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Interactive comment on "Ceilometers as planetary boundary layer detectors and a corrective tool for ECMWF and COSMO NWP models" *by* Leenes Uzan et al.

Anonymous Referee #1

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The manuscript deals with the detection of the planetary boundary layer (PBL) height with ceilometers. The measurements are compared with results of atmospheric modelling and with the daily 11 UTC PBL height derived from radiosonde profiles of meteorological parameters.

The main topic of the paper is not new, but some new aspects are given in this paper and may justify publication.

The study is based on ceilometer observations in Israel. The PBL diurnal cycle is strongly influenced by sea breeze effects which makes the analysis quite complicated. Surprizingly, this aspect is not explicitly mentioned, e.g., in the abstract. The se-

lected case studies, however, clearly show the impact of sea breeze effects and, all in all, leave behind a rather confusing impression of the findings, . . .after reading the manuscript (see my comments below). These sea breeze effects are obviously (partly) not considered in the weather prediction models. There is no discussion on this. The PBL heights derived from the ceilometer and radiosonde profiles are, to my opinion, mostly wrong, and are in contradiction to the traditional definition of the PBL as the lowest well-mixed layer of the troposphere.

In conclusion, the manuscript is not acceptable in the present form.

Major revisions are necessary.

Here my comments and questions in more detail:

Introduction:

P2, L46: The mentioned advantage of ceilometers over lidars must be specified! Regarding what? . . .. is the question! If I would have to select, I would take a sophisticated lidar because such a system is much more powerful concerning emitted pulse energies and the list of aerosol products is long compared to quite 'simple' and 'weak' ceilometers. So, please specify what you definitely mean, . . . with advantage! Probably low costs, robust observations, no complex adjustments and calibrations.

However, the clear disadvantage of ceilometers, operated at water vapour absorption lines around 910 nm, is that the only product you can trust is the range-corrected signal, nothing else!

Research area:

P4, L92: Please provide longitude, latitude and height above sea level for Beit Dagan already here, and where is it located (including distance) with respect to Tel Aviv and Jerusalem.

P4, L109: Please provide frequently, what UTC means in local time. Local time is needed to better follow discussion on PBL evolution and diurnal cycle.

P4, L110-120: There is no general PBL diurnal cycle in Israel, I speculate. But you provide such an impression! The occurrence, onset, strength and impact of the sea breeze circulation depends on given meteorological conditions (marine westerly versus continental easterly air flows, low and high wind speeds, clear or cloudy conditions). The sea breeze event strongly influences the PBL diurnal cycle. All this must be carefully mentioned in the text. And what about the impact of dense desert dust layers (in the PBL and especially in the free troposphere)? Is there any PBL development when there is a dust outbreak event? So all in all, many factors seem to control the sea breeze events and the PBL cycle in Israel. Thus, please provide more details on this.

Instruments:

P6, L161, Why should single-wavelength lidars not allow the retrieval of mass concentration profiles . . . from proper profiles of particle optical properties? Sure, they can be used for this. Ok, this is not the topic of the paper. But the statement is wrong and should be removed.

The ceilometer on the other hand side cannot be used to derive proper optical and microphysical properties. That is true! A ceilometer can only be used to detect aerosol layers as a function of height. This is not much, but sufficient for PBL studies. That should be clearly mentioned.

P7, L185: Please state again where Beit Dagan is located.

P8, L184-187: It should be clearly emphasized that the radiosonde provides ONE value for the PBL height, no diurnal cycle, . . . nothing! Only a snapshot of the PBL height, a few minutes after launch, is provided by the sonde! In contrast, models can produce the diurnal cycle, and ceilometers can measure it. But all this is not shown and discussed!

Methods:

This chapter is much too long. Text book knowledge is presented in unnecessary detail.

For each method, please provide the equation, the explanation of the equation, the link to PBL height, and a proper reference. More is not needed. A short and compact section on methods is desirable.

P9, L247: This is confusing: A ceilometer is made to detect the base of the water cloud, but not to detect the cloud top height. In most cases of low level (liquid-water) clouds there is no chance to detect the cloud top! This needs to be clearly stated.

