Referee's Report: Marine boundary layer cloud regimes ...

A.H. Berner

July 31, 2013

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.

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

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.

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

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

LES 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.

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.

Technical Comments:

1. The abstract is too long and descriptive. It should focus on what was done and what was learned.
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).
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.

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.

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.

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

7. P52L19: The computational efficiency of the scheme is asserted, but has not been demonstrated.

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.

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?

10. P55L01: Lewis and Schwartz (2004) seems like a relevant reference for the sea-spray.

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.

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

13. P61L19: How is the reader to recognize the decoupling?

14. P62L16: "crash" seems like a suboptimal word choice.

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.

16. P68L01: Boundary layer overturning timescales are more order ten or tens of minutes, rather than minutes, i.e., $z_i/w*$ with $w*$ about one.

17. P70L01: Seems like a repetition of what was just said.

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

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?

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