mountain region of Israel between 7-10 September 2015. The list of monitoring stations and their measuring height is given in the legend. The area of monitoring is denoted in the map on the left by a dashed black circle.

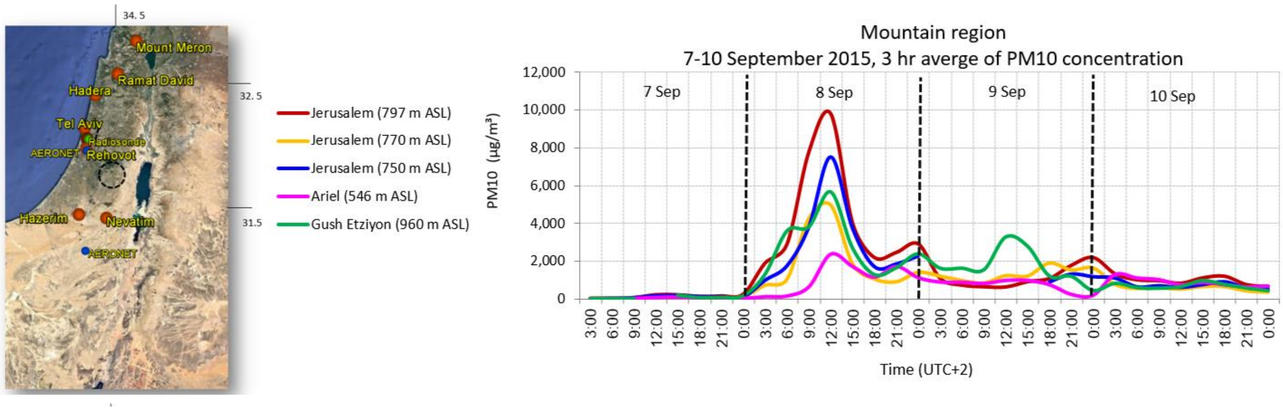


Fig. X9 PM10 measurements in the northern region of Israel between 7-10 September 2015. The list of monitoring stations and their measuring height is given in the legend. The area of monitoring is denoted in the map on the left by a dashed black circle.

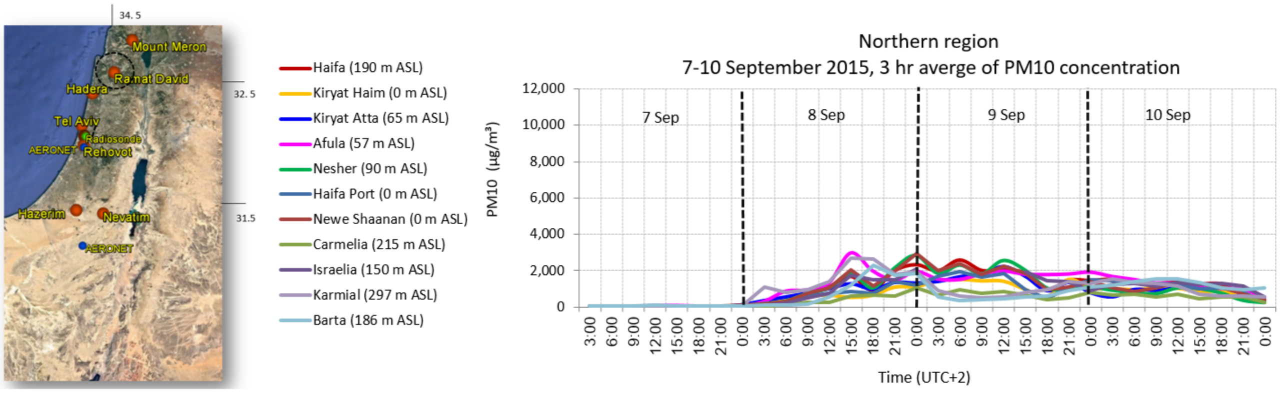
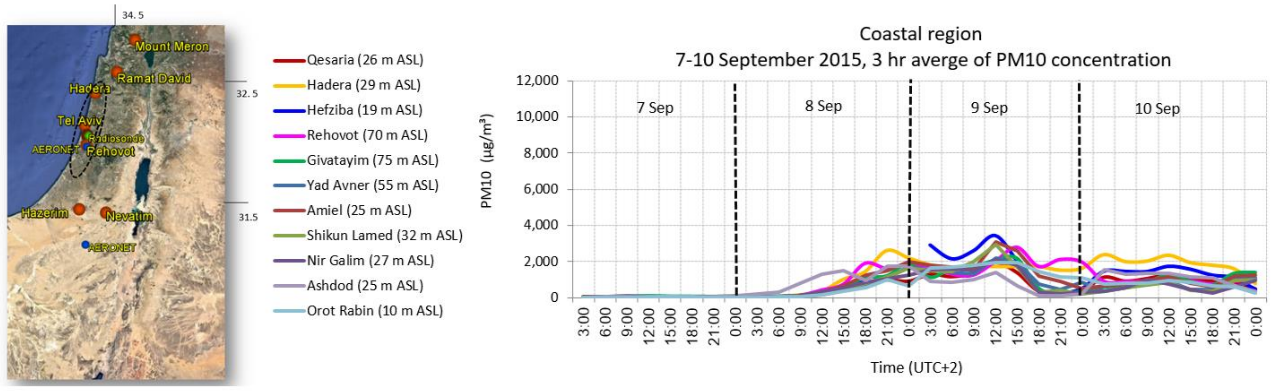
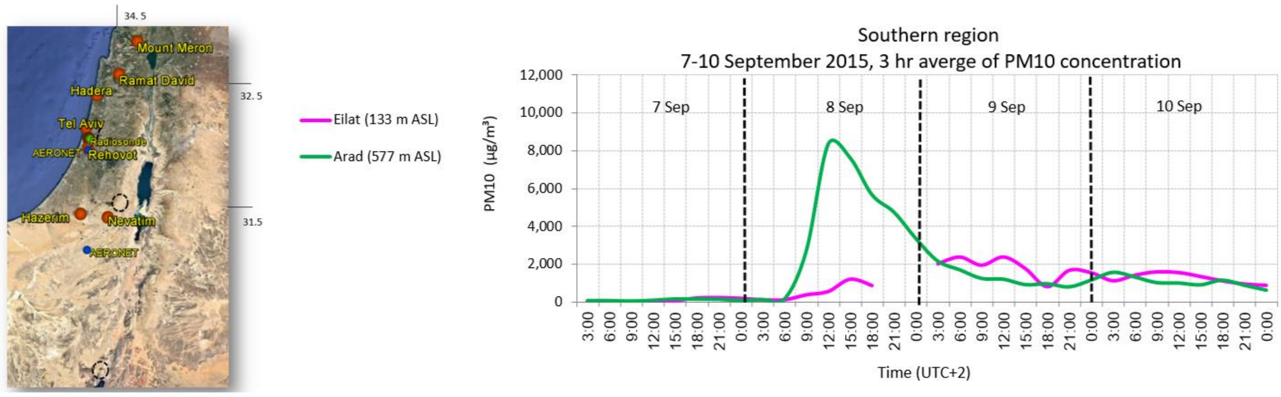


Fig. X10 PM10 measurements in the coastal region of Israel between 7-10 September 2015. The list of monitoring stations and their measuring height is given in the legend. The area of monitoring is denoted in the map on the left by a dashed black circle.



<u>Fig. X11</u> PM10 measurements in the southern region of Israel between 7-10 September 2015. The list of monitoring stations and their measuring height is given in the legend. The area of monitoring is denoted in the map on the left by a dashed black circle.



Author's changes in manuscript: New PM10 measurement plots from 29 monitoring stations were moved to the "Results" section.

