June 8, 2014

Dear Prof. Vogel,

First, we would like to thank you for agreeing to be the editor of this manuscript. We have carefully read the comments of the reviewers and we have done great efforts to answer their questions and to revise the manuscript in accord with their recommendations. We would also like to thank Prof. Ulrich Blahak and the first reviewer for their important comments.

Before replying to the reviewers' comments point by point, we would like highlight few points:
1) We added information about the fraction of activation in the transition-zone clouds for further explanation of this issue in the clouds we study.
2) We have expended the discussion and conclusions about the formation mechanism of the clouds and the origin of the parcels. Other possible scenarios are discussed as well in the revised version.
3) We added information about the measuring site and its position related to the radiosonde site.
4) To provide all the necessary details, we added an appendix with a detailed description of the cloud model (Appendix A).

Addressing all of the reviewers comments point-by-point (reviewers comments in bold):

Reply to reviewer #1

Summary – this is the third paper in a three-part series (Hirsh12, Hirsh13) that uses a data from a new passive remote sensing retrieval of thin clouds to examine conditions governing the formation and character of very thin cumuli with lifetimes of a few minutes and depths of O(100 meters).

Recommendation: Accept with major revisions. Developing a new technique for measuring thin boundary layer clouds and using it to

constrain models of the initiation of convection is definitely a worthwhile project. To make this work publishable however, the authors need to connect it to recent, similar work in cumulus convection. Specifically, they need to address horizontal as well as vertical variability in the thermodynamic variables that determine their initial cloud properties, and incorporate (or explain why they don't have to incorporate) thermodynamic entrainment.

Authors' reply: We thank the reviewer for his general support. The model that was developed for this study, describes the microphysical and thermodynamical evolution of an air parcel as it ascends and transforms from a pocket of dry aerosols through haze up to a cloud. To overcome few delicate approximations that works well for more developed clouds but might miss important components in cases of the weak perturbation, the model incorporates a fundamental representation of the thermodynamic variables, in the sense that it solves directly the first law of thermodynamics, rather than using conserved thermodynamic variables. In this way, it accounts more accurately for the latent heat release by condensation on haze droplets and its impact on the parcel buoyancy. The variability in the thermodynamic variables is treated through the use of ranges of locations and magnitudes of the perturbations (both in temperature and relative humidity) and examination of the resulted clouds. Regarding entrainment, the model do account for it in a manner that is reasonable for the case of weak updrafts (as studied in this manuscript). It is done through consideration of entrainment of momentum explicitly; however, since all the equations in the model are coupled, it affects all the properties of the ascending parcel, including its microphysical ones. Please see the detailed explanations below.

Specific comments:

1) Use conserved thermodynamic variables

This paper focuses on the variation of effective radius, vertical velocity, cloud lifetime and supersaturation as calculated by a Lagrangian parcel model given a starting height and initial relative humidity and temperature. It's much more common for large eddy simulations and

stochastic parcel models to start with thermodynamic variables that obey conservation laws, because this provides a much more natural reference state: the dry and moist adiabat, for which total water and entropy or static energy are conserved. A good example of this approach is Berg04, where they measure the joint probability density function of entropy (theta) and water vapor mixing ratio and use this to drive a series of parcel models. Given adiabatic ascent, the liquid water content is fairly well constrained as a function of height from the initial (theta,wv) perturbation. This approach would make the paragraph starting at line 22 on p. 1057 much more informative – as it stands the range of maximal reff and lwc listed there is arbitrary. Instead, start with a joint distribution of temperature and water vapor perturbations taken either from collocated observations or the boundary layer literature and use that to constrain the model and interpret the results.

Authors reply: we thank the reviewer for the opportunity to elaborate on this important issue. As the reviewer noted, most of the cloud models implement some conserved thermodynamic parameter and use it iteratively to calculate the parcel parameters. However, as mentioned by the reviewer, conserved thermodynamic parameters assume that the parcel movement in the atmospheric column is either adiabatic (if potential temperature, θ, is used), or psuedo-adiabatic (if equivalent potential temperature, θ_e, is used).

Indeed our first approach to the problem was to use a more standard Lagrangian approach using the above invariants. However, we realized that we are dealing with very small clouds that form by delicate perturbations in the boundary layer. This standard approach is not sensitive to the delicate process that we wish to describe in the formation and evolution of those small clouds. Specifically, the purpose of our model was to resolve the parcel motion and to enable detailed analysis of the uptake of water vapor by aerosols and their activation process, while taking into account the water vapor depletion by small, inactivated haze droplets, the latent heat release by this process and its impact on the updraft. Using the conserved quantity of θ for such a task is obviously insensitive to such processes, since by definition, θ is a conserved quantity under dry adiabatic assumption, meaning that latent heat release by condensation on haze droplets is not taken into account. Even using θ_e is inadequate for such a task, since one has to know the saturation-mixing ratio in advance (Wallace and Hobbs, 2006). The whole purpose of our model is to study the temporal evolution of the parcel based on the initial parameters of the environment and the parcel alone. In order to do so, we have taken a step backwards, by solving the core thermodynamic equations. Since the calculation of θ and θ_e is based on the first law of thermodynamics, we have implemented this law explicitly into our model. This has set our model free from any assumptions regarding the latent heat that is released by the condensation on haze or cloud droplets. By knowing how much the droplets have grown, we calculate the latent heat at every iteration and solve the first law of thermodynamics directly. Such delicate processes that can be approximated or ignored for the case of bigger cumulus clouds have to be fully resolved for the small clouds we study here.

