Atmos. Chem. Phys. Discuss., 13, C5810–C5815, 2013
www.atmos-chem-phys-discuss.net/13/C5810/2013/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Marine boundary layer cloud regimes and POC formation in an LES coupled to a bulk aerosol scheme" *by* A. H. Berner et al.

Anonymous Referee #2

Received and published: 12 August 2013

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.

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 focussed 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.

2/ Section 4: Shouldn't a synopsis of the results come after the results themselves are presented?

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?

4/ 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.

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 really still discussing the second indirect effect here? Perhaps merge the paras and remove the first sentence of the new para.

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 modelled 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.

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

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 in the real atmosphere?

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 aerosol scheme. 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?

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 for any reason? Presumably, this could conceivably mean a sudden change in inversion strength at the MBL/FT interface, which could then affect what happens to the MBL depth subsequently?

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.

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. In other words, the initial condition inversion strength is maintained but subsidence is nudged to a higher value (and a very high subsidence climatologically). Could this 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?

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.

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?

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.

Technical comments:

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

P.18146, line 17: What does "...commonly-realized..." mean? Do you mean "typically observed"?

P. 18147: "...drizzliest..." what a horrid word. Consider changing.

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.

P. 18150, line 15. Can anything be "slightly more complete"? Consider changing or removing sentence.

P. 18151m line 6 change to "...an identically..."

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

P. 18145, line 20: change to "...refitted..."

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 18143, 2013.