Responses to Reviewer 1:

We thank the reviewer for a careful reading of the manuscript and helpful comments.

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.

Insights gleaned based on the Reviewer's other questions on the vertical stratification of the boundary layer (questions 2, 3, 5) shed some light on this question. We show in our responses below that the redistribution of TKE from predominantly top-down generated (closed cell) to bottom-up generated (open cell) is associated with both the duration and the magnitude of the N perturbation. Vertical mixing decreases with both duration and magnitude of the N perturbation and recovery therefore takes longer.

We concur that exploring recovery in a range of boundary layers, including much deeper ones, would be worthwhile. For now we believe that the insights gained from the vertical structure, and the existing test on a somewhat deeper boundary layer are sufficient to address this issue. (The current paper would need to be lengthened quite significantly if a rigorous analysis of a much deeper boundary layer were to be performed.)

We make it clear in the revised manuscript that the results pertain to a fairly shallow boundary layer and that recovery might change in deeper boundary layers. We surmise that recovery might be even slower in deeper boundary layers where the potential for vertical stratification is greater.

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 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).

We now support our arguments with a number of different analyses.
i) We point to the fact that the cooling also has to overcome the stratification generated by the precipitation. This issue is to some degree addressed by using smaller

perturbations to N, which result in weaker rainrates, and therefore weaker stabilization. It is also addressed by the analysis of the vertical TKE profiles in response to question 3.

ii) We have performed simulations with both shorter and longer duration N perturbation to explore this issue. Instead of a 4 h open cell duration we have experimented with 2 h 6 h, and 8 h durations (figure below).

Clearly the asymmetry in LWP manifests at all durations of perturbation, and is commensurate with the duration. This is in agreement with the lower precipitation rates caused by the weaker perturbations to N already shown in Figure 3. In the revised text we tie these issues together more carefully.

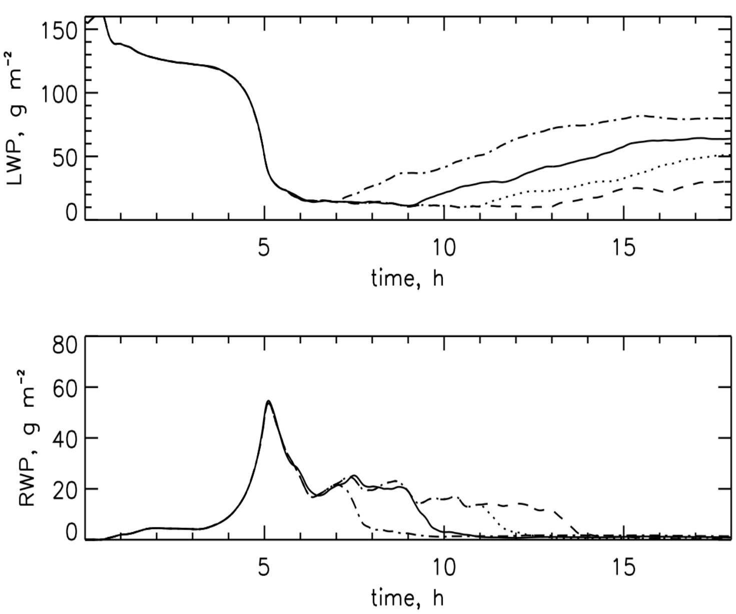
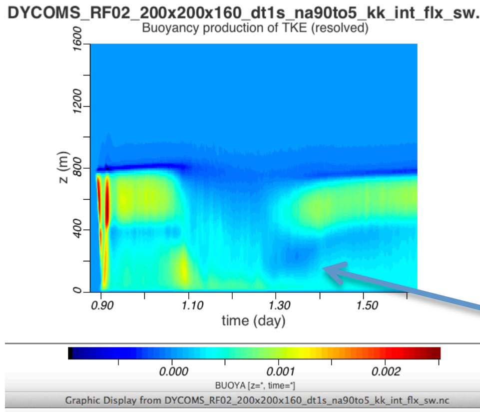