In light the reviewer's comment we added the following to the manuscript (Page 8, lines 7-12): "There are parcel models that use conserved thermodynamic parameters such as potential temperature or equivalent potential temperature (see for example Berg and Stull, 2004). However, the purpose of our model was to resolve the parcel motion and to enable detailed analysis of the uptake of water vapor by haze droplets and of droplets activation process. For this purpose, the model solves the first law of thermodynamics directly."

In addition we wanted to emphasize that the results presented in fig. 4 and the relevant text are not arbitrary. They are based on many simulations conducted for a certain environmental conditions. They consider a range of perturbations in RH (compared to the environment) that are reasonable and based on measurements of RH perturbations in the boundary layer (Sempreviva and Gryning, 1996). Therefore, the results represent a range of possible characteristics of small clouds that form in the Israeli summer.

2) Entrainment and "nature vs. nurture"

A very active research question is the extent to which shallow cloud properties are determined by the characteristic of the updraft they form on, or entrainment events they undergo during their ascent. As far as I can tell from Hirsch13, the parcel model used in this paper accounts for entrainment of momentum (eq. 4) but not of entropy of water vapor. Romps10 is a good example of how entrainment can be accounted for in a (bulk) stochastic parcel model, and how important that potentially is for shallow convection. Looking at cloud properties using an ensemble generated from observed pdfs of conserved variables, undergoing entrainment in line with large eddy simulation estimates of small scale mixing, would connect this work to the current literature and provide a more tightly constrained and more physically consistent set of results.

<ins>Authors reply</ins>: We thank the reviewer for this comment. We agree that entrainment should be treated carefully. It seems there is a general agreement that entrainment in general affects both the momentum of the rising parcel, and the “internal” properties of the parcel by injecting drier and colder air from the environment into the ascending parcel. Nevertheless, most models (such as Romps 2010) analyze the effect of entrainment on the developing cloud, starting their treatment at cloud base. Such models usually assume the updraft is relatively high, and the differences between the relative humidity and temperature of the parcel (which is already saturated) and the environment are considerably large. For example, Romps (2010) considers a cloudy grid to be characterized by an updraft > 0.5 m s^{-1}. However, we believe that for the special subset of small clouds that we study in our manuscript, that form under very weak updrafts, and in humid surrounding conditions, the influence of the entrainment is expected to be limited. For example, in the case study that appears in figure 3 in the manuscript, the updraft of the parcel when it reaches saturation is ~0.3 m s^{-1} and decreasing, while the temperature difference between the saturated parcel and the environment is ~1.2°C.

In this manuscript, for studying these transition zone clouds that form due to weak perturbations along the atmospheric profile, we use a semi analytical model that accounts for the key processes using basic physical principals that allow us to examine the formation of such clouds. The con of such analytical model is that we can solve for the entrainment only in a “bulk” form that give a first approximation, which under the specific condition discussed here should give a good range for it. More specifically, as the reviewer noted, our model (which is introduced in details at Appendix A in the revised manuscript) accounts for entrainment of momentum explicitly (equation 4). However, since all the equations in the model are coupled, the effect of the entrainment on the momentum affects all the properties of the ascending parcel, including its microphysical ones (LWC, r_{eff}, etc.). The following are equations (1)-(3) in the manuscript. One can notice that the updraft (U) affects the supersaturation ($s_{v,w}$), which in turn affects the growth rate of the droplets (dr/dt). The growth rate of the droplets by condensation determines the latent heat release (dq), which affects the change in the temperature of the rising parcel.

$$dT = \frac{1}{c_p}\left(dq + RT\frac{dp}{p}\right)$$

$$r\frac{dr}{dt} = \frac{D_v^* M_w e_{sat,w}(T_\infty)}{\rho_s'' \mathcal{R}T}\left(S_{v,w} - \frac{1}{1+\delta}\exp\left[\frac{L_e M_w}{\mathcal{R}T}\left(\frac{\delta}{1+\delta}\right) + \frac{2M_w\sigma_{\frac{s}{a}}}{\mathcal{R}T(1+\delta)\rho_w r} - \frac{\upsilon\Phi_s\varepsilon_m M_w \rho_N r_N^3}{M_s\rho_w(r^3 - r_N^3)}\right]\right)$$

$$\frac{ds_{v,w}}{dt} = \frac{p}{\varepsilon e_{sat,w}}\frac{dw_v}{dt} - \left(1 + s_{v,w}\right)\left(\frac{\varepsilon L_e}{R_a T^2}\frac{dT}{dt} + \frac{g}{R_a T}U\right)$$

In light the reviewer’s comment we added a discussion regarding this limitation (Page 8 lines 12-17): “As detailed in Appendix A, the cloud model accounts for the effect of entrainment only on the momentum. However, under the specific conditions of relatively weak updrafts, which serve as the "entrainment engine", such representation provides a reasonable approximation. Moreover, the coupling between the model’s equations imposes interaction of the entrainment process with all other processes.”

3) Haze vs. activated droplets

The beginning of the paper makes the distinction between aerosols which have passed through the peak of their Kohler curves and activated, and haze particles. That distinction is then dropped in the later part of the paper, for the arbitrary definition of a cloud as a collection of droplets with a distribution that has an reff larger than 0.5 um. Given that you are carrying 250 different size classes of aerosol in the model, it would be useful to provide more detail on, the role of haze vs. activated drops in forming the size distribution of clouds in the transition zone.

Authors reply: the reviewer is obviously right, and we thank him for this important comment. Indeed, at the beginning of the manuscript we discuss the differences between haze and activated droplets and later we use the criteria of r_{eff}>0.5 μm in order to analyze the lifetime of the cloud. Our analysis suggests that for the clouds that appear in figure 4, this threshold represents the point in time just after the cloud droplets activation starts. We thought that such a threshold could be robust enough as it is relatively simple and can be calculated for any cloud and it does "talk" with remote sensing measurements. In light of the reviewer comment, we added the following (Page 13 line 19-22): "This threshold was chosen since it represents the beginning of the droplets activation process in clouds. For the clouds that appear in Figure 4, when the maximal r_{eff} reaches 0.5 μm, the fraction of activation (in terms of number size distribution) is 0.13%."

