Anonymous Referee #1

General Comments:
This study uses (primarily) a two dimensional model to explore the role of aerosol sources in the dynamics of cloud-topped marine boundary layers. It demonstrates the presence of multiple equilibria, and an important role for the aerosol in influencing the evolution of the boundary layer, at least under certain conditions. It is a very well written, interesting and novel study that, subject to a consideration of some of the points below (none of which are overly critical) merits publication in the ACP. I believe the editor can assess the extent to which the comments below are sufficiently addressed in a revision and have not further need to review a revised manuscript.

We thank the reviewer for their helpful and insightful comments, and address each point below. After repeating a number of runs on the Yellowstone supercomputer at NCAR CISL, we discovered a compiler compatibility issue with the NetCDF library on our system at UW that caused an artificial truncation of the precision of the absorption coefficients in some shortwave bands. This caused calculated radiative heating rates to be slightly too small in the cloud layer of our original simulations. All simulations have now been rerun for the revised version of this paper, altering many details of model evolution, but their qualitative behavior is consistent with the results as originally reported.

Specific comments:

- ***Main message***
  The study is a fascinating illustration of the coupling of aerosol source terms with boundary layer dynamics, but its main message was a bit lost on me. The introduction leads the reader toward the idea that bistability is being investigated, but this is only rejoined briefly at the end, and not conclusively. The manuscript could be improved if its abstract, introduction and conclusion were sharpened to better reflect was was learned, rather than what was done. In this respect some issues that I was left wondering about were:

  *To what extent is the coupling of the aerosol concentration decisive for the state of the boundary layer. Although there are clearly parts of phase space where it matters, how often are these parts of the phase space visited by nature. In the end POCs are fairly rare, and the authors show that some processes (broader regions of enhanced aerosol concentrations, the diurnal cycle) buffer against larger aerosol perturbations, and that the behavior they investigate is sensitively dependent on subsidence, and perhaps other parameters. Moreover, it remains unclear to what extent processes that are not well treated (as mentioned in next paragraph below) would either amplify or damp the types of feedbacks being investigated.

  - The main theme our paper seeks to illuminate is that of regimes and transitions of the coupled cloud-aerosol system. Here, a regime is characterized by a common morphology of cloud, precipitation, and boundary-layer structure, and dominant aerosol sources and sinks that can persist for a period of a day or longer under steady forcing. For a given forcing, we characterize the system evolution in terms of trajectories through an Na-zinv phase space, and different regimes correspond to different parts of this phase space. Even without a change in forcing, the system may naturally evolve across a regime boundary, causing it to undergo a regime transition. We address the reviewer's issues through this lens.

    In some parts of phase space, in which Na is large, the cloud is thin, and there is little precipitation, the time-scale for aerosol adjustment is longer than that for the boundary-layer and cloud evolution. In this case, the coupling is weak and probably of secondary importance to the evolution of a real subtropical cloud-topped boundary layer advected in the trade winds across gradients in SST and other forcings. For instance, the Sc to trade cumulus transition can be induced by thermodynamics (boundary layer deepening and cumulus-coupling) rather than microphysical processes sensitive to aerosol concentration. In other parts of phase space with more efficient loss of cloud water through precipitation, the aerosol adjustment time-scale is much shorter and the cloud-aerosol coupling becomes tighter, e. g. the transition into a POC regime Understanding the location of the transitions in phase space, as well as the aerosol budget term contributions that get the system to that point, can help explain what parts of this phase space real marine cloud-topped boundary layers tend to inhabit under what aerosol and large-scale forcing regimes.

* Is bistability a useful concept for thinking about marine boundary layers?

- Bistability is a useful idea, though not necessarily in the way originally intended by Baker and Charlson. We show that the system evolves within two distinct regimes, characterized by low aerosol and low albedo vs. high aerosol and high albedo. However, it is easy to find reasonable large-scale forcings and free-tropospheric aerosol values such that neither of them reaches a stable equilibrium in a small domain; they rather make up distinct parts of an unrealizably slow limit cycle. While the 3D run seems to reach a shallow, clean equilibrium much like that predicted by Baker and Charlson, it is unclear whether this would persist with smaller subsidence. In a larger domain, mutual feedbacks allow POCs, in which both regimes coexist in a statistically steady state. Bi-regime-ality, perhaps, is the more useful idea: a trajectory in phase space along which the system preferentially evolves through regimes, with sharp morphological transitions at specific times or positions.

*Or is really the point that if the dominant source of aerosol particles is entrainment from the free troposphere a positive feedback can ensue as described in this paper, i.e., the main point is simply to articulate this particular physical process. Here it would seem important to have a control run with a static aerosol concentration.