Referees Comments:

<u>Referee #2:</u> *I asked colleagues from Israel to help me with... the following..., I am not familiar with Hebrew language... for example: http://www.svivaaqm.net/. Technically, you have at http://www.svivaaqm.net/ 'reports from numerous sites'... The dust storm: at September 7, around 22:00 values above 100 are already recorded for two sites (cities): Kiriat Ata and Nesher (next to Haifa). Haifa region (Northern part of Israel) must be included for more comprehensive analyses. The authors stated that: "Unfortunately, there are no PM10 measurements in 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.*

Author's response: All PM10 measurements, including the Haifa region, were included in our calculations (Fig . X9.) Nevertheless, these stations do not cover what we define as northest region in Israel (denoted by a yellow circle in Fig. X12) named "Golan heights" (also referred by Gasch et al.).

<u>Fig. X12</u> The northern region (yellow circle) where no PM10 measurements are held.

Measurements in the Golan heights would have given us a perspective whether the dust plume prevalence over Israel on 8 September 2015, was indeed from the northeast or rather unified but measured mainly by the highest monitoring stations since the dust plume hadn't thus far subsided.

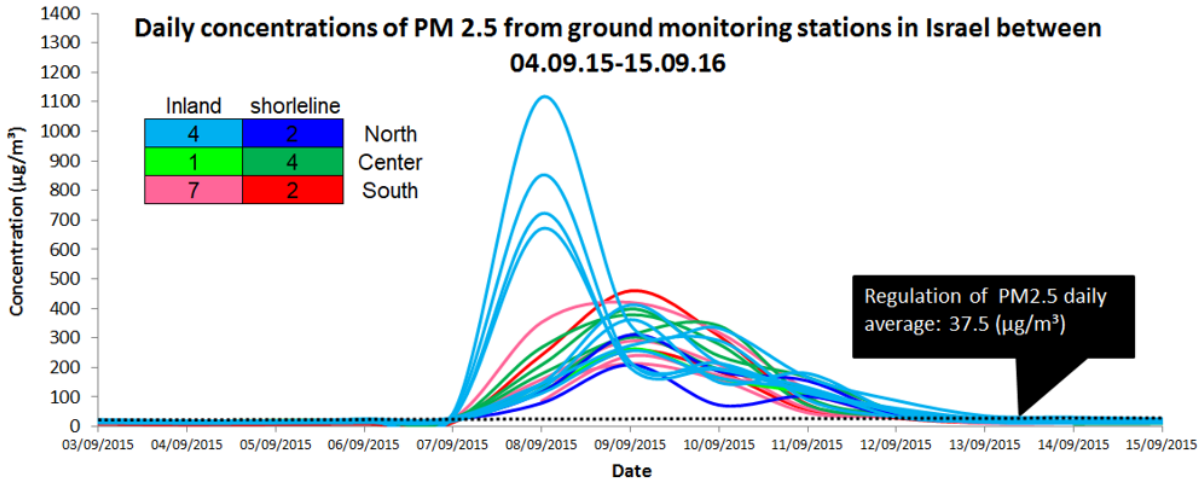
Author's changes in manuscript: New plots presenting 3-hr concentration of PM10 from 29 stations instead of an average concentration per region.

Referees Comments:

Referee #2: *Why did the authors not include PM2.5 sites data.*

Author's response: We did not include the PM2.5 measurements although we've already analyzed them) since the intensity of the dust storm was less profound in PM2.5 measurements (see Fig. X13) and less monitoring stations were available compared to PM10 measurements. We decided PM10 would be sufficient.

Fig. X13 An example of daily PM2.5 concentrations between 4-15 September 2015 from of 20 monitoring stations with available data during the dust event. The monitoring stations were separated to 6 regions by the combination of north/south/center/inland/shoreline. The measurements of the stations were denoted by the colors of the different regions (see legend). The amount of stations in each region is given inside the legend.



Author's changes in manuscript: We can add a figure of PM2.5 measurements in a format preferred by the referee (e.g., parallel to the PM10 measurements). Fig. X12 is just an example.

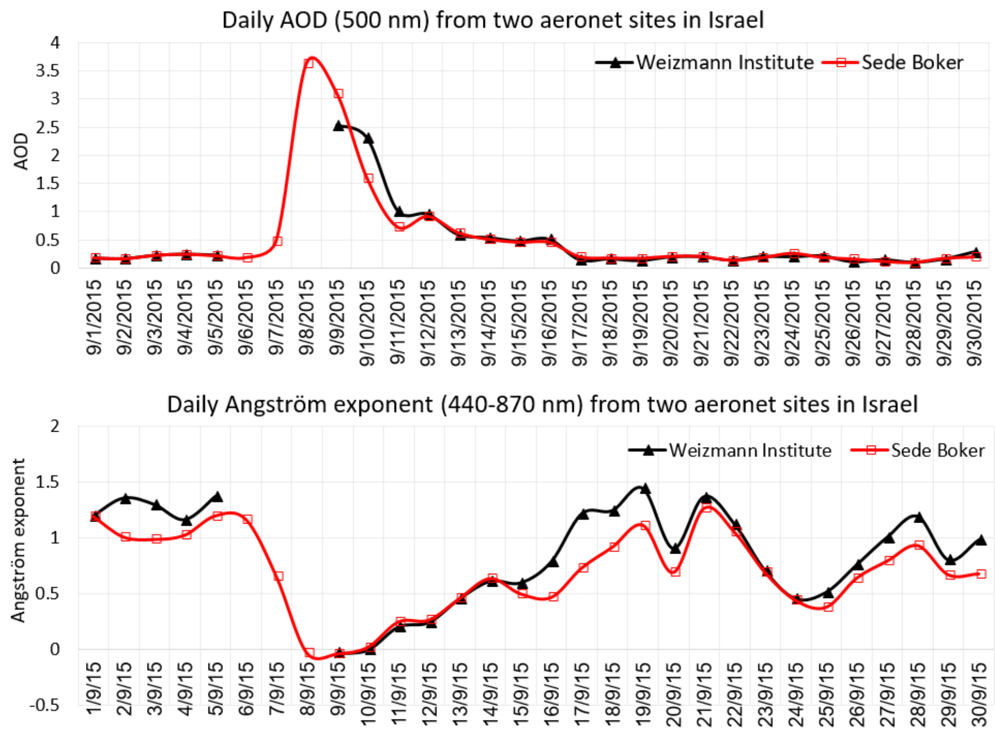
Instruments- AERONET : lines 254-273

Referees Comments:
Referee #1: *"...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.*
Author's response: Comment accepted.
Author's changes in manuscript: The sentence was omitted.

Referees Comments:
Referee #1: *Figs.. 6 also belong to the results.*
line 262: As this is a result it should be moved to Sect. 3. Fig. 6: to be moved to Sect. 3.
Author's response: Comments accepted.
Author's changes in manuscript: AERONET figure was moved to the "Results" section.

Referees Comments:
Referee #1: *I don't understand the x-axis of Fig.6 ? 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)?*
Referee #2: *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?*
Author's response: All the data available from the Israeli AERONET sites was delineated in Fig. 6. The length of the days is different due to missing data. Fig. 6 was an attempt to present the availability of the AODs as the extent of available data changed from day to day. We found the presentation of Fig. 6 more informative than a line plot.

<u>Fig. X14</u> Daily AOD and Angstöm coefficient from two AERONET sites in Israel



Author's changes in manuscript: we changed the plot to daily average (Fig. X14).

Referees Comments:

<u>Referee #2:</u> *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...*

Author's response: The instrument was out of order the due to power failure in the Weizmann institute.

Author's changes in manuscript: We mentioned the instrument was out of order and did not automatically shut down as a result of high AODs.

<u>Results: lines 276-472</u>

Referees Comments:

<u>Referee #1:</u> *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). 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.

Author's response: Comments accepted.

Author's changes in manuscript: The "Results" paragraph was rewritten according to the overall comments of the referees.

Referees Comments:

Referee #1: *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?*

Author's response: Annual mean of 2015 of AOD from Sede-Boker : 0.22 . AERONET Weizmann started operating on 6 June 2015, thus the half year mean is: 0.25. Although Sede Boker is situated in the southern Negev desert of Israel, the Average AOD of the "dusty and windy" season on Oct- Nov remained 0.25. Therefore, the AODs above 3 are rare. Between 11-14 September 2015, AODs are large although the PM10 measurements and ceilometer signals infer differently since they do not include the whole air column. This finding may justify the results of Stavros et al. (2016) and the CALIPSO measurements on the 10 September of an elevated layer at about 3-4 km of dust plume.