In addition, we added the paper an analysis regarding the fraction of activated droplets. Based on that we modified Figure 4, and added information to the text to introduce it (Page 12 lines 11-14):

"The maximal r_{eff} (blue), maximal RH (red), and maximal LWC (black) of the forming clouds were plotted against the initial RH of the parcel. In addition, the fraction of activated droplets (in terms of number size distribution) is plotted against the initial RH (magenta)"

And: “It is also interesting to note that the maximal fraction of activation can be in some clouds lower than 10 % (in terms of number size distribution), which is the lower limit that can be measured in cumulus clouds (Komppula et al., 2005).” (Page 13 lines 13-16):

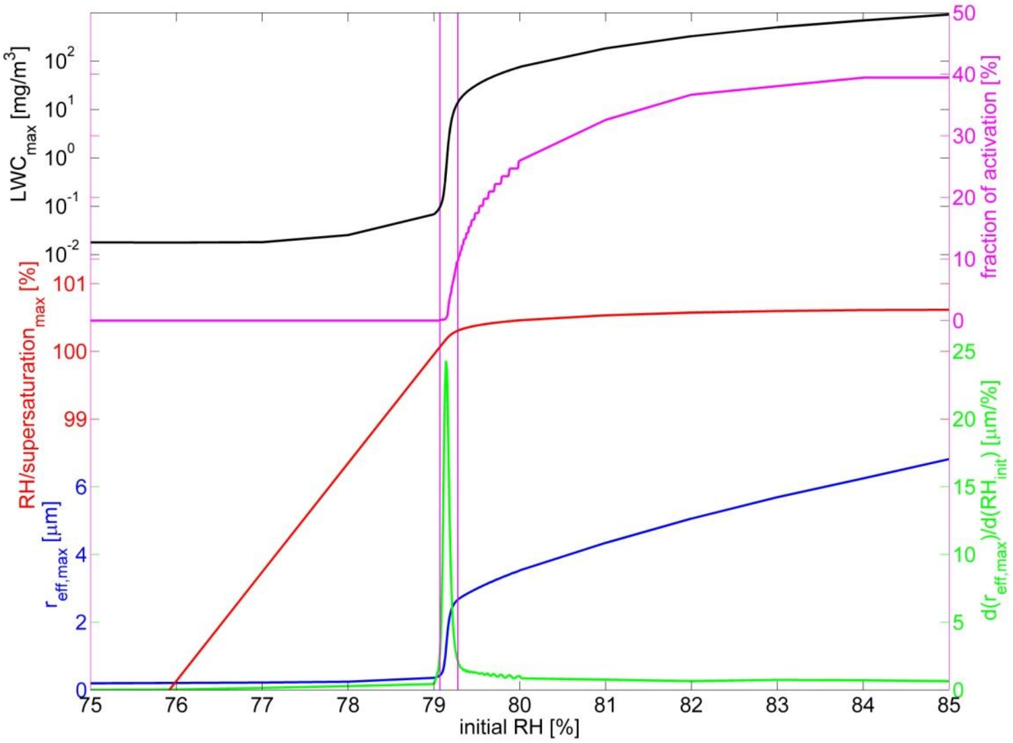


Figure 4 - The maximal effective radius (r_{eff}, blue), maximal relative humidity (RH, red), maximal liquid water content (LWC, black), and fraction of activation (in terms of number size distribution, magenta) of the forming clouds vs the initial RH of the parcel. The green line is the derivative of the maximal r_{eff} with respect to the initial RH of the parcel. Transition-zone clouds are defined within the vertical magenta lines (see text for further explanation)

Reply to Prof. Ulrich Blahak (reviewer #2)

The paper “On Transition zone water clouds” by E. Hirsch et al. describes measurements of some important local cloud properties (Reff; LWC) of small short-lived cumulus clouds during one day of a 2-month field campaign in Israel. The measuring method itself is based on a retrieval technique using vertically pointing radiation sensors in the far infrared region and is described in a 2012 paper of Hirsch et al. The sampled clouds formed on clear sunny days in a relatively dry and shallow convective PBL,

topped by a typical subsidence inversion, they existed only for some minutes and were typically smaller than 100 m in diameter. Associated with this are very small values for Reff, which are below the typically assumed lower threshold of about 4m for longer lived clouds. This leads the authors to call this type of clouds “transitional” in the sense that they are somewhere between haze and “normal” clouds, where the relative radius growth rate of the activated droplets in a supersaturated parcel is still large according to the Köhler theory, but the short lifetime of the updraft and detrainment prevents them from becoming longer lived cumulus clouds. I fully agree with that terminology.

These measurements are very valuable and should be published. I also find the given literature background in the introduction adequate and sufficient.

In a second step, the observed cloud base heights are compared to classical TEMP analyses of a proximity souding (10 km distance to the measuring site), and it is found that the observed height was lower by some 500 m compared to the LCL of 1500 m derived from the sounding (based on both the surface parcel and the 500 m layer averaged parcel). Moreover, the LCL was well above the capping inversion, so that one would not expect any Cu clouds to form in this situation. This lead the authors to the hypothesis that the buoyant parcels which lead to cloud formation are not associated with temperature disturbances in the surface layer (thermals), but with humidity disturbances somewhere in the middle of the PBL.