- The importance of FT aerosol in buffering boundary layer aerosol concentrations and behavior was first pointed out by Mechem (2006), and the importance of that feedback is confirmed in our study. In our framing, FT aerosol is an important parameter that affects the boundary-layer evolution through phase-space regimes, and is crucial in getting the system out of a collapsed regime, but other large-scale forcings, such as lower-tropospheric stability, subsidence rate, and wind speed, are also important to the cloud and aerosol evolution. Editing of the abstract, results and conclusion has sharpened the focus on this point.

All of these points are touched on, but I think the manuscript would be shaper if its contributions were better drawn out with respect to these (or other points it wants to make). Currently it makes the third point most strongly, but this is not really well set up in the introduction, or conclusions.

- We acknowledge that the abstract, introduction, and conclusions can be tightened to better address the results of our work; the final manuscript has been adjusted accordingly.

- ***Distorted Processes***

  Some assessment of how the processes which the modeling framework distorts is warranted. These include:

  - In the revision, we have addressed the reviewer's comments about simplified or distorted processes and their likely impact on our results, adding to the discussion of these issues already in the paper.

  *Free tropospheric nudging during collapse (is this warranted, and does it matter how it is done).

  - FT Nudging: Our framework is to look at the system evolution under constant forcing. The free-tropospheric temperature and moisture profiles being entrained into the MBL are a key part of that forcing. In reality they evolve due to both horizontal and vertical advection as well as radiative heating; we are using the nudging as a substitute for horizontal advection to keep these profiles constant, including when the boundary layer depth is decreasing. This is philosophically consistent; until there are more observational studies of very shallow open-ocean boundary-layers, it is hard to say what is realistic or warranted.

*Representation of shallow boundary layer, in particular shear driven mixing. Here the very sharp increase in entrainment at day nine or so, in the control run, was a bit of a puzzle to me.

- With the relatively coarse horizontal resolution, our model increasingly under-resolves the dominant eddies as the boundary layer shallows. That said, our computationally-driven choice of a 2D framework probably creates larger quantitative errors, even in this regime. Our goal is to qualitatively illustrate important couplings between aerosol, clouds and precipitation; our one 3D simulation suggests that the 2D approach may suffice for this purpose.

*Lack of background aerosol source term. This would create small particles, but these would grow, both directly and through cloud processing, to larger sized CCN, and it seems that the lack of this source (which would become stronger as the air becomes cleaner) might make the aerosol concentration more sensitive to boundary layer dynamics.

- We have acknowledged that the absence of an Aitken mode prevents us from capturing the breadth of the true aerosol distribution and the growth of small aerosol particles to CCN (see revised Sec. 2.1). While this eliminates a possible background source from nucleation or entrained Aitken mode aerosols growing through coagulation or chemistry, it is notable that observations of the RF06 POC (Wood et al., 2011) did not indicate large nucleation or Aitken mode concentrations in the PBL, and qualitative agreement between CCN concentrations in our model and observations is quite good. Without including these terms and testing for sensitivity, it is difficult to address the possible feedbacks, and this is outside the scope of the current work.

*Are the radiative effects of haze particles (deliquesed aerosol) considered? Don't get me wrong, I don't believe that these processes need to be explicilty incorporated in this study, but some discussion of why the results make sense in the absence of a proper treatment of these processes, or how they might be modified by a consideration of such processes, appears warranted.

- The radiative effects of deliquesced aerosol are not included in the present work. As aerosol optical depth is low and marine boundary layer CCN are mostly scattering aerosol (e. g. Allen et al. 2011), the radiative effects of aerosol on atmospheric heating are likely to be of secondary importance, and have not typically been included in comparable studies (e. g. Wang et al. 2010).

- ***LES terminology***

  This study does not employ large-eddy simulation for the bulk of its investigation. Only in one instance is LES used to assess the plausibility of the 2D model. Although there are different definitions of LES in the literature, there is really no definition which admits two dimensional simulations as Large-Eddy Simulation. Not only are the timescales of air parcel trajectories poorly represented (see e.g., Stevens et al., 1996), the distortion of the turbulent cascade in two dimensions means that the sub grid model is not scale adaptive in the way it is in three dimensions, .i.e., using standard approaches (as is done in the present manuscript) one expects the circulation strength to increase as the grid is refined, so the convergence properties of the model are very different than in LES. It would be better to say that a 2D Cloud Resolving Model is used to explore the coupling between aerosol sources and boundary layer dynamics. That said, this is really a question of terminology, not substance, that needs to be cleaned up.

  - We agree with the reviewer that the statistical properties of the turbulent cascade in the inertial range are substantially different between 2D and 3D. That said, there is no standard nomenclature for 2D boundary-layer simulations and in our view, 2D LES would be as accurate a descriptor as any (after all, we are simulating the large turbulent eddies and parameterizing the small ones, and for Sc-topped boundary layers, the all-important entrainment process is not resolved into the inertial range, resulting in strong resolution sensitivity even in 3D LES). However, we have revised the paper terminology describing 2D runs as those of a "2D cloud resolving model (CRM)" and the 3D run as "3D large eddy simulation (LES)".