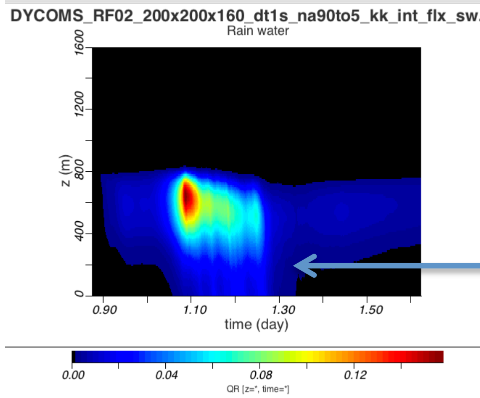
Figure: LWP and RWP time series for a 4h (solid line, as in Fig. 3), 6 h (dotted line), 8h (dashed line), and 2 h (dashed-dotted line). The change in N is 90/mg to 5/mg.

iii) We have analysed vertical profiles of TKE and buoyant production of TKE and found that in the transition from open to closed cellular convection, recovery is hampered by a layer of buoyancy consumption at roughly 300 m altitude, associated with the rain that persists into recovery. It is only after this region of buoyancy consumption peters out that the total water vapour flux can increase sufficiently so as to resupply moisture to the cloud.

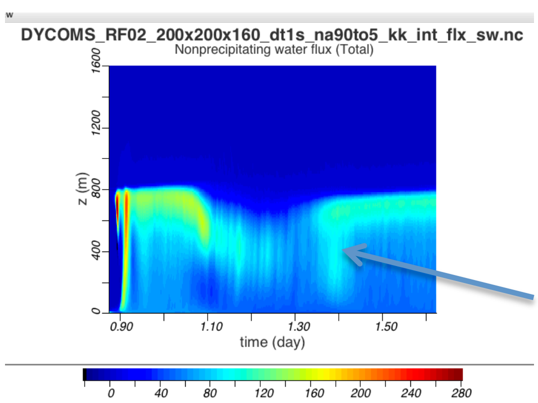
A new Fig. (see temporary snapshots below) is added and addressed along with Fig. 7. Note that we analysed a number of our other results and all point to this region of buoyancy consumption. There is also a clear relationship between the rate of recovery and the magnitude, and temporal and physical extent of the region of buoyancy consumption.



Negative buoyancy production (consumption)



Gradual cessation of low-level rain
(1.3d = 31.2h on Fig. 7; note this is when TKE starts to increase)



Increase in qt flux after buoyancy consumption is significantly reduced (1.4 d = 33.5 h on Fig. 7)

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?

TKE profiles are now explored for the standard GCSS case (Fig. 5).

We do this for two N perturbations (90→5→ 90/mg, left column and 90→35→90/mg, right column in figure below). One can clearly see how the larger N perturbation generates reduction in TKE over a deeper layer, along with weaker mixing in qt and thetal.

