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Interactive Comment

Interactive comment on "On transition-zone water clouds" *by* E. Hirsch et al.

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General comments

The paper "On Transition zone water clouds" by E. Hirsch et al. describes measurements of some important local cloud properties (R_{eff}, 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

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values for R_{eff}, which are below the typically assumed lower threshold of about 4 μm 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 $\approx$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, R_{eff}, LWC) has been explored and found

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to be roughly between 10 and 20 % RH disturbance. Maximal values of simulated R_{eff}, LWC and updrafts during the cloud lifetime are presented. The resulting max. updrafts and lifetimes seem plausible and the informations on R_{eff} 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.

Specific comments

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Section 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.

Section 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.

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

Section 3

Page 1056, lines 14 and 15: The σ 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.

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?

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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, R_{eff}, 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).

Other currently missing informations on your measurements could help, too:

- Location of your measuring site relative to the coast.
- 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.

- 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?

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.

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?

Technical corrections

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

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: . . . "

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 1051, 2014.

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