- ***Tone***
  In many places the manuscript gave the impression of connecting black boxes, by discussing schemes by names, rather than physical content, i.e., saying that the Morrision Scheme is coupled to the Abdul-Razzak Activation, or saying that SAM does this. Throughout emphasis should be placed more on the physical content of the model that is being developed, some of these points are commented upon in the Technical Comments below.
    - To some extent, this is intentional, as we did not wish to focus on articulating the details of parameterizations that are already well known and described in the literature, but rather on the new aerosol parameterization presented and the resulting model behavior. Where requested in the technical comments, we have revised the paper to clarify the assumptions and construction of the parameterizations used.

Technical Comments:

1. The abstract is too long and descriptive. It should focus on what was done and what was learned.
   Agreed; the abstract has been edited accordingly.

2. P46,L04: The reference to Albrecht is incorrect, as it does not discuss a broad class of effects. See Stevens and Feingold (2009) for an overview of lifetime effects, and also one should not continue to neglect studies such as Ou and Liou (1989).
   The reference to Albrecht (1989) has been amended; references to Stevens and Feingold (2009) and Ou and Liou (1989) have been added for completeness.

3. P46,L22: The reference to the Ackerman collapse study is problematic because of fundamental flaws in the early one-dimensional models of aerosol-cloud interactions (see for instance Stevens et al., 1996 for a discussion of the issues.
   While early 1D studies (such as Ackerman et al., 1994) have errors and biases due to issues with the parameterized coupling between dynamics and microphysics, their flaws do not lessen their importance as first attempts at the problem.

4. P47,L28: Actually the early work on aerosol-cloud interactions was done in 2 and 3D by the CSU (Cotton, Feingold, Stevens) and OU (Lilly, Khairoutdinov, Kogan) groups.
   The CSU work is noted lower down (with the reference to Feingold et al., 1996 on 49L16); the chronology in the introduction as originally written is somewhat inconsistent, as an attempt was made to separate studies aimed at testing more detailed aerosol cloud interaction schemes from those that used higher resolution LES with specified microphysical changes to indirectly examine the impacts of microphysics on stratocumulus dynamics. The citation has been added for Kogan et al. (1995).

5. P49L25: I always associated the idea of one particle upon evaporation of a cloud drop with the work of the Mitra et al., (1992) although the Flossmann reference has precedence that seems to be an empirical study.
   Reference added for Mitra et al. (1992)

6. P51L04: "Sam partitions water". Sam does not have free will. At best, "in SAM the water is partitioned…"
   Rephrased as requested.

7. P52L19: The computational efficiency of the scheme is asserted, but has not been demonstrated.
   We assert that it is efficient by means of comparison to an explicit scheme in which a vastly larger number of scalars must be transported and interactions between bins calculated. A direct comparison between the bulk approach and a full explicit scheme seems unnecessary.

8. On pages 53-56 I found the introduction of the aerosol model, particularly Eqs 7-12, rather superfluous, the notation somewhat unwieldy (mp for microphysical processes, perhaps µp would be better notation,

and Fig. 1 not particularly informative. Perhaps this could be simplified.
The idea of eqs. 7-12 and Fig. 1 is to inform the reader of the pathways through which aerosol mass and number are transported and modified within the microphysics. Notation changed as requested.

9. A key issue in the model is also Eqs 1 and 2. The authors follow previous work, but this does not really clarify how well these relations capture the processes being considered. Put another way, where did Baker and Charlson go wrong?
The mass transport of aerosol by water species is somewhat unrelated to the main issues with Baker and Charlson. Baker and Charlson made overly simplistic parameterization of the loss term in response to microphysics, along with the dynamical simplifications of mixed layer theory and constant cloud top radiative flux divergence.

10. P55L01: Lewis and Schwartz (2004) seems like a relevant reference for the sea-spray.
While Lewis and Schwartz (2004) provides an encyclopedic review of sea salt aerosol production, this paper is more focused on the cloud-aerosol interactions than surface production, and the Clarke et al. (2006) scheme used to parameterize the surface aerosol flux includes this reference; it seems unnecessary to include it here.

11. P60L07: "A similar bifurcation between cloud thickening…" I am not sure I understand this sentence, it might have to do to not understanding what it is similar to.
The wording is indeed confusing; we merely meant to state that the behavior of the model is qualitatively similar when the diurnal cycle is included. Sentence revised.

12. P61L18: It would be helpful to be more specific, by identifying time periods for the three regimes.
Done.

13. P61L19: How is the reader to recognize the decoupling?
Decoupling is evident in the development of a surface layer with distinct aerosol and moisture properties, indicating the boundary layer is no longer well mixed. Wording to this effect added.