To support this hypothesis, the authors developed a simple parcel model (described in an accompagnying paper by Hirsch et al., 2013, in ACPD) and, starting from the values of T and RH of the proximity sounding, imposed positive RH disturbances on initially stationary elevated parcels to simulate their subsequent rise, cloud formation and decay. By variation of the parcel starting values (magnitude and height of the RH disturbance) in a Monte-Carlo sense, the range of possible candidate parcels to resemble the observed clouds (base height, Reff, LWC) has been explored and found to be roughly between 10 and 20 % RH disturbance. Maximal values of simulated Reff, LWC and updrafts during the cloud lifetime are presented.

The resulting max. updrafts and lifetimes seem plausible and the informations on Reff and LWC are very valuable by itself. The variations were however limited to the scenario of starting heights between 250 m and 750 m. This leads the authors to the conclusion that elevated RH disturbances are the only plausible explanation. However, I find the presented evidence and facts not sufficient to draw such a firm conclusion. Other possible scenarios have not been discussed and some important informations on the measuring site and the measurements itself have not been mentioned. This does however not corroborate the abovementioned valuable modeling results, because in the end it is not so important at what height the parcel originated, as long as the resulting cloud properties are realistic.

Therefore I would recommend to relax the conclusion in the sense that elevated RH disturbances are a plausible hypothesis with the need of further investigation and to discuss possible alternatives. I would consider this as a major revision.

But I agree with the authors that such clouds might play a somewhat larger role in the radiative forcing than we currently attribute it to be. However, I would not per se say that they are very important, because of their short lifetime and usually low covered area, but this surely needs further research.

Authors' reply: We thank Prof. Blahak for his kind words and important comments. According to his recommendations, we changed the discussion about the origin of the parcels, we relaxed the RH conclusions, and added some more information about the measuring sites in order to give a more complete background for other possible explanations to our results. The elevated RH disturbances are presented in the revised text as a possible hypothesis and other explanations are discussed as well. In addition, we added a detailed description of the model in the appendix of the revised manuscript.

More specifically:

In the manuscript we presented the results of the parcels that originated at heights of 250-750 m because these initial heights resulted with clouds' base height similar to the measured values by the ceilometer. Other (lower or higher) initial heights of the parcels formed clouds at wrong vertical position. When we introduced figure 5 we wrote: “The figure presents only cases which resulted in cloud-base heights similar to the measured height (initial heights between 250 and 750 m)”. In light with Prof. Blahak’s comment we elaborated more on this issue in the revised manuscript (Page 15, lines 4-7): “According to the cloud model results, only parcels that initiated at altitudes between 250 and 750 m formed a cloud base height that is comparable with the actual measured one. Therefore, only those parcels results are presented”.

Moreover, it appears that in the original manuscript, the description of the two measuring sites was not clear enough. The IMS station at Beit-Dagan conducts the daily measurements of the atmospheric profile by radiosonde and in addition, they conduct cloud base heights measurements with a ceilometer. Thus, the atmospheric profile and cloud base height are measured at the exact same geographic location. At the measurement site in Nes-Ziona, we measured the COD, LWP, and r_{eff} of the passing clouds. Following Prof. Blahak’s comments regarding the possible different surface conditions we made this point clearer in the revised manuscript. Specifically we wrote (Page 7 line 13 - Page 8 line 2):

“Complementary measurements of the atmospheric profile and cloud base height were conducted by the Israel Meteorological Service (IMS) at Beit-Dagan station, which is located 10 km north of the cloud-measurement system, in a similar distance from the coast. Thus, the influence of the sea breeze on the atmospheric conditions at both sites is expected to be similar. The cloud base height was measured by ceilometer, and the atmospheric conditions were measured twice a day by a radiosonde (at 0:00 UTC and 12:00 UTC). The data were downloaded from the University of Wyoming website (Website: Atmospheric sounding). The radiosonde provides information on temperature, pressure, humidity, and horizontal wind speed profiles from the surface to the end of the troposphere. As explained later, the daily 12:00 UTC atmospheric profiles were used as an input data for the theoretical cloud model and for calculating the expected cloud-base height (based on LCL). Those values were compared to the measured cloud base height at the same geographical location, at Beit-Dagan."

Moreover, in light Prof. Blahak's comments, we have relaxed some of our conclusions regarding the origin of the parcels and we added some possible alternative explanations:

We changed "short-lived convective clouds that form as a result of RH perturbations" to "short-lived convective clouds under weak updrafts regime in a humid boundary layer bounded by a thermal inversion" (page 20 lines 9-11).

We also added the manuscript some information about the measuring site in Nes-Ziona (where the COD, LWP, and r_{eff} were measured) in order to give a complete description and to better support our conclusions. We added: "The measurement site was in Nes-Ziona, which is located approximately 10 km away from the coast." (Page 6 line 13 - Page 7 line 1).

In addition, we wrote in the discussion: "There is a possibility that the atmospheric conditions at the measurement site (at Nes Ziona) where the clouds characteristics were measured, were different from the atmospheric profiles measured in Beit Dagan station, but due to the similar distances of both stations from the sea shore it is not plausible" (Page 20 lines 18-22).

Please see the detailed answers to specific comments below.

Specific comments

Section 1

1) Page 1053, line 14: Were the results of Mordy (1959) on droplet growth times obtained under the assumption of constant supersaturation, i.e., neglect of supersaturation depletion during the process? If yes, you could mention this, because it should cause a certain systemtic underestimation of the growth times, which you avoid in your parcel model.

Authors' reply: We thank Prof. Blahak for his suggestion. Indeed, using constant supersaturation would imply systematic underestimation of the growth times. However, examining the study presented by Mordy (1959) reveals that his parcel model did take into account RH/supersaturation decrease due to water vapor depletion over the condensing water droplets.