The maximum signal you measure cannot be interpreted as cloud top. This is a very erroneous statement! The maximum backscatter signal is somewhere between cloud base and cloud top. The maximum signal is at that height where the attenuation effect becomes so strong that the signal immediately drops to the sky background level. This needs to be clearly stated. The height of the maximum signal maybe 100, 300, or 1000 m below cloud top. Nobody knows!

P10 L268: Therefore also the following statement is wrong: Our algorithm denotes the PBL height as the top of the shallow cloud. As just mentioned, you are unable to see the cloud top with ceilometer, only exceptional, in cases with optically rather thin clouds. Please improve your statements. The discussion is unacceptable in the present form.

Results:

P10, L286, and Figure 3: This is the worst case you can select in a comparison paper. There is the PBL development, there is the sea breeze effect, and there is cloud evolution! As a consequence, the PBL depth is more or less undefined at these complex atmospheric conditions. Fortunately, the radiosonde temperature profile indicates the PBL height at about 800m because for this height range (from 50 – 800m) the layer is well mixed indicated by the almost height-independent virt. pot. temperature. Then the pot. Temperature strongly increases with height and prohibits vertical mixing higher up. However, in Fig.3, the PBL heights obtained by the authors (from radiosonde, ceilometer, COSMO and IFS model) are between 1000 and 2200m? This is confusing! The

PBL height is clearly not at 1000m, 1400m, 1700m, or even 2000m. So, the ceilometer result of 1700m is totally wrong to my opinion. The reason is obviously that the range-corrected signal (and the wavelet analysis) cannot be used at these cloudy conditions to detect the true PBL height. What you see is some arbitrary height where the range-corrected signal takes its maximum. . .

If the radiosonde observations of temperature, relative humidity, wind speed, and wind direction would be shown, we would have the chance to see what is going on here. But all this is not presented. Height resolved trajectory analysis would be helpful as well in the discussion of the complex meteorological conditions. Please provide at least the wind and RH profiles of the radiosnde in the figures. The reader may want to know more about the meteorological situation.

This case study is rather confusing and not helpful. Unambiguous, cloud free conditions would be desirable to check the different approaches of PBL height retrieval.

P11, L308: Again, Figure 5 shows a rather difficult case (PBL evolution plus sea breeze effect). There is obviously a marine boundary layer (with top at 600m, clearly seen by the radiosonde) and, on top, the upper part of continental PBL up to about 1500m (also visible in the radiosonde profile). But, per definition, the lower PBL counts (the lowest well mixed layer above the surface is the boundary layer, as defined by Stull 1988). And that is the marine boundary layer, indicated by the potential temperature profile and the ceilometer data. But the PBL height obtained from the ceilometer profile analysis is again around 1700 m. This is an error of more than 100%!

Please show RH and wind profiles (direction and speed) so that more information about the complex PBL develepment at sea breeze conditions is available. Again, the selected case and the discussion are rather confusing. The results are at all not convincing, and not understandable. What is then the message of the study?

Obviously the IFS model does not simulate the impact of the sea breeze impact correctly or even ignores sea breeze effects so that the continental pot. temperature profile

is obtained with this model. The IFS PBL heights seem to be in contradiction with the IFS pot. temp. profile.

The COSMO pot. temp. profile is in good agreement with the radiosonde profile and shows the PBL height at 600 m. Very stable conditions higher up are simulated with COSMO so that not vertical mixing is possible above 600 m height. Surprisingly, the COSMO PBL height is at 1700 to 2100 m. This is totally confusing! This seems to be simply a mistake! Please clarify!

P12: Is section 6.3 needed? It is a very specific regression approach, just applicable to Israel.

P12-13 The conclusions must be rewritten after clarifying all the contradictions.

As a general, summarizing remark: Both case studies are not well selected. They indicate very complex meteorological conditions. The authors do not provide sufficient meteorological information. Additional trajectory analysis would be helpful. The results are at all not convincing. What will the reader learn from such a confusing study? . . .. except that the PBL diurnal cycle is not easy to predict in areas with sea breeze effects and cloud formation over the day.

How can we then trust the findings presented in Figs. 2, 4, 6, and 7 when we have such confusing results in Figs. 3 and 5? In the present state, the paper cannot be accepted.

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