14. P62L16: "crash" seems like a suboptimal word choice. "
Crash" replaced with "sharp decline."

15. P63L09: "transition" is not clear in this context, do you mean the transition from a cumulus to a stratocumulus topped layer. It would be clearer if the transition you mean were more clearly spelled out.
The transition in question is one from a well-mixed, stratocumulus-topped boundary layer to a collapsing, decoupled boundary layer with fractional cumuliform cloudiness. This is spelled out in the revised text.

16. P68L01: Boundary layer overturning timescales are more order ten or tens of minutes, rather than minutes, i.e., zi=w* with w* about one.
Revised to "tens of minutes."

17. P70L01: Seems like a repetition of what was just said.
Revised to eliminate redundancy.

18. P73: The buffering ideas of Stevens and Feingold (2009) appear relevant here.
Reference to Stevens and Feingold (2009) added.

19. Fig10: The grey points (those at the most upper right, at least that is how they appeared on my printer) do not appear on the color scale. What are they?
    These points are the first two 12 hour period averages, which occur during the initial thermodynamic adjustment of the system onto the slow manifold. This is indicated in the caption to the figure. Similar text is added to the caption for Fig. 11.

20. Fig16: Indicate the notation OVC as overcast, currently it is implicit.
    Done.

Anonymous Referee #2

Summary
This paper aims to explore various stratocumulus cloud regimes under a range of different but constant external forcings using a LES model based on SAM with a novel single-mode bulk aerosol scheme. It examines each regime in the context of boundary layer aerosol-cloud-precipitation interactions (and free tropospheric aerosol-cloud interactions in some cases) and resulting boundary layer dynamics and structure. Later in the study, these interactions and their implications are nicely illustrated using a reduce-order phase-plane analysis, which highlights the important processes implicit to each sensitivity study. The paper is generally well written and its science is well-executed within the scope of the study. The outputs of this work are of importance to those researching boundary layer dynamics, cloud-aerosol microphysics and cloud-climate interactions amongst others. It represents another step forward and another point of discussion in understanding this very complex and important subject area. That said, the current document could be improved in places and I do have some questions about the setup and execution of the study (especially the single mode scheme and how it might affect the validity or realism of the model runs) and potentially some advice on how to better structure the paper to give maximum impact to the reader.

We thank the reviewer for their helpful and insightful comments, and address each point below. After repeating a number of runs on the Yellowstone supercomputer at NCAR CISL, we discovered a compiler compatibility issue with the NetCDF library on our system at UW. This bug impacted calculated radiative heating rates, which were too small in the cloud layer of our original simulations. This has quantitatively altered many details of model evolution, but the qualitative behavior is consistent with the results as originally reported. We have noted where this relates to the reviewer's questions.

General comments:

1. Abstract: This is a little confusing to read before having read the whole paper. The authors have tried to summarize too much here and the result is a confused attempt to present key observations from some (but not all) of the regimes studied. There is a detailed discussion on the evolution of the MBL in the control case which is best kept in the body of the paper. It tells the less focused reader very little that would help them to decide whether to read the paper in full and neither does it give the initiated reader sufficient information to capture the main message(s). This needs some thought.
   Agreed; the abstract has been edited and more tightly focused on the key results.

2. Section 4: Shouldn't a synopsis of the results come after the results themselves are presented?
   We agree this is out of place and perhaps redundant, and have removed it accordingly.

3. A summary of exactly which runs will be done (i.e. what range of NA and W) etc would be useful before they are presented. Perhaps a nice way to summarize the differences between each run and the key results from each would be a table?
   We believe Table 1 does this adequately

4. I became confused about what the control run was and how this differed from the other runs. I'll come back to this in the specific comments below.
   Terminology amended; rather than referring to the W5/NA100 runs as a control, we now refer to it by name. While it was the first run performed, aerosol processes are consistent throughout all runs, with the only variations between runs being in subsidence, initial aerosol concentration, inversion height, and the presence of a diurnal cycle.

5. End of Section 1. In the description of what will be tackled in each of the subsequent sections, a synopsis of the results is given. Again, this is not appropriate here. By the time we read the results section, we have already read a synopsis of the results (in varying degrees of detail) three times. Recommend removing any summary of the results until after they are presented.
   Agreed, revised as in 2.

Specific comments

1. P. 18145, line 8: A new para begins by suggesting that cloud microphysics and dynamics also affect aerosol. The end of the previous para discusses the second aerosol indirect effect. The new para implies a new discussion about something different but aren't you still discussing the second indirect effect here? Perhaps merge the paras and remove the first sentence of the new para.
   The first para introduces the idea of first and second aerosol indirect effects as the influence of the aerosol population on cloud microphysics and dynamics. The second para points out the coupled nature of cloud microphysics on the CCN population, which is not always made explicit in discussions of second indirect effects. As these are somewhat distinct ideas, we prefer to maintain them as separate paragraphs.