Section 2

2) Page 1055, line 4: Were is the measuring site exactly in Israel? Of interest is also the distance to the coast because of a possible influence of a sea-breeze on the near-surface moisture in contrast to the radiosonde site at Beit-Dagan.

Authors' reply: We agree with Prof. Blahak. The distance to the coast might influence the humidity and temperature profiles. However (as explained above), the location of the ceilometer that measured cloud base heights and the radiosonde measurements of the atmospheric profiles were identical - at the Beit Dagan meteorological station. So the method we used to choose the relevant initial altitudes of the rising parcels and the magnitude of the RH perturbations is reasonable since it compared the model output for a measured profile to the measured cloud base heights in the same geographic position. The difference between the conditions in Beit Dagan site and Nes Ziona site where the measurements of clouds characteristics were conducted (beside base height) could influence the comparison between the simulated cloud microphysical characteristics and the measured ones. However, the measuring site at Nes-Ziona and the radiosonde station at Beit-Dagan are both located similarly approximately 10km away from the coast (see attached image). Therefore we don't expect to find significant differences in the atmospheric conditions and in the forming clouds between the two places.

In light with Prof. Blahak's comment we add the following to the manuscript: "The measurement site was in Nes-Ziona, which is located approximately 10 km away from the coast." (page 6 line 13 - page 7 line 1). In addition, "Complementary measurements of the atmospheric profile and cloud base height were conducted by the Israel Meteorological Service (IMS) at Beit-Dagan station, which is located 10 km north of the cloud-measurement system, in a similar distance from the coast." (page 7 lines 13-16).

3) Page 1055, line 14: Add one or two statements about the accuracy/error bars of the measurements.

Authors' reply: We thank Prof. Blahak for his comment, and we added the following to the manuscript: "Since the retrieval is based on spectral analysis, its accuracy is not constant for all values of r_{eff}. However, the retrieval is at its highest sensitivity for thin clouds, and the error is estimated to be ±0.5 μm for $r_{eff} \approx 2$ μm and for LWP< 10 g m^{-2} (see Hirsch et al, 2012 for further details)." (page 7 lines 9-12).

Section 3

4) Page 1056, lines 14 and 15: The values of LWP and COD are larger than their averages, indicating extremly asymetric distributions. Giving percentiles (e.g., the 10th and 90th) instead of standard deviations would be more appropriate.

Authors' reply: We agree with Prof. Blahak and we corrected it in the new version of the manuscript (page 9 lines 5-10): "The temporal average r_{eff} of the cloud (as it passed in the zenith) was 1.24 μm (with standard deviation of $\sigma = 0.2$ μm), the average LWP was 0.13 g m^{-2} (with 0.01 g m^{-2} and 0.37 g m^{-2} as the 10th and 90th percentiles respectively), and the average COD at 550 nm was 9.15 (with 0.66 and 30.3 as the 10th and 90th percentiles respectively)."

5) Page 1056, paragraph starting at line 19: Please add some words about the assumed aerosol spectrum. Also, is it the same for the data in Fig. 4?

Authors' reply: The aerosol size distribution used in this part of the paper is based on measurements presented by Asmi et al, (2011) for a station in Crete and it is the same for the data in figure 4. In light with Prof. Blahak comment, we added information about the dry aerosol size distribution that was used in this specific part of the results (page 10, lines 7-10 in the revised manuscript): "The dry aerosol number size distribution that was used is based on measured aerosol size spectra from the island of Crete, and the diameters can be represented as a sum of two log-normal distributions centered at 86 and 189 nm (see full description in Asmi et al, 2011, station FKL)."

When introducing Figure 4, we added: "Every point in the figure represents the results of a complete simulation, similar to the one presented in Figure. 3, using the same dry aerosol size distribution (based on measurements from Crete, Asmi et al, 2011)." (page 12, lines 8-11 in the revised manuscript).

6) Page 1058, line 11: Based on the evidence you presented in the paper, I disagree with your firm conclusion ". . . that such clouds must be a result of RH perturbations in the mixing layer". You relax that in the next sentence by calling it a valid hypothesis (which I agree), but at other locations in the paper you convey it as a proven fact (at least it sounds so in my ears). And this is not justified because your modeling can only show that it would be a valid hypothesis. (Nevertheless, your modeling results regarding updraft speeds, Reff, LWC and cloud life time are interesting and important by themselves).

Now I'll play devil's advocat and develop an alternative scenario: If the measuring site is located nearer to the coast than the radiosonde station Beit-Dagan, it could be more influenced by sea-breeze with moist air near the ground. I estimate that a 10 - 20 % higher RH in the sea-breeze air could easily lower the cloud base for "classic" surface-based shallow convection to the observed values. The initial buoyancy of such parcels would not necessarily have to be caused by temperature disturbances, but could (partly) also be due to close-to-ground RH -disturbances at the sea breeze front. In other words, the near-surface radiosonde values at Beit-Dagan might not be representative for the measuring site, despite only 10 km difference. And, if the hypothetical strong RH-disturbances in mid PBL would exist, the radiosonde could have sampled one by chance (e.g., the very dry value above the moister surface value).