Author's changes in manuscript: We added a discussion on the comparison between measurements (AERONET, particulate matter, global radiation, ceilometers, Satellite pictures) after 11 September 2015.

Referees Comments:

Referee #1: line 281 ff: Move lines 281–283 to Sect. 2.

Author's response: Comment accepted.

Author's changes in manuscript: The sentences were moved to the ceilometer discussion in the "Instruments" section.

Referees Comments:

Referee #1: *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 m−1 sr−1) are confusing. lines 281–283: Check carefully the following numbers of the attenuated backscatter; some of them are unrealistic (7·10−1 m−1 sr−1).*

lines 281–283: 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"?

Referee #2: *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. 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 m**2'. So the values are not unitless, but usually given in arbitrary units. Next, by dividing these data by 10ˆ(-9) ... does not change anything. You still have just range-corrected signals. You 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!*

Author's response: A fundamental mistake. The attenuated backscatter presented in Fig. 7 had already been divided by 10^{9}, therefore, 10^{-9} should have been omitted from the x-axis label. The numbers shown in the ceilometer plots are signal counts.

Author's changes in manuscript: Ceilometer profiles were correct to units of attenuated backscatter in the order of 10^{-5} m^{-1} sr^{-1}. A title of "counts" was added to the legend in all ceilometer daily plots.

Referees Comments:

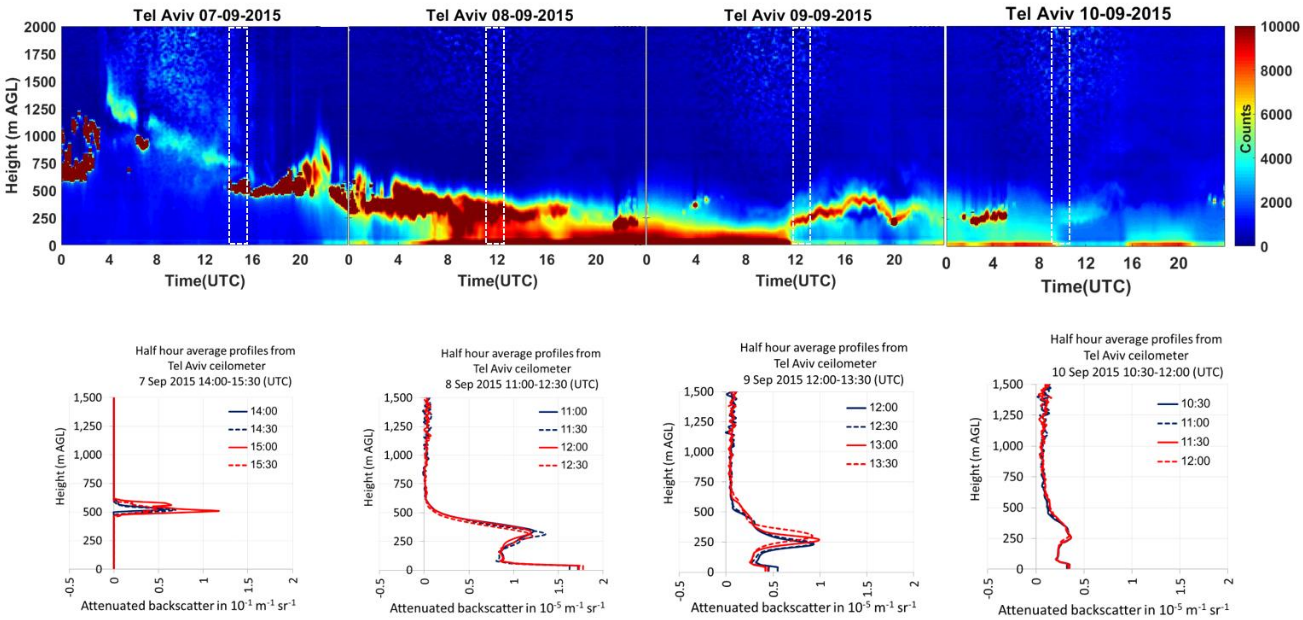
Referee #1: *The authors should be aware that a comparison of brown vs. brown color or brown vs. blue color (Figs. 10–16) is not suitable for a scientific publication. Please use quantitative numbers! Think about plotting coincident attenuated backscatter profiles of the 8 sites (similar to Fig. 7), this may help to see temporal delays in the arrival and decay of the plume.*

Referee #2: *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.*

line 320: "the time corrected from local time to" can be removed.

Author's response: After several trials we found the current plot contours are the most informative. Therefore, we took your advice and added profiles (as in Fig. 7) for each of the ceilometer plots at 4 times of interest: the dust storm penetration (7.9.17, 14:00-15:30 UTC), downward dust transport towards ground level (8.9.17, 11:00-12:30 UTC), dust ascent (9.9.17, 12:00-13 UTC) and the time of CALIPSO passage over Israel (10.9.17, 10:30-12:00 UTC) (Fig. X15).

<u>Fig. X14</u> Tel Aviv ceilometer daily plots (top panel) and attenuated backscatter signals (bottom panel) at time of the dust storm penetration to Israel (7 Sep 2015), downward dust transport (8 Sept 2015), dust ascent (9 Sept 2015) and time of CALIPSO passage over Israel (10 Sept 2015). The period of the profiles is denoted by white dashed lines upon the ceilometer plots.



Author's changes in manuscript: Updated the ceilometer plots as Fig.X14.

Referees Comments:

<u>Referee #2:</u> *P13, L324: again and again: you were able to track the dust layer base only, this must be clearly said. In Figure 8, the numbers for 'your' attenuated backscatter are suddenly up to 15000, compared to values of about 10ˆ(-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.*

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.*

Author's response: 6 Out of the 8 of the ceilometers belong to a governmental office. We were not informed whether the "message profile noise h2" was off or on therefore whether an automated range correction was done.

Author's changes in manuscript: No changes.

Referees Comments:

Referee #1: *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.*

Referee #2: *P13, L330: A visibility of 200m (visual meteorological range is defined by an AOD of 3, after Koschmieder for an AOD of 4) according to an AOD of 3 means that the particle extinction coefficient was 15 km-1 or 0.015 m-1 and the backscatter coefficient is then 0.0003 m-1 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!*

Author's response: We took the visibility observations under 100 m AGL and compared to the attenuated backscatter received at ~500 m AGL. We understand it is misleading and therefore we decided to omit this paragraph.

Author's changes in manuscript: The paragraph was deleted.

Referees Comments:

Referee #1: *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.*

Referee #2: *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.*

Author's response: We intended to explain that we do not know the detection limit of the attenuated backscatter signals at high AODs. Therefore, we cannot put a direct limit to the ceilometer threshold. Instead we tried to use other methods to verify the first layer height. In this subsection we attempted to retrieve the inversion height from the ceilometers and radiosonde. Since aerosols are usually trapped under the inversion height and this height is similar in the radiosonde and WCT calculation as a tool to define the height of the first dust layer.

Author's changes in manuscript: We rephrased the sentences.

Referees Comments:

<u>Referee #2:</u> *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.*

Author's response: Comment accepted.

Author's changes in manuscript: We have listed the ceilometers limits in the Instruments section and referred to these limits in the results.

Referees Comments:

<u>Referee #2:</u> *P13, Fig. 9 shows the dust base height. To my opining 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.*

Author's response: We accept the referee's opinion on the 8 September since the global radiation (Fig. X2) was marginal for a significant thermal creation. However the trend of the wind speed in Beit Dagan, Jerusalem and Haifa sites (Fig. X16 a-c) indicates the intrusion of the sea breeze had occurred (Uzan et al., 2012, Fig. X16,d). Wind speed measurements of 8 September indicate lower values and a delay in the wind speed intensification. As stated in the paper, we assume the change in wind speed is owed to the low radiation (below 300 W m^{-2} in the three sites mentioned above, Fig. X2). Therefore, the thermals were not significant in the formation of the mixed layer height.