2. P. 18146, line 29: Mentions bistability will be addressed in the paper but I can't see that it has (at least not explicitly)? The paper does look at stable regimes and cycles but so far as I can tell, one of the conclusions of the paper is the strong sensitivity to small changes in modeled external forcing, i.e. that most regimes represent unstable equilibria (P. 18167, lines 10-5). If I have interpreted this conclusion correctly, this message should be made more abundantly clear in the abstract and conclusions and where relevant – it is an important and useful result.
   We agree that after introducing this concept, we did not adequately revisit the discussion. One byproduct of the compiler issue was the second equilibrium in our phase plane analysis of Fig. 11; this disappeared when repeating the runs with radiation functioning correctly, as less shortwave absorption at cloud top strengthens entrainment, allowing even the collapsed BL to return to the deep, well-mixed equilibrium. On the other hand, our extended 3D run was able to find a shallow equilibrium much like that predicted by Baker and Charlson (1990). For realistic, weaker subsidence forcing, it may be that no truly stable equilibrium exists. That said, the specific regimes, with characteristic dynamics, macrophysical properties, and aerosol budgets, take several days to evolve through, even when not ultimately in equilibrium. Thus a more useful concept is probably bi-regime-ality, not exactly bistability; we have emphasized this idea in the revised paper. Nonetheless, this behavior is very much in the spirit of what Baker and Charlson predicted.

3. Section 2.1. Single-mode scheme: I have some questions about this scheme. I realise it has been devised to minimize computation and optimize compatibility with the Morrison scheme; however there are some systematic issues with its formulation that I am worried could manifest themselves as apparent artificial aerosol feedbacks in the runs presented in this work. This single mode scheme is essentially a lognormal with a fixed width and mean radius fitted to a number size distribution derived from an empirical wind-driven relationship (where mean radius essentially scales as wind speed cubed). The resulting lognormal is scaled to ensure that N is always 50% of the total number calculated by this wind-driven (Clarke) relationship. Now herein lies a problem: the Clarke distribution will be rapidly skewed to larger sizes with increasing surface wind speed but the single mode scheme formulated here will always normalise N within a narrow width of 0.13um to 50% of the Clarke N. This could represent a source of systematic and unrealistic bias in the scheme and could relate to the discussion of the sensitivity to wind in Section 6.2. It

also imposes a normalization of Na, which could be very different from the Clarke Na in the same size range. In effect, this formulation convolves two fields (wind and Na) in an unrealistic way, also making it impossible to disentangle them reliably or correctly in the subsequent analysis.
Our reading of the Clarke et al. (2006) relationship is that there is a fixed number and mass flux, and the scaling with wind is due only to increasing whitecap fraction. Thus while flux increases with wind speed, mass and number fluxes scale equally, and the relation does not, as the reviewer suggests, skew towards larger sizes. While our simplistic method of shoe-horning the Clarke number and mass fluxes into a uni-modal, log-normal distribution doubtless has some issues, we believe the scaling of CCN flux with wind speed is sound.

4. Aerosol formulation continued: On a separate point to the above, and as noted by the authors, this scheme does not include new particle formation. This is an important point to keep in mind for the later analysis. In the ultra-clean regime, one would expect rapid new ultrafine particle formation, which could conceivably grow over the timescale of these runs and act to modulate or mitigate many of the cycles described (e.g. the control case after runaway preip scavenging and POCs). I guess a full analysis of this is beyond the scope of this paper but it would be remiss not to discuss its potential implications on the conclusions you make here. E.g. Could new particle formation mitigate the speed of the MBL collapse?
We agree that the absence of a background CCN source due to new particle formation and subsequent growth by chemistry and coagulation is a significant simplification of the real system. Observations of the aerosol characteristics in POCs (e.g. Petters et al., 2006 or Wood et al., 2011) are insufficient to really constrain the role of nucleation in POCs; while theory suggests it should be viable during the day within the POC's "ultra-clean layer," the fact that POCs are observed to persist over many days with coherent spatial structure (i.e. a region of the broader airmass that transitions to open-cellular convection upon POC formation is likely to remain open-cellular over subsequent days) suggests that any background source in precipitation scavenged regions is insufficient to restore the local CCN concentration against continued precipitation losses in a daily-mean sense. It is possible that some restoration of CCN could occur through nucleation and subsequent growth via coagulation and chemistry, but the feedbacks are likely second order and beyond the scope of this paper. Some additional discussion of potential feedbacks has been added to the revised text.

