

# ***Interactive comment on “Ceilometer evaluation of the East Mediterranean summer boundary layer height – first study of two Israeli sites” by L. Uzan et al.***

## **Anonymous Referee #1**

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The paper presents the evaluation of two ceilometers in Israel over two summer months: The scientific products include the mixing layer height and cloud cover. The retrieved mixing layer height is compared with independent measurements from radiosondes and calculations of weather forecast models. Moreover, correlations of ceilometer signals with PM10 are investigated. This large amount of scientific topics and results, and the fact that these ceilometer data seem to be the first from Israel makes the paper suitable for AMT.

However, before publication the paper must be streamlined and better structured. I recommend to clearly distinguish between the 'technical part' and 'scientific results'. This would help the reader and improve the scientific impact. Moreover, some of the

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conclusions are not as convincing as they could be. Finally, the language should be improved by a native speaker.

So I recommend to combine section 2, section 3 and the first part of section 4 to a new section summarizing all technical details of the instrument and the MLH-retrieval. I would not focus too much on the retrieval as there are several earlier studies dealing with this; in other words, the WCT can be considered as well established.

The second part of section 4 (the comparison with the model and the PM10-correlation) includes quite interesting aspects and should be discussed separately as part of the results. With respect to the model it should be clear that the ceilometer retrieval is used to validate the model (not the other way round, otherwise provide a stringent argumentation). Give references to all models. Give a brief description/reference on how WRF calculates the dust distribution and which vertical resolution is used. Is the model's limited resolution (10 km) sufficient to resolve differences at the two ceilometer sites? I don't see 'major' differences, but in general a very good agreement! What about the TAU model: are there results to be compared to the ceilometer measurements as well (this could be interesting)? Extend the discussion to have sufficient material for a separate section.

The 'PM10-part' is a new scientific topic and should be discussed in a dedicated section. I am not sure if one can trust the first range bin of a CL31 ceilometer, so I expect a critical discussion of this issue. To make this part more quantitative, correlations can be determined and the 20 m range bin can be investigated as well. How is the ceilometer calibrated, i.e., how is it possible to get 'backscatter coefficients' (?) in absolute units? Is this provided by the manufacturer or calibrated by the authors? The quantity 'backscatter intensity' should be explained as well: is it the 'aerosol backscatter coefficient' or 'attenuated backscatter'? Note, that water vapor absorption should be considered. If it is too early to get ultimate conclusions, the results should be labeled as 'preliminary' and further steps should be outlined.

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All scientific topics covered in the 'results'-section should be extended (more detailed discussion) and their value should be highlighted. The conclusions should include an outlook (identification of open questions, expected benefit of more data, etc.)

### Minor/technical comments

Some minor/technical suggestions are listed below (given are page and line number).

- 2–31: Beit Dagan: give the geographical coordinates (of all sites)
- 3–1: 'This research focuses...' → 'This paper focuses...'
- 3–3: Add a scale to Fig. 1.
- 3–5: 'sections 4, 5 and 6 present the methodology, results and conclusions.' This should be updated according to the re-organization of the paper and then should be somewhat more detailed.
- 3–15: 'tens of thousands of short pulses are emitted... of 2 s'. Give the pulse repetition frequency; 10 kHz according to Wiegner et al. (2014).
- 3–19: 'AGL': make sure that all acronyms are explained when they first appear.
- 3–20: 'a 15 -16s reporting time interval is sufficient'. What is the reason for this statement (with this temporal resolution the SNR is large enough to detected the MLH?)?
- 3–29: 'due to the transition': What is meant by this? transition of what? Moreover, Fig. 2 must be explained: I neither understand the message nor the meaning of the vertical and horizontal lines.
- 3–32: Wiegner et al. (2015) is not existing.

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- 4–11: 'The profiles were hourly (or half hourly) averaged'. What is the reason for the different averages, and under which conditions do the authors apply 60 minutes and 30 minutes, respectively?
- 4–29: 'depending on the ceilometer type'. What is meant by this? The authors only use one type of ceilometers (CL31).
- 5–3: '(dealt in...)'. Maybe better '(discussed in...)'.
- 5–11: Fig. 4: Do the authors have an explanation why the noise is much higher for the TLV-ceilometer?
- 5–26: What is meant with the 'diurnal average'? Is it the 'mean diurnal cycle'? How are the 'error bars' in Fig. 8 determined? It is difficult to see which error bar belongs to which curve.
- 5–31: 'based on a 3D mesoscale mathematical model'. This model should be explicitly mentioned (MM4?).
- 5–32: 'showed the same results'. Here a 1–2 sentence summary of the results should be included.
- 6–2: 'Here, the inland...'. Maybe better: 'In the present study it was found that...'
- 6–7: I am not sure if I understood the statement 'height of 650m AGL until sunrise' correctly? Do the authors refer to the temporal range from sunset to sunrise? There is a difference between July and August at BD. Is there an explanation for this?
- 6–9: The authors may think about changing the order of Figs. 9/10 and Fig. 8: it might be better to first discuss specific examples/cases and then proceed to the 'general view'. Anyway: Figs. 9 and 10 must be fully explained including all

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symbols (the red cross) and the slope-calculation must be moved from the figures to the text and discussed in more details. Rather give m/h than m/min.

- 6–14: The location of the Lap-3000 should be included in the maps. A (very) short description of this instrument might be added to the 'technical section'.
- 6–22: 'inflated' → 'extended'
- 6–26: The agreement between the ceilometer retrieval and the RS is very good. Moreover, Figs. 11 and 12 perfectly confirm the findings of Fig. 8 (higher MLH at BD at noon). Thus, this outcome should be emphasized and the discussion extended.
- 6–30: 'between in' → 'at'; and 'between' → 'at'
- 6–32: 'due to large gaps of hundreds of meters': what does this mean? Data link failure of the RS? Isn't it better to omit these cases for the calculation of correlations?
- 6–34: 'collide' → 'coincide'.
- 7–4: 'luck' → 'lack'.
- 7–5: Why is there a temporal gap (e.g. from 3:00 to 4:00) between the time periods? The cloud cover should be determined directly for each 'profile' (e.g. 5 or 15 minute average): I assume that this would provide a more precise result and the authors avoid a discussion of what 'sporadic' is. Anyway: this part of the paper should be extended as well.
- Fig. 3d: Why is the time axis starting from 1:00 hours?
- References: 'Atmospheric sounding: <http://weather.uwyo.edu/upperair/sounding.html>': where is this used? Shall it be shifted to the acknowledgements? The

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Schween et al. paper is in ACP now. Thus, check all citations and add missing ones as described in the comments above (e.g. description of the numerical models, papers on previous studies of the comparison of MLH and models and ceilometer issues, similar studies from the eastern Mediterranean area if existing).

- Figures: add a horizontal (and vertical) grid to Figs. 2, 6, 7, 8 (similar to Figs. 11 and 12).

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