Author's changes in manuscript: The "Results" paragraph was rewritten according to the overall comments of the referees.

<u>Fig. X16</u>

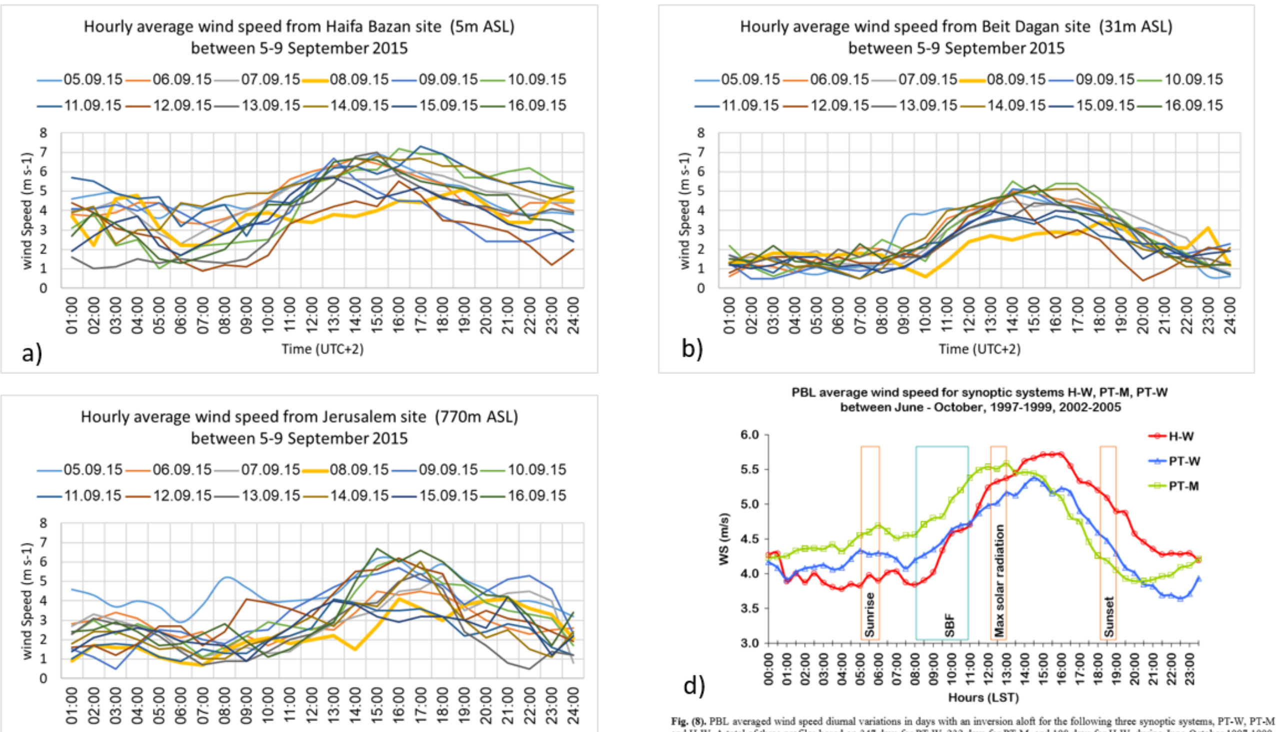


Fig. (8). PBL averaged wind speed diurnal variations in days with an inversion aloft for the following three synoptic systems, PT-W, PT-M and H-W. A total of three profiles based on 347 days for PT-W, 232 days for PT-M, and 198 days for H-W, during June-October 1997-1999, 2002-2005. The profiles were measured by the LAP-3000 profiler (Sec. 2). Also indicated are the time period of sunrise and sunset, the period of maximum solar radiation and the SBF times of entrance and weakening. See text for the method of calculation.

Referees Comments:
Referee #1: *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.*
Author's response: Comments accepted.
Author's changes in manuscript: We changed the results section and described the dust storm evolution along it's generation rather than by dates.

Referees Comments:
Referee #2: *P15, L383: Please clearly state where the dust layer top was found by Solomos et al. (2017).*
Author's response: Stavros et al (not Solomos, my mistake) analyzed by CALIPSO overpass two layers, 2 km (total attenuation was reported up to 1.5 km) and between 3-4 km.
Author's changes in manuscript: The overview of Stavros et al. was extended.

Referees Comments:
Referee #2: *"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?*
Author's response: *7* September 2015.
Author's changes in manuscript: The "Results" paragraph was rewritten according to the overall comments of the referees.

Referees Comments:
Referee #1: *line 401: What is meant by "decrease"? Concentration or altitude?*
Author's response: We meant the subsidence of the dust plume.
Author's changes in manuscript: The "Results " paragraph was rewritten according to the overall comments of the referees.

Referees Comments:
Referee #1: 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?

Author's response: We based our conclusions on the wind direction profiles from radiosonde launches in Beit Dagan. Apart the radiosonde, we do not have auxiliary measurements to prove our assumption. Therefore, if the referee could not relate to our conclusions, we decided to omit this hypothesis.
Author's changes in manuscript: The sentence was erased.

Referees Comments:
Referee #1: *line 408: Please clarify what" these model findings" are? The vertical distribution of dust (backscatter)?*
Author's response: We were referring to the hydraulic jump upstream the dead sea rift valley explained by Gasch et al., as the dust plume entered from east and progressed southwest. On the other, the ceilometers plots revealed the penetration of the dust plume occurred straightaway from north (Ramat David site) to south (Hazerim and Nevatim sites) and to west (Hadera, Tel Aviv, Beit Dagan and Weizmann sites).
Author's changes in manuscript: we revised the discussion of the "Results" section and the comparison to previous studies.

Referees Comments:
Referee #1: *line 422: 2000 µg/m3: is this in contradiction to 9800 µg/m3 in line 416*?
Author's response: We were referring to the second "jump" in PM10 measurements which occurred simultaneously in all monitoring stations on the 8 September at~17:00 (UTC+2) after the extreme values of 9800 µg/m3 were measured only in the high altitude stations on the 8 September at ~ 12:00 (UTC +2).
Author's changes in manuscript: We rephrased the sentence in the scope of the "Results" section.

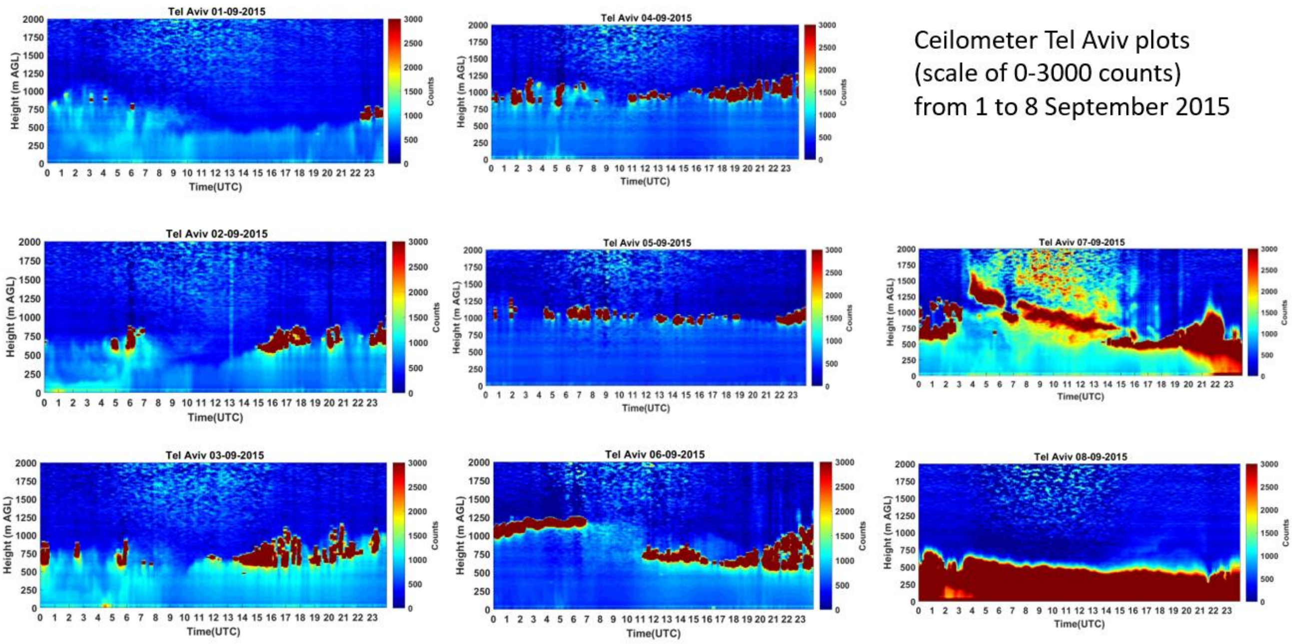
Referees Comments:
Referee #1: *line 437: "...limited radiative transmitted...": what does this mean?*
Author's response: We meant the decrease in the global solar radiation.
Author's changes in manuscript: The sentence was corrected and updated with additional data of global radiation measurements in Israel (23 sites across Israel) focusing on the 8 and 9 September 2015.