5. Aerosol formulation further continued: P. 18157, line 10-20: As the authors note, the Morrison Scheme adds Nd to keep droplet size distribution within empirically derived norms. This is accounted for by partitioning N in the new scheme used by the authors. The authors note that this can result in (rare) spurious source of total N when Nd exceeds Na as might be expected in the ultraclean regimes described in the study. This is apparently "kept track of" – can you expand on this and comment on when (how rare) and how it manifests and whether it could have any important implications?
We have instrumented the Morrison microphysics scheme to output statistics for both positive and negative contributions due to microphysical limiters. For instance, by careful examination of the budget term time series in Fig. 9 (d), small peaks on the order of 4-8 cm-3 day-1 appear during the collapsing phase of the simulation, after which accretion losses resume. While this is potentially indicative of an artificial oscillation driven by spurious aerosol number source from limiters that is subsequently removed by precipitation, it represents a smaller source term than both entrainment and surface fluxes, suggesting relatively weak influence on the overall number budget and simulation.

6. P. 18158, line 14: It is stated that the free tropospheric thermodynamic profile is nudged to its initial condition if the inversion shallows by more than 150m in a 1-hour timestep. Why was this necessary? What does such a nudge induce in the model and could it affect the new stability of the MBL after the nudge versus prior to the nudge? In other words, could you be artificially forcing the MBL to collapse?
We wish to find the equilibrium state of the PBL for the original idealized profile. The nudging method described in the paper avoids greatly disturbing circulations immediately above the inversion which may be important for entrainment. By design, it removes the effect of subsidence warming by maintaining a linear FT lapse rate. While present in the actual time evolution of the PBL, subsidence warming complicates the picture when searching for the equilibrium state of a given initial condition. Subsidence

warming would tend to strengthen the inversion, inhibiting entrainment, and making a steady equilibrium more difficult to achieve.

7. Throughout, the units of aerosol concentration (I think concentration) are expressed as mg-1. What is this? Do you mean cm-3 or is this some conversion to number per unit mass of dry air? I suspect it should be cm-3.
   Aerosol concentration is expressed as number per milligram of dry air, as this is the conserved quantity in a well-mixed boundary layer, and is essentially quantitatively equivalent to number per cubic centimeter.

8. Presumably, this could conceivable mean a sudden change in inversion strength at the MBL/FT interface, which could then affect what happens to the MBL depth subsequently?
   Answered in the response to 6.

9. P. 18159, line 4: I'm worried about the high subsidence rate that it was found necessary to impose in order to limit rapid onset of MBL deepening. This represents an important departure from the initial conditions used from the observed POC case. In other words, the initial inversion strength is maintained but subsidence is nudged to a higher value. Would this not represent an immediate disequilibrium that would result in the immediate shallowing of the MBL as is observed in the control case (but not observed in the RF06 case study). In other words, are you not inducing a large perturbation on the MBL/FT dynamic that would take many days to reach a new equilibrium (as the control case seems to show in the discussion on P. 18163), regardless of any aerosol-cloud interaction?
   It is important to note that the 2D framework will tend to exaggerate entrainment and thus somewhat distort the PBL energy budget. For the purposes of this qualitative study, various pilot runs were conducted until a subsidence value was found for which a stable equilibrium existed. The values used should not be quantitatively applied to make inferences regarding the real system; indeed, even in three dimensional simulation frameworks, model to model variability in entrainment rate remains significant, so quantitative inferences regarding subsidence, aerosol-cloud interactions, and system equilibria are shaky at best. Even if the real system were in equilibrium for the original sounding (highly unlikely, at any rate), one would indeed expect some boundary layer adjustment in the absence of any aerosol cloud interaction.

10. Sensitivity to FT aerosol: Section 6.1. This is interesting. What you appear to be saying is that without FT aerosol, the runaway precipitation sink causes transition to a collapsed MBL. But with FT aerosol at 100/cm3 with a 4.5. mm/s subsidence, a cloudy, deep MBL can be maintained. This is an important result and should be strongly highlighted. This process (if true) could explain POC formation by itself, if, for example, a polluted discrete FT layer is gradually entrained into the cloud layer, supporting a deep, cloudy MBL; but then suddenly disappears (after the diecrete FT layer is fully subsided/entrained), resulting in a transition.
    This is indeed our contention, though the timescale for cloud scavenging of interstitial aerosol to reduce the CCN population sufficiently to initiate precipitation is fairly long (days). As such, the variability of other forcing terms is likely to remain as or more important than the immediately available FT aerosol. It is interesting to note, however, that further simulations have shown the FT aerosol concentration affects the depth from which the collapsing boundary layer recovers (i.e. larger FT aerosol concentrations allow earlier recovery from a collapsed state; a PBL with no FT aerosol source remains collapsed to the end of the simulated three weeks).

11. Section 6.2: There is discussion of the reasons why the 2D model might not be producing a realistic surface wind speed during the diurnal insolation cycle. Couldn't this confirmed by a quick 3D run to make sure?
    Multi-day 3D simulations are quite expensive, and would need to capture at least one diurnal cycle after the PBL begins to collapse. While we will address this in future work, it is beyond the scope of the present paper.

