Response summary

We would like to thank both reviewers for their insightful comments and helpful recommendations to improve the manuscript. The manuscript has undergone significant changes, and figures in particular were changed to reduce file size and improve viewing. Several figures were removed from the manuscript.

Reviewer 2

General comments

1. There is no observational constraint whatsoever. It is a highly idealized modeling study, but given it appears to be loosely based on the DYCOMS-II period, the authors should be able to make some broad-brush comparisons with the observations.

Simulations with the model used in this study has been compared qualitatively and quantitatively whenever possible with observations in previous studies (Wang and Feingold, 2009 a, b; Wang et al., 2010; Kazil et al., 2011). In those studies, it was found to produce realistic closed- and open-cell states, as well as the transition from the former to the latter.

The primary goal of this study is to investigate, explain, and illustrate internal processes of the cloudy marine boundary layer using idealized simulations of closed and open cells. These internal processes of the MBL are, to our knowledge, currently not accessible by observational means, which warrants theoretical studies that can stand on their own. The simulations employ meteorological profiles observed during DYCOMS-II RF02, modified by deepening the boundary layer by 100 m to allow for a higher liquid water path, stronger cloud top cooling in the closed-cell state, and enhanced rain formation in the open-cell state. This information is now provided in the (revised) manuscript. The reason to modify DYCOMS-II RF02 meteorological profiles was that they were readily available; other meteorological profiles producing long-lived closed- and open-cell states would be equally suited. Given that not the original DYCOMS-II RF02 profiles are used, and that the focus of the study is to investigate processes that are, to our knowledge, not studied experimentally, no effort was made to reproduce DYCOMS-II RF02 observations.

2. The figures need substantial revisions. Many of my specific comments below address problems with the figures. Additionally, the sheer number of figures seems to distract from the main point of the paper. If there is any way for the authors to reduce the number of figures (currently 18), it would make for a much easier-to-read paper.

We concur; the number of figures was reduced by removing Figure 8 b, 11 b, d, f, 14, and 17 a and c, and the figures were modified to reduce file size and improve viewing.

Specific comments

1. Page 18856, lines 12-13. "The responsible mechanism is the entrainment of dry free tropospheric air into the boundary layer." This is not clear. I believe the authors mean the greater magnitude of entrainment in the closed-cell state, but this is not clear from the abstract alone.

We concur that the abstract does not explain the points of the manuscript in detail; detailed explanations are provided in the main text of the manuscript. The meaning of the passage in question is intended as written.

2. Page 18856, line 13. "The open-cell state drives oscillations..." should probably be "...is associated with oscillations..."

The corresponding passage was changed as suggested.

3. Page 18856, lines 22-26, sea-salt flux in depleted aerosol conditions may help maintain the open-cell state. This suggests that CCN concentration can become small enough, in the absence of surface CCN flux, to keep clouds from forming. Does this ever happen in nature?

The collapse of marine boundary layer stratocumulus clouds into a shallow fog layer due to an insufficient number of CCN has been proposed, based on model simulations, by Ackerman et al. (Dissipation of marine stratiform clouds and collapse of the marine boundary layer due to the depletion of cloud condensation nuclei by clouds, Science, 262, 226–229, 1993). To our knowledge, no observational studies have focused so far on this phenomenon. However, observations of ship tracks indicate that collapsed boundary layers do exist, and that additional aerosol from the ship exhaust re-initiates cloud formation therein. For example, Wood (Stratocumulus Clouds, Month. Weather Rev., 140, 8, 2373-2423, 2012) writes "It is interesting that the most spectacular ship tracks tend to form in collapsed boundary layers. These tracks are significantly elevated above the surrounding patchy clouds (Christensen and Stephens 2011), which suggests that increased colloidal stability can mitigate collapse and perhaps even regrow the boundary layer to a state that can support stratocumulus." We take this as indication that the answer to the question is "yes". A striking example of a boundary layer with aerosol concentrations that are too low for cloud formation, albeit in the Arctic, is given here: http://www.youtube.com/watch?v=EneDwu0HrVg. The observations were made by Mauritsen et al., An Arctic CCN-limited cloud-aerosol regime, Atmos. Chem. Phys., 11, 165-173, doi:10.5194/acp-11-165-2011, 2011.

4. Page 18857, line 8. Comma needed before "reflect."

Done.

5. Page 18861, Lines 16-19. So the aim is to represent open and closed cells over the northeast Pacific (NEP)? Although the case is highly idealized, this is an important distinction given the different character of MBL clouds over the NEP, SEP, and NEA. This should be acknowledged. The "11 July 2001" initialization date suggests that this is a DYCOMS-II case. Would the authors please expand upon this and give a bit more in the way of observational context?