Referees Comments:
Referee #2: *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 differential sea/land heating? Please keep the discussion free of speculation.*

Author's response: On the 8 September, the maximum wind speed was below 3m/s and maximum global solar radiation up to 264 W m-2. The ability to generation of thermals under these would be rather weak. on the next day, on the 9 of September the maximum global radiation more than doubled reaching 621 (w m-2) and wind speed increased to a maximum of 5 m s-1 at 13:00 (UTC+2). At these conditions, convection and creation of thermals is possible.
Author's changes in manuscript: We've added an explanation and figures regarding the possibility of thermals creation on the 9 September.

Referees Comments:
Referee #1: *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 inter-comparison 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*.
Referee #2: *P17.... Figure17 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).*
Author's response: Comments accepted.
Author's changes in manuscript: We added an underlined the explanation of the different signal counts (up to 3000) of figure 17 in the manuscript compared to the rest of the ceilometers plots (up to 15,000) and daily AOD measurements from both AERONET sites in Israel, Sede-Boker and Weizmann, for the whole month of September 2015 (Fig. X13). For a comparison of a "typical background", we created ceilometer Tel Aviv plots for 1-8 September based on a scale of 3000 counts (Fig. X17). If the referees find the plots contribute to the paper, we will delineate similar plots for the rest of the ceilometers.

Fig. X17



Referees Comments:

Referee #2: *P17, L444: You state: The ceilometer reveals 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, 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, L498-499: When were the AE values high again? They were continuously <0.5 even on 14 Sep (Weizmann AERONET).

Author's response: The "clearance" we mentioned was referred to the lower part of the atmosphere (up to ~1 km), as the PM10 values decreased considerably from the 11 September (Fig X18). By 11 September the amount of signal counts from all 8 ceilometers declined (Fig. X19 for example). The possibility of a profound decrease in the ceilometer signal counts while the AE is still high is owed to the fact that AERONET measures the whole atmospheric column, including the second dust layer indicated by CALIPSO (Fig. X) and Mamouri et al., (2016) to be at ~ 2-4.5 km. The ceilometer on the other hand was capable to detect only the first km. This comparison may point out residual of dust plume aloft even at 15 September, in contrary to MODIS –aqua imagery (Mamouri et al., 2016) and AOD<1 from MSG SEVIRI (Fig. X20).

Author's changes in manuscript: We have added a discussion on the dust ascent in the form mentioned above.

Fig. X18

The mixed layer height based on 00Z and 12Z profiles from radiosonde Beit-Dagan vs PM_{10} daily average concentration from the Ceilometer Hadera site

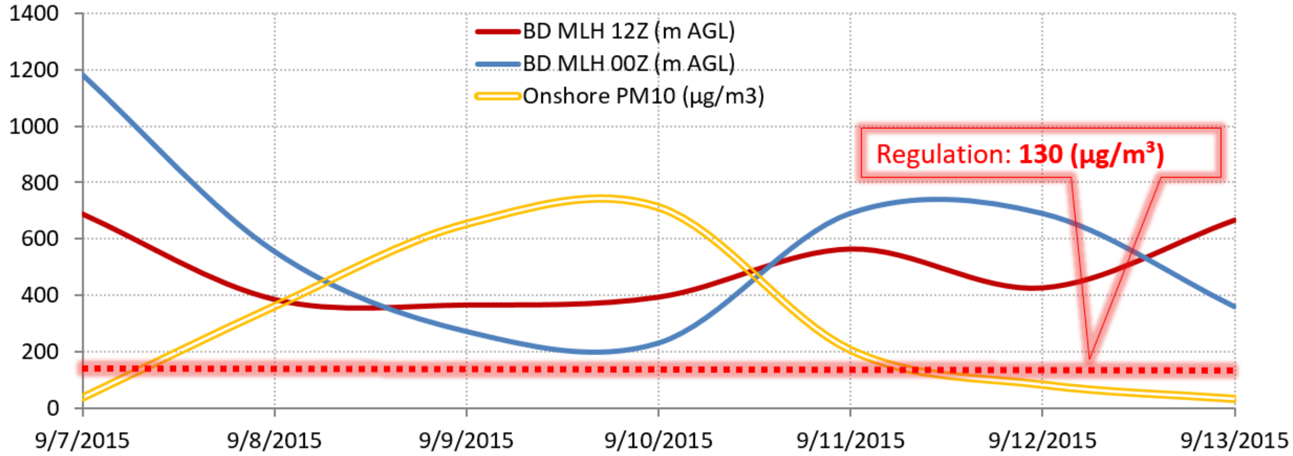


Fig. X19 Daily attenuated backscatter plots from ceilometer CL51 Weizmann-Rehovot (central Israel). Y-axis : Height (m AGL), X-xais: Time (UTC) , Scale : 0-15,000 signal counts. Ceilometer profiles were averaged by 15 minutes running average for better SNR.