12. P. 18167, line 1-5: This is an important result. Are you saying that MBL Sc cloud regimes are essentially unstable equilibria against typical variability in external forcings? This contradicts some other prevailing thinking that talks about bistability and stable Lorenz attractors. I would be inclined to agree with the unstable argument. You could reference the following papers as a study that discussed one such source of external forcing that appears to induce Sc transition:
Allen G, Vaughan G, Toniazzo T, Coe H, Connolly P, Yuter SE, Burleyson CD, Minnis P, Ayers JK. 2012. Gravity-wave-induced perturbations in marine stratocumulus. Q. J. R. Meteorol. Soc. DOI:10.1002/qj.1952
Connolly, P. J., Vaughan, G., Cook, P., Allen, G., Coe, H., Choularton, T. W., Dearden, C., and Hill, A.: Modelling the effects of gravity waves on stratocumulus clouds observed during VOCALS-UK, Atmos. Chem. Phys. Discuss., 13, 1717-1765, doi:10.5194/acpd-13-1717-2013, 2013.
Our work does indicate that there is sharp sensitivity in the system to external parameters. It remains unclear how much variability in the relevant meteorological and microphysical fields (Wls, qt, Na, etc.) is necessary, to which it is most sensitive, or to what extent the mechanism relies on the non-linear response to multiple simultaneous perturbations. While the system may ultimately be unconditionally unstable to separate mechanisms on a timescale of weeks (e.g. the Sc to trade Cu transition), the POC transition seems to involve timescales on the order of several hours, limited by the time required for precipitation to scavenge aerosol out of the layer. This remains an important area for further work.

Technical comments:

1. P.18144, line 18: What is inversion cloud? Not sure this is the best use of terminology.
   Revised for clarity.

2. P.18146, line 17: What does "…commonly-realized…" mean? Do you mean "typically observed"?
   Revised as suggested.

3. P. 18147: "…drizzliest…" Consider changing.
   Revised.

4. P. 18147, line 27: Change to "…elaborate contrived formulations…". Without this it could sound like you think the GCM's "elaborate formulations" are superior, which as most will know are not.
   Done.

5. P. 18150, line 15. Can anything be "slightly more complete"? Consider changing or removing sentence.
   Revised.
6. P. 18151m line 6 change to "…an identically…"
   Done.

7. P. 18153, line 6: Change "gasses" to "gases".
   Done.

8. P. 18145, line 20: change to "…refitted…"
   Done.

Anonymous Reviewer #3:

General Comments:

1. Very interesting results from 2D and 3D LES modeling of POC formation in Sc using improved but still simplified aerosol parameterization. Simulations are impressive and the results seem compelling. My only request would be to make the comparisons to past work (by Bretherton and others) more explicit, showing comparable results in graphs and tables for comparison. Similarly, the many references to qualitative similarities with obs would be much more useful if key plots from prior work were repeated here for comparison (on comparable scale) so that the reader can easily see what is described as qualitative similarity. This would be a much more useful contribution to the literature, and would help the reader to see specifically which improvements in this version of an ongoing series of similar simulations resulted in the more accurate simulation of the VOCALS results. I realize this is not always straightforward to do, but I am confident that the authors can do better job in doing this (since they know the important changes and how to highlight them whereas after reading this lengthy manuscript it is not clear to me). Since I realize that many things are published in ACP without such explicit comparisons to literature, I can only ask that the authors consider this a plea to help out the more novice reader by going a step beyond what is required – since this work clearly merits publication.
While we appreciate the referee's desire for a review paper covering the last decade of POC observations and modeling, this is outside the scope of the current paper, though we have comprehensively cited the relevant literature. Our work is intended as a research article specifically examining the slow evolution of the PBL, subject to cloud-aerosol interactions and without variation of external forcing; the existence of aerosol-cloud-precipitation interactions make rapid regime shifts possible in certain parts of phase space. We apply this concept to POCs as a demonstration of the PBL simultaneously supporting both regimes, and that by virtue of their interactions, the combination of both regimes is more stable than either in isolation.

2. Also, I think this paper concludes that POCs are formed by horizontal heterogeneity in the spatial distribution of aerosols. Is that only a sufficient condition or is it necessary? Or does that require further work to explore. I ask since this is an interesting point that merits further study.
We have shown that in our 2D CRM framework, heterogeneous spatial distribution of aerosol number is a sufficient condition to form a POC. The work of Wang et al. (2010) investigated other forms of perturbations, including moisture, temperature, and surface flux anomalies, and showed that these are also capable of forming a POC. It is likely that POC formation in the real world results from the combined effects of perturbations in multiple dynamical and microphysical fields. Further work in this area will be necessary to fully untangle the system behavior.