The goal of this study is to investigate, explain, and illustrate internal processes of the cloudy marine boundary layer using idealized simulations of closed and open cells. A specific focus is internal processes which, to our knowledge, are not accessible by observational means. The simulations employ meteorological profiles observed during DYCOMS-II RF02, modified by deepening the boundary layer by 100 m to allow for a higher liquid water path, stronger cloud top cooling in the closed-cell state, and enhanced rain formation in the open-cell state. This information is now provided in the (revised) manuscript. The origin of the meteorological profiles and the modifications applied to them are acknowledged in the revised manuscript. Other meteorological profiles producing long-lived closed- and open-cell states would be equally suited. Given that the original DYCOMS-II RF02 profiles are not used, and that the focus of the study is to investigate processes for which, to our knowledge, an observational context is currently not available, no effort was made regarding the ability of the simulations to reproduce specific observations, or to address differences of closed- and open-cell states in meteorological conditions at different locations.

6. Page 18863, lines 4 onward. The authors might point out that neither of these simulations reaches steady state, despite the assertion in the introduction of the stationarity of closed cell circulations.

The reviewer raises an important distinction which needs to be made in the manuscript. The closed-cell state dynamics is stationary only in theoretical studies (e.g. Helfand and Kalnay, 1983; Shao and Randall, 1996). In nature, the circulation of the closed-cell state evolves, and is hence not stationary. However, it evolves much more slowly than the precipitating open-cell state: Koren and Feingold (Sci. Rep., 3, 2013) found that closed-cell fields maintain their tight spatial structure down to a resolution of a few km over the course of a day. After correcting for horizontal wind advection, the organisation of the field is approximately fixed in space. Hence the closed-cell state is stationary in good approximation on time scales of hours with respect to its cellular structure, although it may not be in steady state with respect to bulk quantities, such as LWP. The text was updated to reflect this.

7. Page 18865, lines 18-20, "...while the boundary layer air detrains into the free troposphere in the open-cell state." The only interpretation that can be made from this figure is that of the relative measures of entrainment rate and subsidence on boundary-layer depth. For the open-cell case means that entrainment rate is quite a bit smaller than the subsidence.

This is indeed a helpful comment – as we have not accounted for the entrainment by large-scale subsidence. The sentence

"*This means that free tropospheric air entrains into the boundary layer in the closed-cell state, while boundary layer air detrains into the free troposphere in the open-cell state.*"

was replaced with

"*When large scale subsidence is accounted for, the boundary layer entrains free tropospheric air in both simulations, however, on average at a higher rate in $S_\bullet$ and at a lower rate in $S_\circ$.*"

8. Page 18866, lines 12-19, latent heat release, leading the oscillations, followed by LWP, and the lag in rainfall. Shouldn't the LWP oscillations follow the latent heat oscillations exactly? It is not obvious to me why the latent heat leads the LWP slightly.

There is no compelling reason why latent heat release and LWP should peak concurrently. LWP is an accumulated quantity, while latent heat release is an instantaneous quantity: LWP can continue increasing after a peak in latent heat release, even in an individual updraft: assuming a constant water vapor content for simplicity, updraft velocity can peak early in the updraft's lifetime, with a corresponding peak in latent heat release. If the updraft carries on at a lower vertical velocity, with reduced latent heat release, LWP will keep accumulating and may peak at a later time.

However, an open question remains: why are the delay pattern between domain-averaged latent heat release and LWP robust? The peaks are not caused by individual updrafts, but by updrafts in different locations, which need not to be perfectly synchronized.

And how exactly is "latent heat release" calculated? Is it condensation rate minus evaporation rate over every model output interval?

Latent heat release is calculated in the model by the cloud microphysics routines as the heating of air by the condensation of water vapor. Cooling of air by evaporation of liquid water (latent heat uptake) is not considered, because it cannot serve as a diagnostic of cloud-forming dynamics.

Upon initially reading this section I wanted to ask the authors to please quantify the lag more rigorously, but later in the manuscript they call out Table 3, which answers much of my criticism. It would be helpful to in this paragraph at least mention that later in the paper they will be more completely discussing the lag between the various quantities.

Done.

9. Page 18867, lines 1-11. I am really struggling to see how adding a (ug, vg) vector of (1 m/s, -1 m/s) will transform Fig. 8a into 8b. Look, for example, at the cell at x = 60 km, y = 20 km. Fig. 8a shows a

relatively symmetric divergence signal. Adding the small (ug, vg) to this drastically changes the appearance of the vectors. I suspect something is amiss either with the calculation, or more likely, with the scaling of the vectors.

We have investigated this and the change in the length and direction of the vectors from Figure 8a to 8b is consistent with adding the north-westerly geostrophic wind of sqrt(2) m s^{-1}. To illustrate the fact, blow-ups of Fig. 8 (a, b) are shown here (Fig. R7). Please note that the reference vector represents different wind speeds in the two panels.