<u>Authors' reply:</u> Thank you for this comment and for highlighting the important points that need to be described in a clearer way. In accordance with the above comments, we changed the discussion about the origin of the parcels in the paper and we added some more information about the location of the measurement system in order to give a more complete background for alternative explanations to our results. Specifically, Prof. Blahak is right in that sea breeze could change the conditions near the surface and cause RH perturbations. Therefore, differences in the location of the measuring site and the meteorological station compared to the seashore may suggest another explanation for our results and for the triggering mechanism. However, as stated

above, the cloud base height and the atmospheric profiles were measured both in the same location in Beit Dagan. Therefore, the method to use the measured base as a restriction to the simulated clouds, based on the measured profiles, is likely to be consistent. The 10 km distance between Nes Ziona and Beit Dagan can influence the comparison of the measured and simulated other clouds characteristics. However, (luckily for us) the two sites are located in parallel to the coastline, therefore assuming mostly weak westerlies (which is the right advection for most of the summer days), the distances of the measuring site and meteorological station from the coast are very similar. Based on that we think that it is reasonable to assume that the temperature and humidity profiles in the lower atmosphere are similar in Bet Dagan and in Nes Ziona and so are the forming clouds. Nevertheless, we accept the comment, and we relaxed the conclusions regarding the RH perturbations and we added other possible explanations to the discussion. In the revised manuscript, we write "There is a possibility that the atmospheric conditions at the measurement site (at Nes Ziona) where the clouds characteristics were measured, were different from the atmospheric profiles measured in Beit Dagan station, but due to the similar distances of both stations from the sea shore it is not plausible". (page 20, lines 18-22).

We also agree with Prof. Blahak that a bigger perturbation in RH near the surface (RH perturbation of 16% in our case) would result in a simulated cloud base height at the measured height. However, please note that even smaller perturbation would result in a cloud but with higher base height. This contradicts the measurements of cloud base heights that point to a small variability (see further details in the answer to comment no. 7.2) in their bases. The following graph (similar to figure 5a in the manuscript) presents the resulted cloud base height as a function of the initial height of the parcel and the magnitude of the perturbation in RH. It is notable that surface based RH perturbations (even very small perturbation) would result in a cloud formation. In this case we would expect to find clouds at various heights (from the high LCL to the low actual ceilometer readings). However, when we examine the ceilometer readings we see very small fluctuations in the cloud base heights.

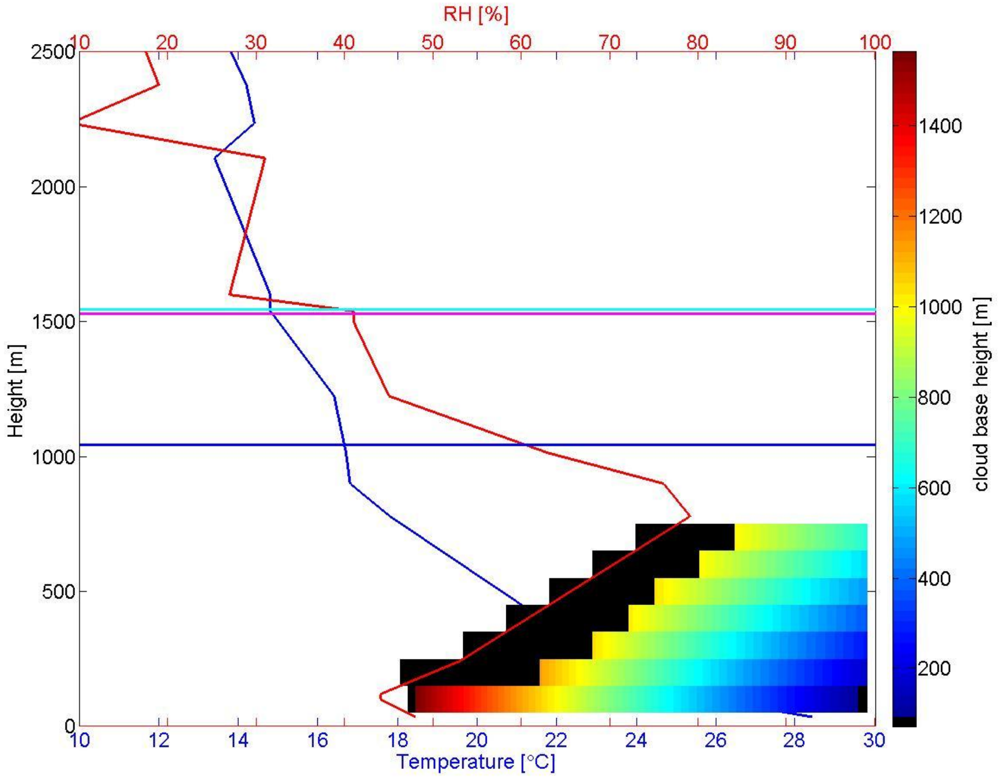
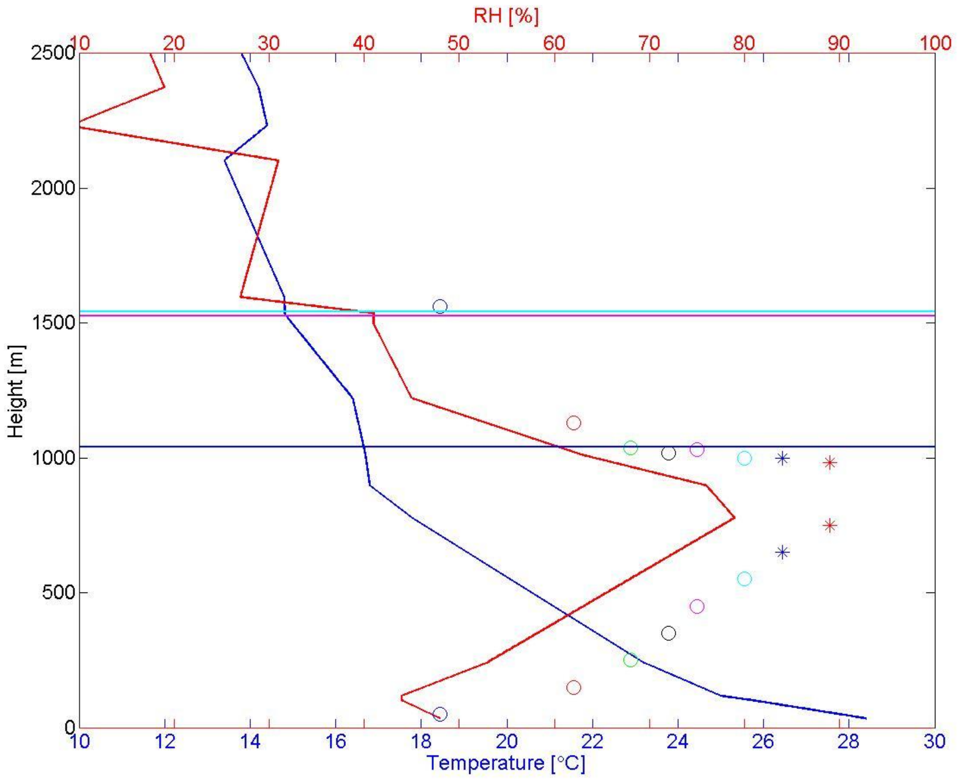