3. Abstract: This reads more like "what we did" rather than "what we learned". I recommend shortening and focusing on the latter, to highlight: (1) improved aerosol scheme and scavenging, (2) qualitative consistency with some features of B+C oscillations, (3) aerosol gradients necessary/sufficient for POC formation, (4) constant forcing for persistence.
Agreed; the abstract has been revised and tightened as requested.

4. Section 3.2 in order to make it possible compare directly to published msmts, provide number concentrations in #/cc.
#/mg of dry air roughly (within 10%) translates to #/cc within the cloud layer; the value also has the benefit of remaining a consistent measurement throughout a well-mixed layer, whereas #/cc is not.

5. Section 6.2 it would be good to justify more explicitly why the 2D winds do not have a major impact on results rather than just saying "we don't think" it does.
The diurnal cycle of surface wind in the simulations including a diurnal cycle tends to weaken aerosol surface fluxes during the day, simultaneous with the reduction in cloud LWP and lower aerosol scavenging rates. A weaker diurnal cycle in surface wind speed would increase the ability of the surface aerosol flux to buffer cloud and precipitation scavenging, possibly shifting the regime transition to higher diurnally

averaged LWP. While the computational resources to confirm this are unavailable and it is left for future work, text describing this possible sensitivity has been added.

6. Incorrect: "Thus we hypothesis…"
   Fixed.

7. Fig. 17 – how is this different from standard parameterizations? Would be more useful to overlay with such standard isopleths (e.g. Seinfeld and Pandis) so that differences are evident. Otherwise it is not clear why it is needed.
   This figure was merely included to illustrate the sensitivity of scavenging as a function of aerosol and collector drop size.

Specific Comments:

1. The abstract was hard to understand. It sounded more like a large summery, but I could not follow much of it until after reading the entire paper.
   Agreed; revisions as in (3) above have addressed this.

2. In line 10 of P.18147 it says "Satellite observations showed POCs form preferentially in the early morning…" I think 'early morning' is open to interpretation. Early morning could be just before or just after sunrise, which are completely different meteorological conditions.
   We agree "early morning" lacks specificity; the cited Wood et al. (2008) study showed POCs tend to form a few hours before sunrise. Wording has been changed for clarity.

3. It seems counterintuitive to call an equation "liquid-ice static energy" but then neglect ice. (line 1 of P.18152)
   Agree this is somewhat awkward; the model conserves liquid-ice static energy, but no ice is present in our model due to the warm clouds considered and the microphysics used.

4. There are several sources referred to throughout the introduction with similar results that I feel could be better consolidated. [ex. POC observations in "ultraclean layer" (line 8-10 of P.18147), "…found increased open-cellular organization with decreasing cloud droplet or aerosol concentration…" (line 10 – 11 of P.18148), "…development of open-cell organization smoothly increased as initial CCN Decreased…" (line 6 of P.18149)] Currently these similar ideas are spread across a few pages.
   The ordering of the introduction attempts to list work both chronologically and by complexity of the aerosol scheme used. The increase of open cellular organization/decrease in cloud fraction with decreasing aerosol concentration is true even for the lowest complexity approach (fixed aerosol or cloud droplet concentration), so the idea appears in the context of studies with different levels of complexity.

5. Sigma_g is not really defined, I am a little confused as to what it is.
   Sigma_g is the width parameter for the log-normal distribution. The wording in section 2.1 has been improved to clarify this.

6. I am not sure how Autoconversion is used here. Doing a quick search gives several definitions, so it would be great if the authors specified their use.
   In our usage (and the Morrison microphysics scheme, autoconversion is the interaction between cloud mode droplets (radii less than 25 microns) yielding a droplet with radius larger than 25 microns, moving the resulting droplet into the rain mode. This definition has been included in section 2.1.

Technical Comments:

1. The mention Na in line 5 of P.18146 before defining it (in line9-10)
   Definition moved up.

2. I do not think drizzliest is a word. (line 11 of P.18147)
   Changed to "most heavily drizzling."

3. There are several locations in which 'an' is used when 'a' should be used.
   Fixed.

4. Varying the hygroscopicity of the aerosols might be interesting. Specifically, for a POC case. In the POC cases they vary the horizontal number concentration of aerosol. In a marine environment, I am not sure how a horizontal aerosol distribution like the one mentioned could develop. I would expect some variation in sources to cause this, and a variation in sources between regions, might also mean a variation in hygroscopicity between aerosols emitted.
   In our present scheme where only one dry aerosol species is used, variation of hygroscopicity cannot easily be included. In practice, variations in hygroscopicity would act somewhat like the variations in aerosol shown here, as the result would be differing cloud droplet concentrations due to differences in activation. This could be explored with future modifications to the scheme to incorporate additional dry modes, though this is not planned at the current time.