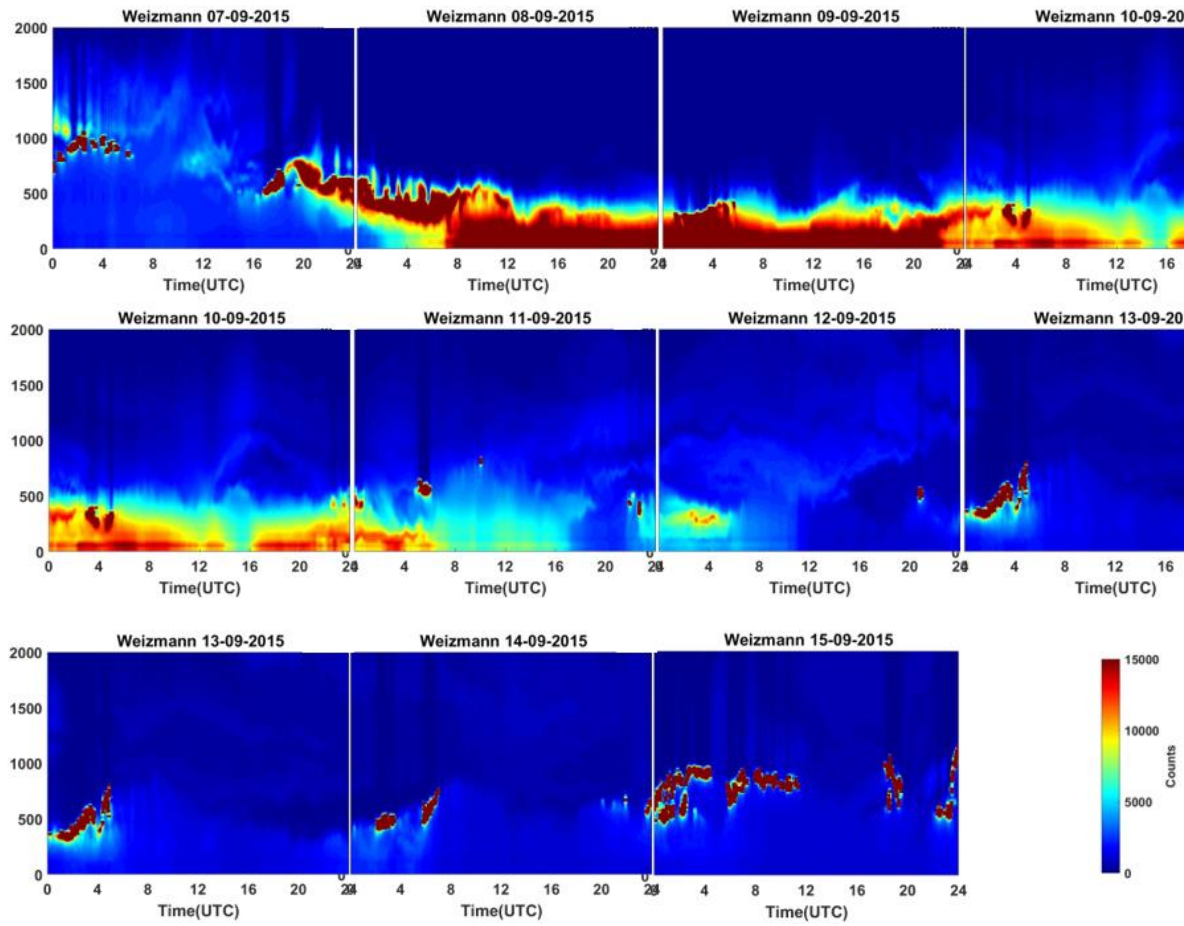
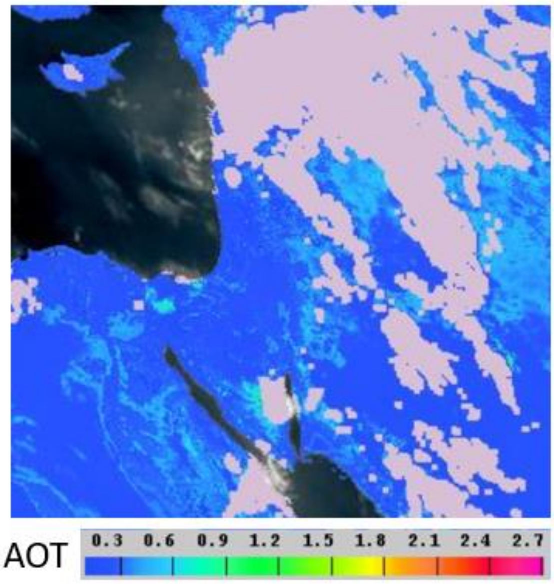


Fig. X20 NAScube optical thickness based on MSG SEVIRI from 15 September 2015 (source: http://nascube.univ-lille1.fr/cgi-bin/NAS3_v2.cgi)



Referees Comments:

Referee #1: *line 454: 250 m: is this the vertical extent or the altitude?*

Referee #2: *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.*

Author's response: Comments accepted.

Author's changes in manuscript: Considering the ceilometers limitations to detect attenuated backscatter signals above a dense dust layer, we omitted the assumptions regarding the vertical extent of the dust layer.

Referees Comments:

Referee #1: *line 486: The PM10 measurements are considered as in-situ measurements, not remote sensing.*

Author's response: Comments accepted.

Author's changes in manuscript: We corrected the sentence referring to PM10 measurements as in situ.

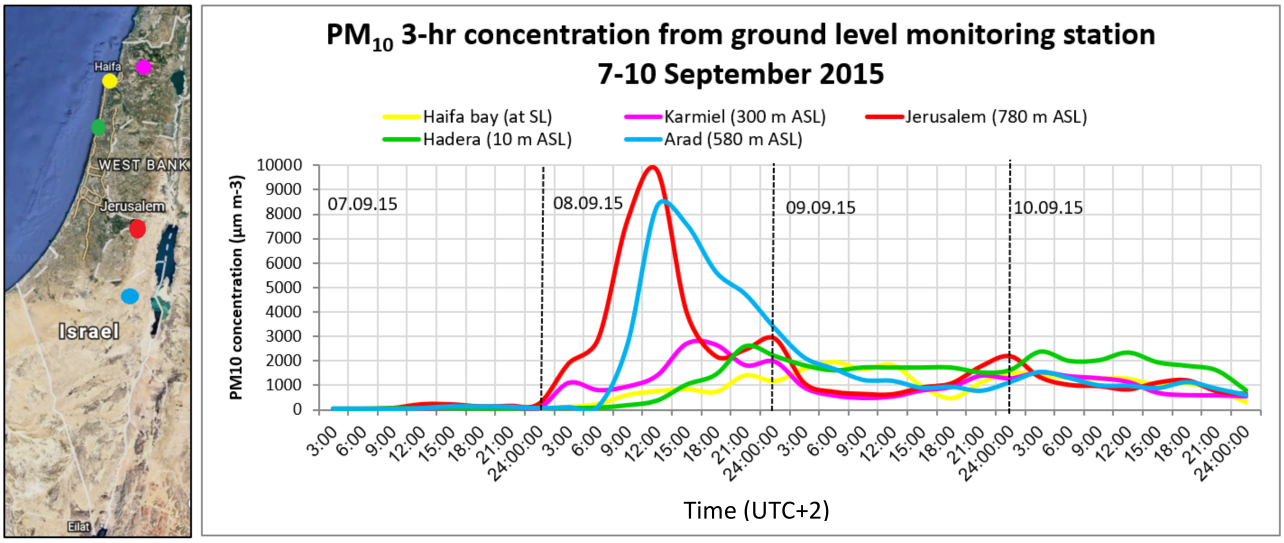
Conclusions and discussion: lines 480-514

Referees Comments:
Referee #2: P19: The conclusions have to be rewritten completely after improving all the text before along the lines this review and the other review.
Author's response: Comment accepted.
Author's changes in manuscript: We rephrased the "Conclusions and discussion" section according to the referees' comments.

Referees Comments:
Referee #2: 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.
Author's response: Comments accepted.
Author's changes in manuscript: We listed the ceilometers limitations in the Instruments sections. In the discussion section we referred to these limitations in the as part of the process of evaluation.

Referees Comments:
Referee #2: *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/m3 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? 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*
Author's response: We analyzed PM10 and PM2.5 measurements from all available monitoring stations in Israel. Unfortunately, we did not recognize a second jump from northern region compared to the rest of the country. To emphasize our conclusions, we prepared Fig. X21 presenting PM10 3 hr concentrations, including monitoring stations the referee mentioned: Haifa Bay, and the northest station in Israel (Karmiel). We added three more stations from the central shoreline (Hadera), eastern Israel (Jerusalem) and southeast (Arad). To our opinion, it is difficult to declare a second "jump" on the 8 September exclusively in the northern sites.

Fig. X21



We estimate the dust plume penetrated Israel was disclosed firstly by the global radiation measurements (Fig. X2). After the dust subsided (based on personal knowledge in environmental dispersion models, the physics behind the mathematical assumptions treat PM10 by the characteristics of gaseous dispersion), PM10 measurements delineated the dust plume dispersion on ground level (Fig. X22). On the other hand, TSP (Total suspended particles, larger particles than PM10 but below 45 µm aerodynamic diameter) maximum concentrations were measured on the night between 8-9 September (Fig. X23). This may indicate of local meteorological conditions generating resuspension not rather a consequence of the dust plume descent.

<u>Fig. X22</u> PM10 maximum concentration 8 September 2015 measured across Israel in 9 sites. Indication of height of each measuring site (ASL) is given upon the map.