Figure 1: The temperature (blue) and RH (red) profiles are plotted along with the ceilometer measurement of cloud-base height (horizontal blue line) and the theoretical LCLs (horizontal cyan and magenta lines). The position of the colored region on the graph represents the initial height of the parcel and the magnitude of the RH perturbation, i.e. the difference between the lower colored region and the red line represents the perturbation in the RH. The colored region on the graph correspond to the resulting cloud base height (see colorbar in meters at the right side of the graph).

The next graph is figure 5a from the manuscript with the addition of the relevant information for parcels rising from initial heights of 50 and 150 meters. One can notice that the smallest perturbation that would create a cloud, for all the heights of 250-750 meters, would result at almost the same cloud base height. Therefore, we think that the smaller variance suggests that the origin of the parcels is RH perturbations from the middle of the boundary layer, rather than thermals from near the surface.



7) Other currently missing information on your measurements could help, too:

7.1) • Location of your measuring site relative to the coast.

Authors' reply: Thank you for this comment. We added the following information to the manuscript: "The measurement site was in Nes-Ziona, which is located approximately 10 km away from the coast." (page 6 line 13 - page 7 line 1). In addition, "Israel Meteorological Service (IMS) at Beit-Dagan station, which is located 10 km north of the cloud-measurement system, with a similar distance from the coast." (page 7 lines 14-16).

7.2) •How large is the standard deviation of observed cloud base heights during the afternoon hours? If it is small, it would suggest surface-based triggering of thermals. Otherwise, it could be a hint towards elevated RH-disturbances without connection to the ground.

Authors' reply:

The IMS station at Beit-Dagan has provided us the 10-minute average readings of the clouds base height. The mean cloud base height during noontime of June 30th 2011 (14:00-16:00 local time) was 1041 m and the standard deviation was 43 m. The variance in the cloud base height is rather small. This example represents well the cloud base measurements in other days.

However, as explained above we think that the small variance is in fact supporting the hypothesis that the origin of the clouds is RH perturbation in the middle of the boundary layer. The model shows high sensitivity in the cloud base height, to the magnitude of thermal perturbation near the surface.

7.3) •What is the typical cloud cover and spatial distribution of the transient clouds? Are they attached only to a coastal strip or are they all over the place?

Authors' reply: This is a very good question. Since the field campaign was local, we can't answer this question based on our current set of observations. However, based on our experience and our knowledge about the atmospheric conditions in the Israeli summer, small, thin clouds are very common during this time of year. We are currently preparing a manuscript that estimates the radiative forcing that such clouds pose. Based on the field campaign, we can state that clouds with LWP<10 g m^{-2} were measured during ~30% of the summer time, and that they were responsible to ~83% of the zenithal reflectance. The following graph presents an example of a few days during the field campaign and it can be seen that the small clouds (LWP<10 g m^{-2}) are the dominant clouds during noon time.

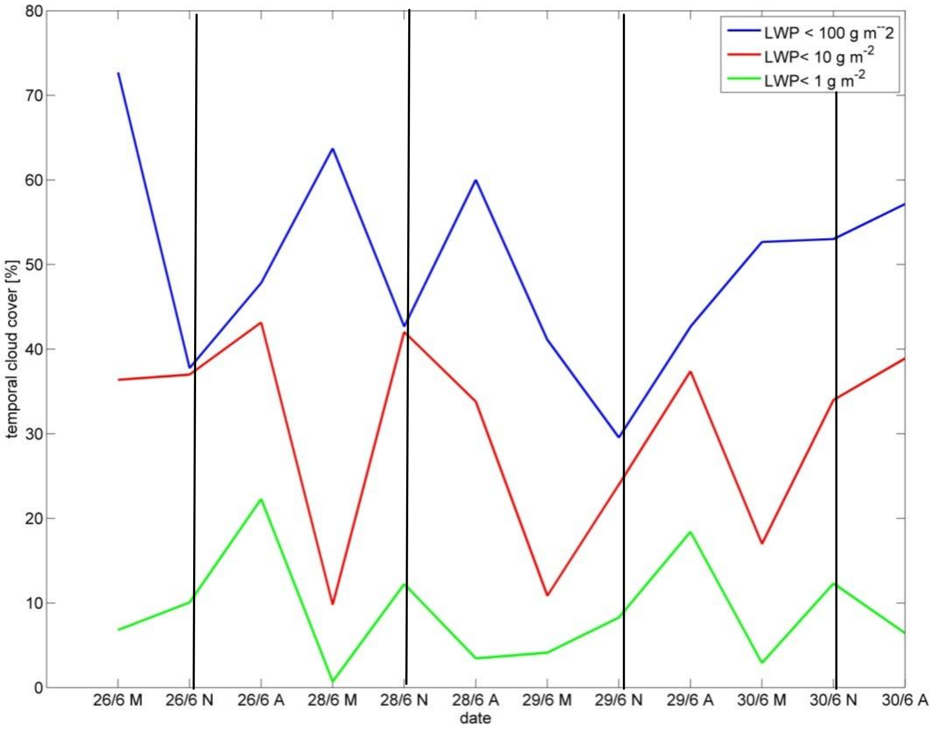


Figure 2 - the temporal cloud cover (in percent) during 4 days during our field campaign. M - represents morning hours (8:00-11:00 local time). N - represents noon hours (11:00-15:00 local time), and A represents afternoon hours (15:00-19:00 local time). It is notable that clouds with LWP < 10 g m-2 dominate the cloud cover, especially during noontime.