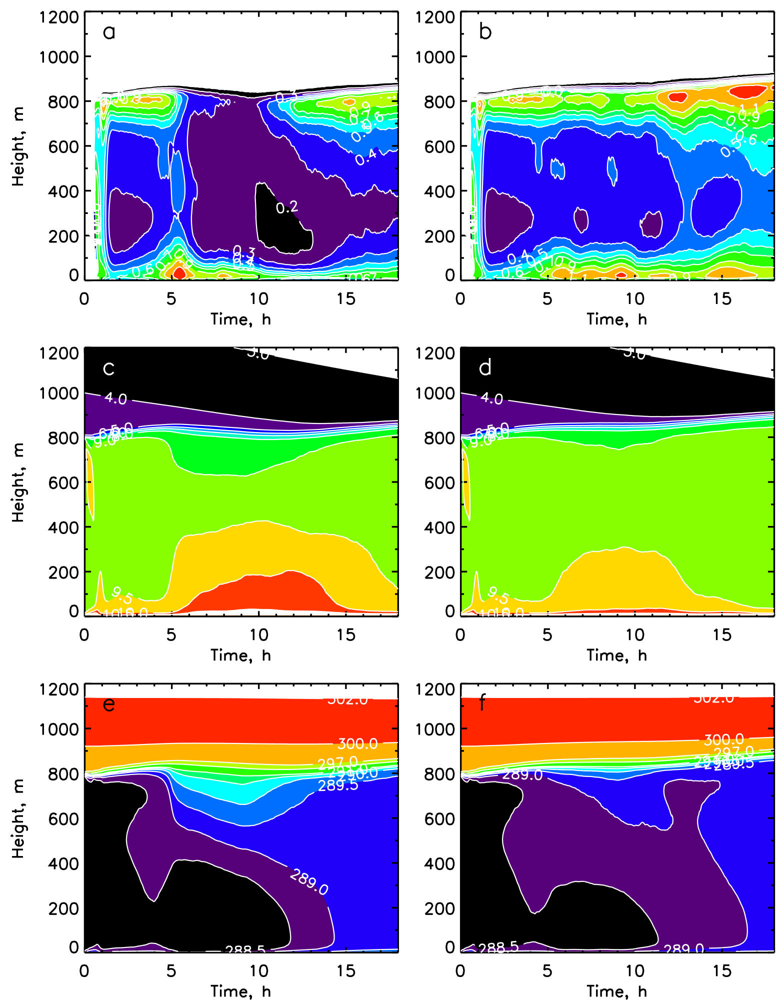


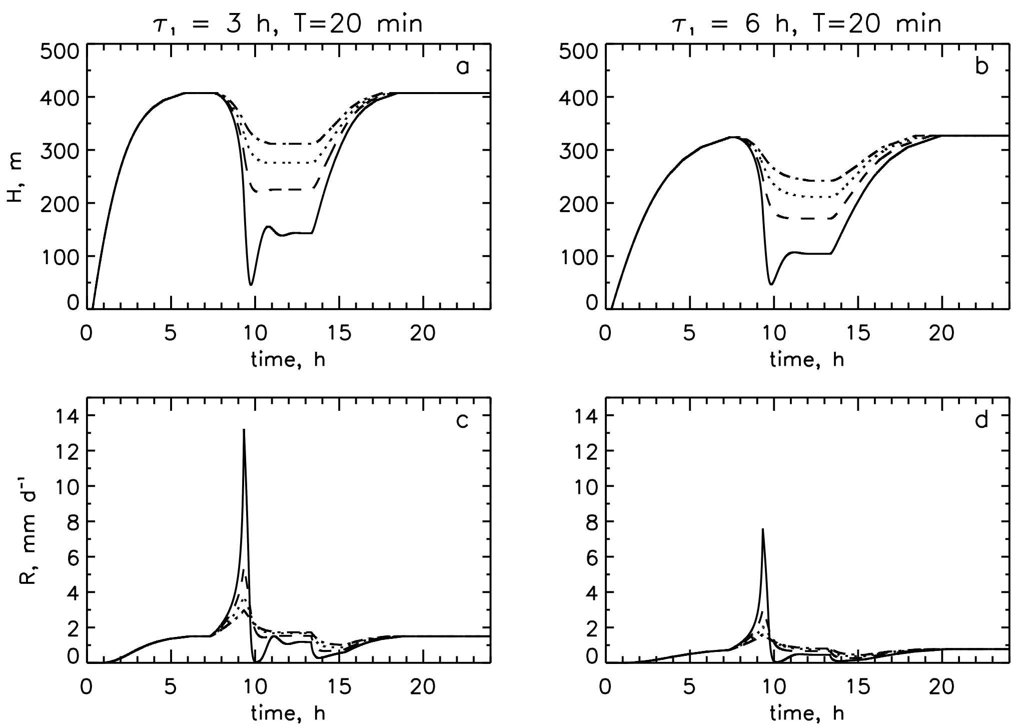
Figure: Profiles of (a, b) TKE, (c,d) qt and (e, f) thetal for left, the 90-to-5/mg perturbation (4 h) and right, the 90-to-35/mg perturbation (4 h).

We note that thetal and q profiles are shown in Fig. 5 for two of the cases (90→5→90/mg and no N perturbation) which already shows the reduction in vertical mixing associated with drizzle.

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.

The reviewer is correct that the rate of recovery is an external parameter and that the predator-prey equations do not address *what* drives recovery.