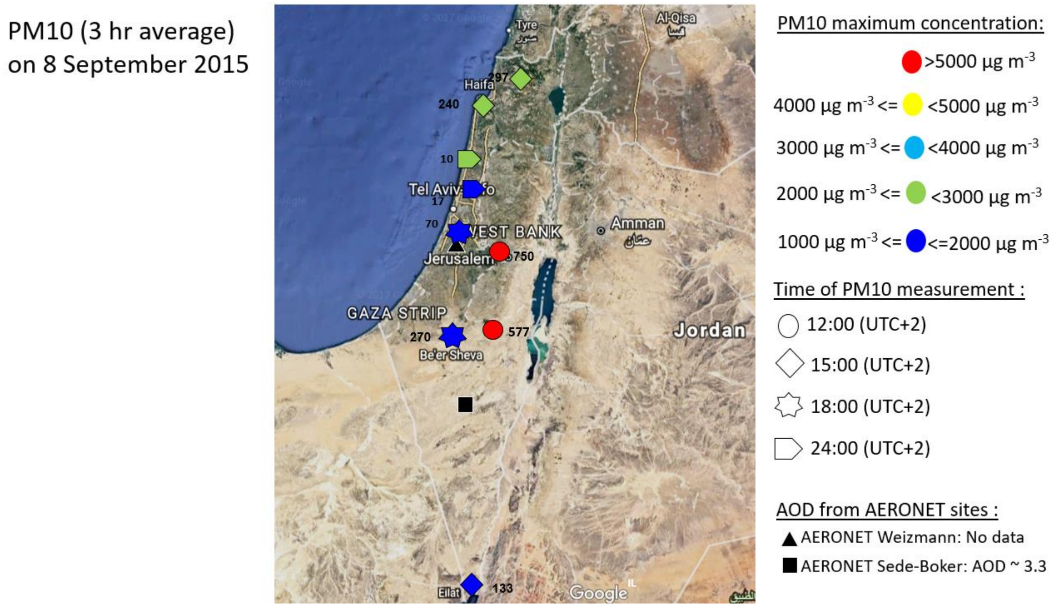
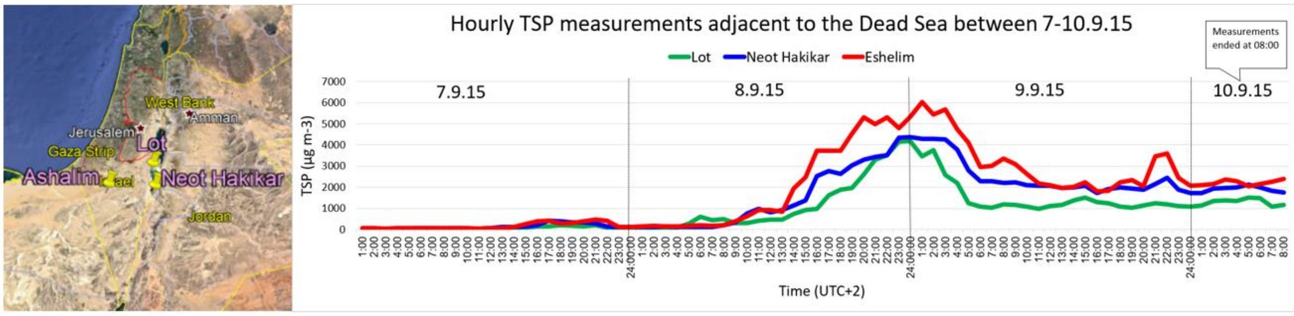


Fig. X23 TSP measurements from three sites adjacent to the Dead sea



Author's changes in manuscript: We suggest to exclude Fig. 18. Instead we my insert the discussion disclosed above with auxiliary data presenting the global, direct and diffused radiation from several sites cross Israel (Fig. X25-X30):