This analysis is a part of the next paper that is currently in preparation.

8) In the conclusions, the related sentence Page 1061, line 2 ff. should be relaxed accordingly, as long as you do not have more conclusive observational evidence.

Authors' reply: we have changed the relevant sentences to be: "Our results can be generalized, suggesting that there are convective clouds with maximal size in the range of a few hundred meters or less, that form below an inversion layer, with low supersaturation values." (page 21, lines 9-11).

9) Page 1061, line 18: "These findings suggest that . . . has an important radiative forcing effect . . . ". But did the cited paper Wood and Field (2011)

not suggest that, despite such clouds dominate the number distribution, their total area and radiative impact is limited?

Authors' reply: This issue is still debatable. Koren et al (2008) showed cases where the small clouds contributed ~50% to the total reflectance. We cited Wood and Field (2011) as an example for global analysis of the number size distribution. In the revised manuscript (page 21 line 23 - page 22 line 3) we write "Since previous studies reported cases where small clouds contributed ~50% of the reflectance (Koren et al., 2008), these findings suggest that at some environmental conditions the subset of transition-zone clouds has an important radiative forcing effect which is currently either not considered, or wrongly attributed to aerosols"

Technical corrections

10) Page 1052, line 23, and page 1053, line 20: Köhler, not Kohler

Corrected

11) Page 1055, line 8: The sentence "The method was specifically designed to retrieve the properties of thin clouds and . . . " is doubled (cf. one sentence before). Delete until "and" and keep "It relies on three elements: . . ."

Corrected

12) Page 1056, line 24: From Fig. 3, I read an intital perturbation of 13 – 14 % instead of 11 %. Am I mistaken?

Authors' reply: the initial perturbation is 11% (79% with regard to environmental RH of 68%). The labeling on the original figure was not clear. We thank Prof. Blahak for this comment and we corrected this issue in the revised manuscript (see image below)

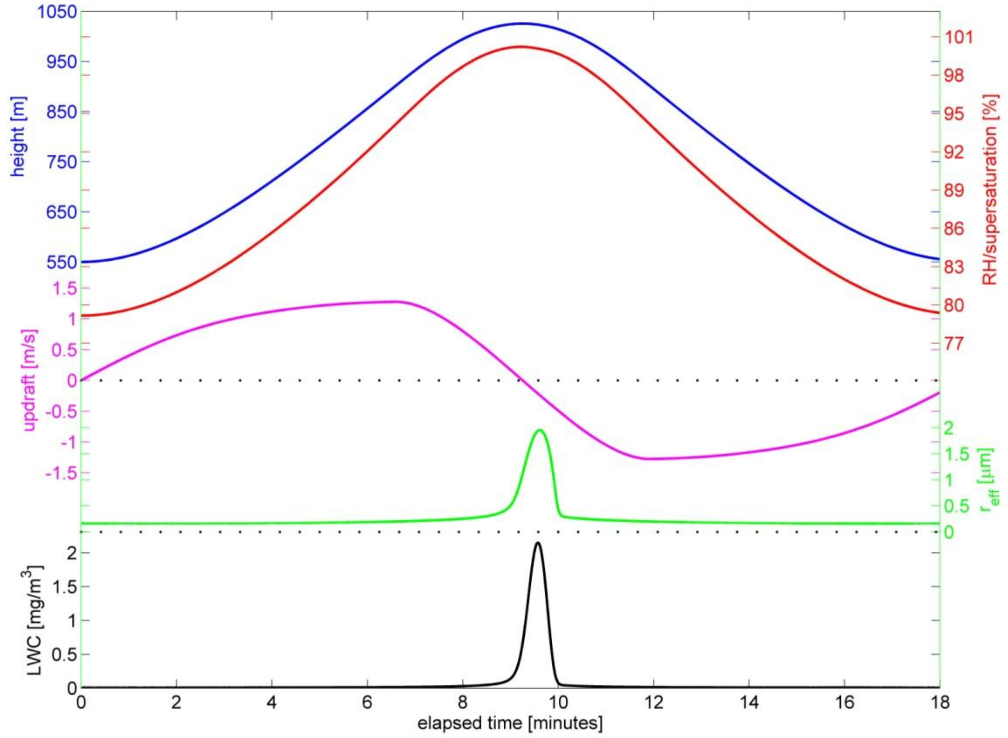


Figure 3 - Temporal evolution of an air parcel. Vertical position (blue), relative humidity (RH) and supersaturation (red), updraft (magenta), effective radius (r_{eff}, green), and liquid water content (LWC, black).

13) Page 1058, line 14: Where can I read the RH -disturbance in Fig. 5? I presume it is the difference between the lower symbols and the red line, but please clarify in the text.

Author's reply: We clarified it in the revised manuscript: when we introduced figure 5 in the text and in the caption of the figure we wrote: "The position of the colored region on the graph represents the initial height of the parcel and the magnitude of the RH perturbation, i.e. the difference between the lower colored region and the red line represents the perturbation in the RH." (page 15 lines 15-18).

References

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