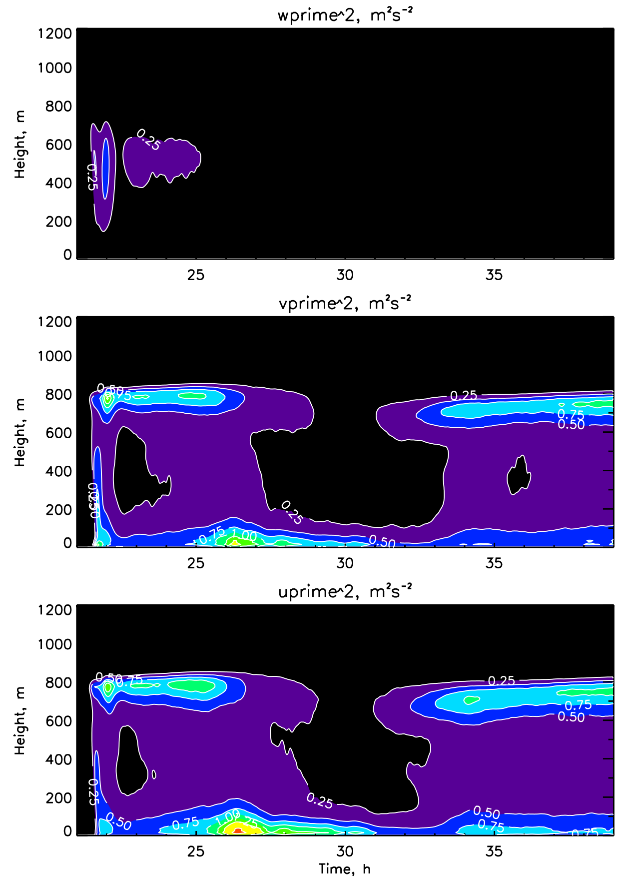
The predator-prey analysis is now expanded. First we show results for different levels of Delta N, as in the CRM results. Secondly, we show results for more realistic tau_1 (3 h and 6 h). Thirdly, we discuss how the delay terms in the equations create an inherent asymmetry in the system.



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?

The figures in response to questions 2 and 3 (above) serve to address this question. One can clearly see the shift from TKE maximum at cloud top during the closed cell period and the rapid shift to the surface upon transition to the open cell state (t ~ 5 h)
We show further analysis of the TKE and its contributions below.
We see how the TKE maximum shifts from a cloud top maximum in closed cell state followed by a rapid shift to a surface source on transition to the open state. The initial strong rain event drives strong surface TKE in the outflow, which slowly decays with time.

The TKE is broken into components: This analysis pertains to the analysis in current Fig. 7. All have the same color scale.



The heavily raining stage is one where cold pools form and maintain TKE through their interactions.

The revised Fig. 5, and new Figure discussing the recovery mechanism show TKE and buoyancy production of TKE profiles to illustrate this point.

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.

Reference added as suggested.

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?

This was the intent. However because the model doesn't represent CCN we have reworded the text.

3. P5560, line 25. Didn't Pawlowska and Brenguier uncover a 1/N (not 1/sqrt(N) as stated here) dependence of precip on cloud drop concentration?

Agreed. Our original intent was to use a published relationship as an example but we now make this more general.

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.

We now clarify this issue and revise the wording. We note that there are multiple aspects of a dry free troposphere that cannot easily be isolated. E.g., stronger radiative cooling (which enhances cloudiness) can be offset by entrainment (which may dilute cloud water). However, the latter depends on inversion strength and drizzle, amongst other factors.

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

The simulations starting at night that include shortwave forcing pass through midnight so that at t> 24 h, one needs to subtract 24 to get local time. Because there are occasions when simulations with and without shortwave radiation are compared (e.g., Fig. 11), we prefer to keep a simple time axis and alert the reader to this representation of time.

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?

No. This is stated in the original text.

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?

The point is to show yet another aspect of the asymmetry, namely one in which the relative dispersion of the "recovered" closed cell state is characterized by higher relative dispersion in LWP for a given mean LWP.