Atmos. Chem. Phys. Discuss., 15, C819–C822, 2015
www.atmos-chem-phys-discuss.net/15/C819/2015/

Atmospheric Chemistry and Physics Discussions

ACPD
15, C819–C822, 2015

Interactive Comment

Interactive comment on "On the reversibility of transitions between closed and open cellular convection" *by* G. Feingold et al.

Anonymous Referee #1

Received and published: 16 March 2015

Review of On the reversibility of transitions between closed and open cellular convection

Authors: Feingold, Koren, Yamaguchi, Kazil

This manuscript uses a series of cloud resolving model simulations based on a subtropical marine stratocumulus case to explore the extent to which sharp decreases and subsequent increases in cloud droplet concentration (N) affect transitions from closed-to-open and open-to-closed cellular convection. Transitions from closed to open cells driven by rapid drops in N are found to occur more rapidly than transitions from open to closed cells caused by rapid increases in N. The loss of turbulent kinetic energy caused by reductions in cloud top longwave cooling is the primary driver of the closed to open cell transitions, and is also required for the recovery of the closed cell state. However, the simulations show that this recovery takes substantially longer than the closed-to-open transition, even when the N recovery is almost instantaneous. Factors that increase the efficiency of longwave cooling (which include changing to a more realistic radiative transfer model, and also reducing the emissivity of the free tropospheric air) tend to result in more rapid recoveries, but increased surface fluxes also hasten recovery. Recovery of the closed cell state is slowed down when the N increases coincide with sunrise and a reduced overall radiative cooling. Overall, this is an interesting study that will be of major interest to readers of Atmospheric Chemistry and Physics. The manuscript is well-written and should be suitable for publication given some minor revision. I include a number of questions, comments and suggestions below that the authors may wish to consider.

SPECIFIC POINTS.1

1. One burning question I had is whether the results would hold up for the much deeper open and closed cell cases found over the Southeast Pacific during VOCALS. The simulations in Wang et al. (2011) could be used here. The authors do experiment with the impact of PBL depth (section 4.3), but the PBL height difference in the contrasting case looks to be only about 100 m higher (Fig. 5b). The VOCALS cases were more like double the PBL depth.

2. The authors do a good job explaining how the lagged recovery appears to relate to the difficulty establishing strong longwave cooling against precipitation losses. However, I wonder if the explanation is a little simplistic. To recover a closed cell state does not simply require LW cooling, but it requires that parcels cooled by LW cooling are able to sink under their buoyancy to a level whereby surface moistening can replenish the moisture supply to the cloud layer. I would therefore expect that the recovery timescale might also depend on the time that the PBL has been allowed to remain in a decoupled state (i.e., the time between N drop and N increase). An open cell PBL has a rather stratified upper PBL, so the LW cooling driving recovery will need time to drive efficient

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and deep mixing. The authors do not specifically mention this. It would be interesting to complete a sensitivity study where the low N period is either shortened or extended (perhaps both).

3. In my view, the connections between TKE and LW cooling need to be explored further. Can the authors show how different levels in the vertical contribute to the TKE and to its recovery. This would help strengthen the argument about a lack of reversibility. It might also explain why relatively small reductions in N seem to cause a more reversible transition, despite driving significant reductions in precipitation. What do the vertical profiles of theta and q look like during the transition?

4. The predator-prey model results seem obvious to me, unless I am missing a subtlety. The authors essentially tune the rate of cloud building (tau1) and show that this affects the rate of cloud building (recovery). Why is this a surprise? The big question is what drives the slower recovery time. The predator-prey model, as far as I can tell, specifies this as an external parameter.

5. Figure 7. It is remarkable that during the period with the highest RWP (hour 25-26), the TKE remains unchanged, and only reduces when the RWP falls from its peak value. Could the TKE be preserved despite significant precipitation because of cold pool formation?

MINOR ISSUES:

1. P5555, line 10. Wood and Hartmann (2006) quantifies a number of important aspects of open and closed cells, including their aspect ratios, geographical distributions, meteorological situations etc.

2. P5560, line 23. What aspect of cloud formation is CCN limited? Are the authors referring to increased supersaturation and slowed condensation under low CCN conditions?

3. P5560, line 25. Didn't Pawlowska and Brenguier uncover a 1/N (not 1/sqrt(N) as

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stated here) dependence of precip on cloud drop concentration?

4. P5562, line 21-23. Subtropical marine stratocumulus tend to occur in regions with very dry free tropospheres, yet here we see recovery slowed down by entrainment of dry FT air. Thus, the statement that factors driving the rate of recover are the same as those driving cloudiness in general, seems to be a little questionable.

5. Why are the times in Fig. 7 given as >20 hours? I thought the simulations were about 18 hours long.

6. Section 4.2. Do the simulations with a dry FT in this section allow the low moisture to impact the PBL moisture budget upon entrainment?

7. Section 4.4. I didn't understand the significance of the mean vs variability LWP phase space. This seems to connect with another paper, but what is the point of showing it here?

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