Fig. 25 Global, direct and diffused radiation from the northest site

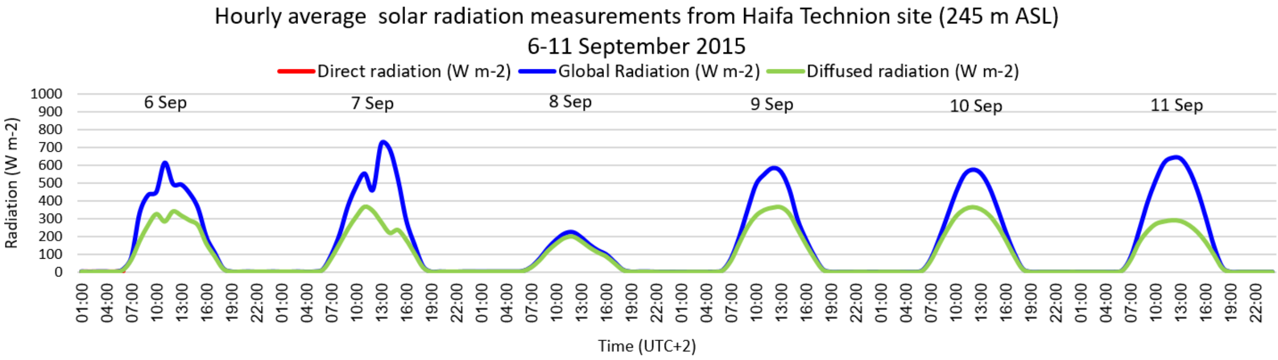


Fig. 26 Global, direct and diffused radiation adjacent to ceilometer Beit Dagan

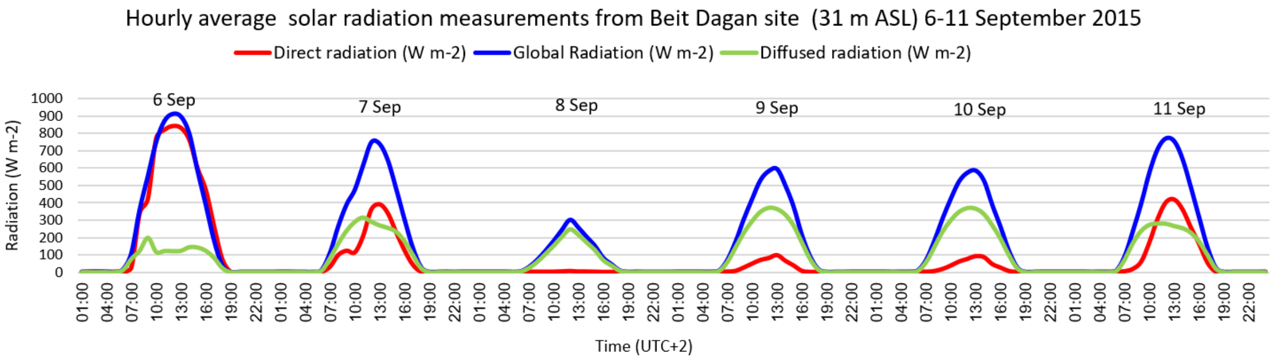


Fig. 27 Global, direct and diffused radiation from the highest measuring point

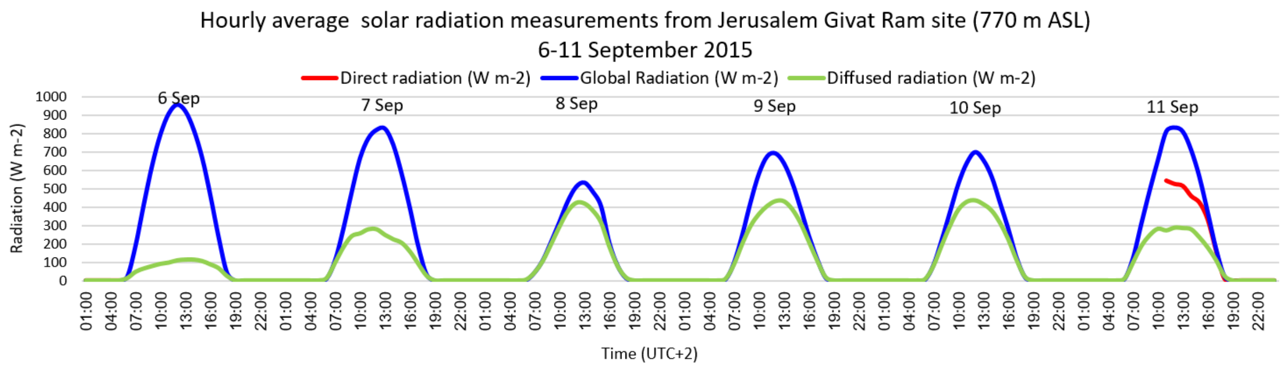


Fig. 28 Global, direct and diffused radiation from southern Israel

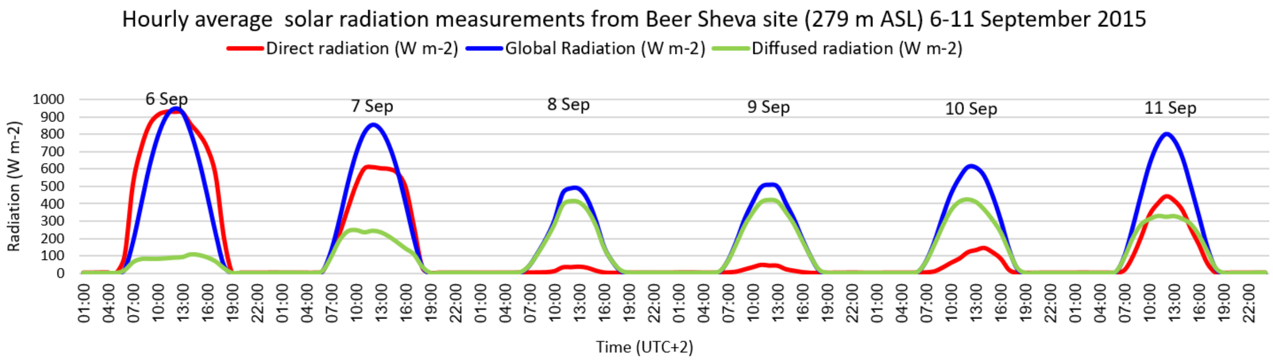
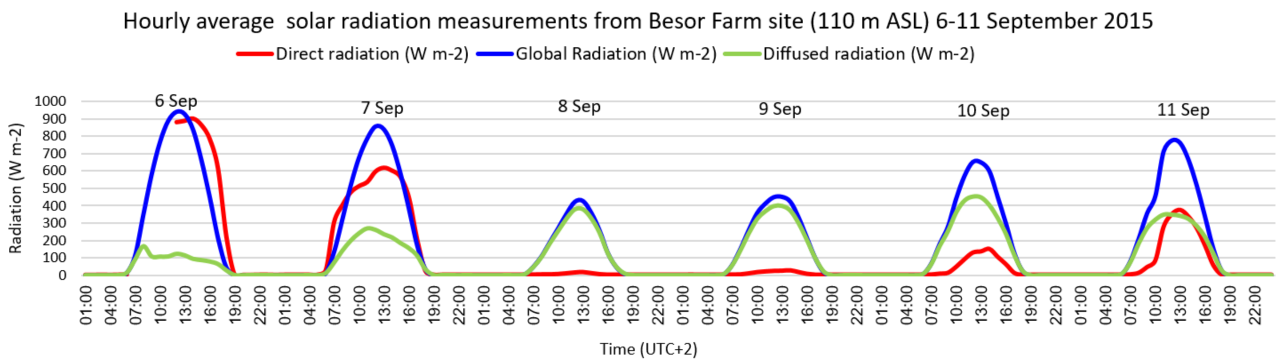
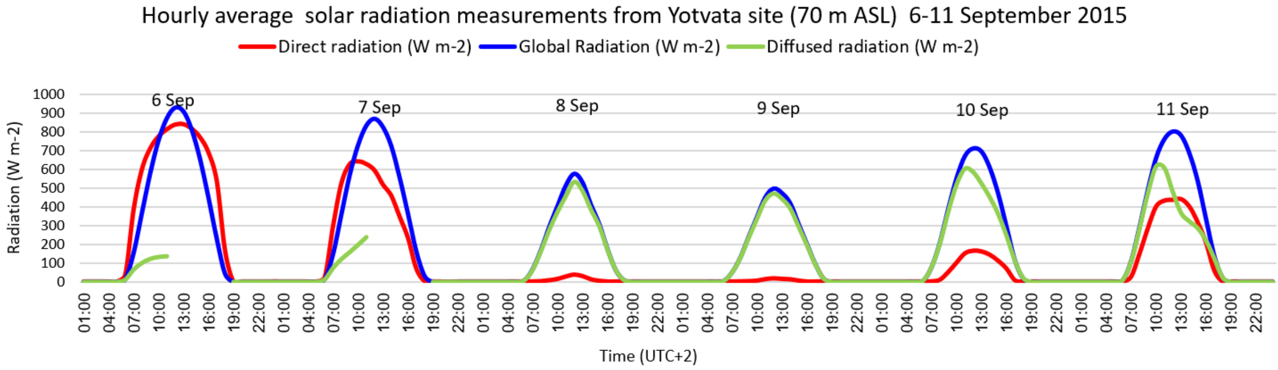


Fig. 29 Global, direct and diffused radiation from southern Israel



<u>Fig. 30</u> Global, direct and diffused radiation from southern Israel



Referees Comments:
<u>Referee #1:</u> *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).*
<u>Referee #2:</u> *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.*
Author's response: Comments accepted.
Author's changes in manuscript: We rephrased the sentence and emphasized the contribution of the ceilometer measurements to the analysis of the lower part of the atmosphere (from ground level up to ~1 km) as a completion to previous studies concentrating on the generation and propagation of the dust plume down to ~1.5 km.

Referees Comments:
<u>Referee #1:</u> lines 492, 497:" plume"!
Author's response: Comment accepted.
Author's changes in manuscript: Typing mistakes were corrected.

Referees Comments:

Referee #1: *line 494:" mainly of mineral dust": where is this information coming from?*

Author's response: We based our conclusions on Sede-Boker AERONET Angstöm coefficient measurements (Fig. X13)**.** Mamouri et al., (2016) studied the dust layer particle linear depolarization by an EARLINET lidar stationed in Limassol Cyprus. They concluded the linear depolarization ratio of 0.25-0.32 on 7 and 10 September, indicated the dominance of mineral dust (the lidar was inoperative on the 8 September).

Author's changes in manuscript: We added the citation to the AERONET Angstöm coefficient measurements in Israel and referred to the conclusions from Mamouri et al., (2016).

Referees Comments:

Referee #2: *P19, L494: As a result, ... of what?*

Author's response: As a result of the low boundary layer.

Author's changes in manuscript: We rephrased the "Conclusions and discussion" section according aforementioned overall comments of the referees.

Referees Comments:

Referee #2: *P19, L502-504: This is speculation, at least to my opinion. Be more save with your statements.*

Author's response: Comment accepted.

Author's changes in manuscript: The sentence was omitted and a comprehensive analysis of the meteorological measurements (global radiation, direct radiation, diffused radiation, ground temperature, wind speed) and environmental measurements (PM10, PM2.5, TSP) were added in the attempt to explain and reveal the meteorological conditions held as the dust storm prevailed in Israel.

Referees Comments:

Referee #2: 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.

Author's response: We accept the comment. Additional data from the CALIPSO passage over Israel on the 10 September (given here in Fig. X) indeed shows a dust plume between ~2.5-4.5 km.

Author's changes in manuscript: We omitted the statement and refereed to the ceilometers data in the context of the evolution of the dust plume at the lowest level of the troposphere. We stated the ceilometers' limited ability to detect attenuated backscatter signals above the dust layer detected at ~ 1 km.

References: lines 546-637

Referees Comments:
Referee #2: *P21, L554: No authors.*
Author's response: The reference is a report edited and distributed by the U.S Environmental protection Agency with no specific authors mentioned upon the report.
Author's changes in manuscript: No change.

Referees Comments:
Referee #2: *P23, L621: TOASJ...?*
Author's response: Acronym for The open atmospheric science journal.
Author's changes in manuscript: The acronym was converted to the full name of the journal.