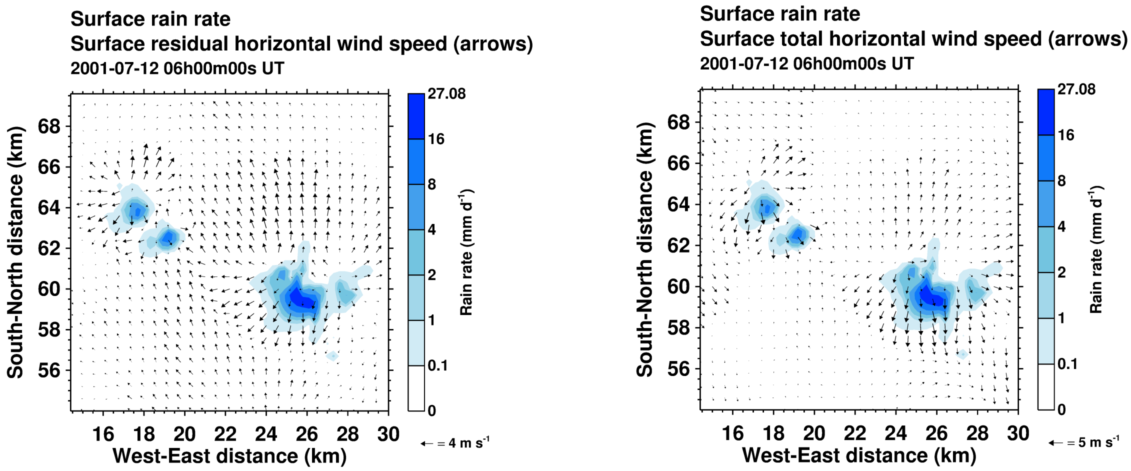

Fig. R7: Blow-up of Fig. 8 in the original manuscript.

In response to the request to reduce the number of figures, we have omitted Figure 8 b in the revised manuscript.

Comments on Figures

Fig. 2. This figure needs substantial revision. Having these four panels on one plot suggests the reader is supposed to be comparing them, so it's vital that the color table limits be identical in each. Or are they, except at large optical depths?

Indeed, the same color scheme is used in the panels of Figure 8. The appearance of being different is caused by different optical depth maxima in each panel. The reason for this approach was to provide information on the maximum value of the data in each plot in a convenient way.

In any case, it would be best to have a single color bar. In that case, you wouldn't need four color bars; one would suffice. The panels could be placed closer together and some of the redundant axis titles and/or labels omitted. Finally, why not put the S_open and S-closed, along with the time descriptors, at the top of each panel? That would be of great help to the reader.

Redundant information (color bars, figure titles, etc.) was removed from the figure.

Fig. 3. There's no compelling reason to have the right-side y-axis be identical to the left side. Omit the right side; color the left side in black.

Done.

Fig. 4. It would be greatly helpful to put S_open and S_closed symbols at appropriate places, and to label the top of the columns "Sensible heat flux" and "Latent heat flux." Also, I think the axis titles could be more concisely placed; at present they're too wordy and take up too much space.

Done.

Fig. 6. See comments above.

Done.

Fig. 7. See comments above.

Done.

Fig. 8. These panels have redundant bits (legends, y-axis titles/labels), and the flow features discussed in the text. I had to blow it up on my 27" monitor just to have a fighting chance at seeing the features in question.

Figure 8 was modified to be more easily viewable. One measure taken was to show only a selected region of the model domain, which ensures that wind field vectors are clearly visible. To accommodate the request to reduce the number of figures, Figure 8 b was removed, as it showed the actual (as opposed to the residual) wind field, which is not essential to convey the key points of the manuscript.

Fig. 9. Many of the criticisms of Fig. 2 (above) apply to this figure. The panels need more concise titles.

Figure 9 was modified to be more easily viewable: Select regions are shown instead of the entire model domain, and redundant information was removed from the figures.

Also, what is the reason for the odd contour intervals?

The contour intervals are placed at given percentiles of the data (e.g. on the 1,5,10, ... 90, 95, 99 percentile levels). This ensures a better representation of the variability in the data at low, intermediate, and high values, at the price of irregular contour levels. A corresponding explanation is now given in the caption.

Fig. 10. See comments on Fig. 9.

Figure 10 was modified to be more easily viewable: Select regions are shown instead of the entire model domain, and redundant information was removed from the figures.

Fig. 11. See comments on Fig. 9.

Figure 11 was modified to be more easily viewable; redundant information was removed from the figure. The entire domain is shown, as it is a key finding of this manuscript that the horizontal structure of the open-cell state creates a corresponding horizontal structure in surface properties.

Fig. 12. See previous comments about helpful panel titling and redundant panel components.

Redundant information (color bars, figure titles, etc.) was removed from the figure.

Fig. 13. (see also Fig. 3.) There's no compelling reason to have the right-side y-axis be identical to the left side. Omit the right side; color the left side in black.

Done.

Fig. 14. See comments of Fig. 8.

Figure 14 was removed as it only provided an illustration of the spatial distribution of what is shown in Figure 14 of the revised manuscript.

Figs. 15-18. Many of the comments from previous figures apply to these as well.

Corresponding changes were implemented in these figures.