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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "On the reversibility of transitions between closed and open cellular convection" *by* G. Feingold et al.

Anonymous Referee #2

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Overall

This study of transitions from closed to open and back to closed cellular convection in idealized simulations of the cloud-topped marine boundary layer shows that the response to suddenly decreasing cloud droplet concentrations is much faster than the response to suddenly increasing them. The abstract and conclusions are supported by the evidence provided, for the most part. The main text is not particularly tight and a few sections are problematic. Before it is acceptable for publication, the manuscript needs a number of revisions, as described in detail below, in order of their appearance.

Specific comments

1. Abstract: "Sysyphusian" is not a word. Perhaps the authors meant "Sisyphean"?

But that term refers to a task that cannot be completed, which does not apply here because (a) there is no agent performing a task here, and (b) the system does completely recover from the open cellular convection. I would use a term that is consistent with the study's findings, instead.

2. p 5556, l 22: The claim that avoiding aerosol entirely and instead directly controlling cloud droplet concentrations, "allows a more direct assessment of the importance of the rates of aerosol removal and replenishment" does not make sense and needs clarification. How is it that bypassing aerosol completely allows for assessment of aerosol sinks and sources (which are never assessed)? This sentence would make sense if "does not" were inserted before "allows".

3. p 5557, l 5: I would say "SAM solves the anelastic equations" or so rather than the confusing statement that "SAM is an anelastic system". Note that it also provides an option to solve the Boussinesq equations for shallow convection in LES mode according to the paper cited.

4. p 5557 l 16: "Grid size" should be replaced with "Grid spacing" or so and "smaller...grids" should be replaced with "finer...grids" or so, since the term "small grid" and "grid size" describe the size of a grid, not mesh refinement.

5. p. 5558, l 9: "Rainrate" is not a word. Also, is the rain rate defined at the surface or cloud base or what?

6. p. 5559, l 17: The notation "m gˆ-1" means "meters per gram" where the authors certainly intended "mgˆ-1", meaning "per milligram". This notational error pervades the text and figure captions.

7. p 5560, l 5: Given that LWP includes cloud water and rainwater, the modifier "cloud" before "liquid water" should be omitted.

8. p 5560, l 16: The term "commensurate" does not fit here. "Incommensurate" would be closer to what is being described, but I'd rephrase and pick another term entirely.

9. p. 5560, l 23: "Cloud formation is CCN-limited" seems odd here, since there are no CCN in the simulations and clouds apparently form just fine in the simulations even at extremely low cloud droplet concentrations of 5/mg. Some rephrasing or omission is needed.

10. p 5560, l 25: It is stated that R goes as LWPˆ1.5/sqrt(N) as if that were some universally-accepted relationship. It's not. It might be interesting to show how the results here compare with that relationship, though.

11. p. 5560, l 27: I would define ambiguous terms upon their first mention, such as f_c, z_i, and z_b, which can be defined in many ways. I would provide the definitions used in the analysis here.

12. p. 5561, l 18: The "left panel" is referred to but there are three of them. Perhaps "left column of panels"?

13. p. 5561, l 19: It would be helpful to note after stating that the cloud layer warms that one can figure that out by noticing that theta_l is steady while q_l decreases, which implies that theta must have increased.

14. p. 5561, l 21: The interpretation seems to imply that the 9.5 g/kg isosurface marks the top of the near-surface layer. The thinking is unclear.

15. p. 5562, l 6: "Largescale" is not a word.

16. The first paragraph of Section 3.2, which is attempt to explain the relationship between LWP, precipitation, and TKE, could use a good bit more attention and clarification so that it becomes clear and that physical understanding is effectively conveyed.

17. p. 5563, l 1: Conceptual elements are missing from the assertion that precipitation reaching the surface cools the surface and warms the cloud layer, because the statement does not make sense as presented.

18. p. 5563, l 8: When stating "LWP drives production of TKE" it would be helpful to

note that there is a positive feedback at work in which TKE also supports LWP.

19. p. 5563, l 10: It would be helpful to explain why there is a roughly 1-h delay between LWP decreasing and the drop in TKE.

20. p. 5553, l 17: It is stated that the phase space trajectory "nicely" shows a limit cycle, but it does not. The very essence of a limit cycle is that a trajectory is closed, but the trajectory that is shown is open. High concepts are great, in principle, but readers may question their value when casual inspection reveals that they don't actually fit the evidence provided.

21. Section 3.2.2: It is unclear why the authors choose to increase the surface sensible heat flux with a goal of accelerating recovery. Increased sensible heat flux should reduce the relative humidity of the boundary layer and instead of generating thicker cloud, as mentioned on line 13, should generate thinner cloud, no? Or another angle – the authors seem to understand that increased radiative cooling is needed for the system to recover. Increased radiative cooling is removal of sensible heat from the system, working in the opposite direction of a "strong influx of energy" mentioned on line 8. So it seems to me that the entire notion of attempting to accelerate recovery by adding sensible heat is backwards, and it should only serve to slow down recovery.

22. p. 5564, l 15: It is stated that "higher SH and LH are typically as [sic] drivers of open-cell formation" but aren't changes in sensible and latent heat fluxes the result of other changes associated with open-cell circulations, rather than drivers? Otherwise, open cells could be generated by simply increasing SH and LH fluxes. Can they? Furthermore, if higher latent heat fluxes are drivers of open-cell formation, how does that conform with open cells being associated with lower latent heat fluxes in fig 4b?

23. The foregoing issues regarding surface heat fluxes also appear in the abstract and conclusions.

24. Section 4.1 contributes no understanding to this reader and the manuscript would

benefit from omitting it. Either that or it needs to be fleshed out and tied into the rest of the study in a manner that adds value and conveys understanding.

25. The rain rates for the predator-prey model seen in fig 9 are greater than those for the CRM by orders of magnitude, yet this is never even noted, let alone remarked upon. Seems like the dynamic regime of the predator-prey model is very different from that of the CRM simulations. Given such an adjustable model, the authors should either adjust it to be consistent with the CRM simulations or explain why that is impossible.

26. Section 4.2: The authors' understanding of the purpose of Beer's law longwave parameterization does not make sense to me. The reason it is used in model intercomparisons is to reduce possible sources of discrepancy between models, which typically use different radiative transfer schemes. The notion implied here that the Beer's law treatment represents an alternative treatment to real radiative transfer is very much off-target. The Beer's law treatment provides a small number of adjustable parameters that Larson et al. (2007) have shown allow it to reproduce the heating rates from real radiative transfer models. So if the authors find that the Beer's law formulation does not produce heating rates that are comparable to those with their radiative transfer model, that just shows that the authors failed to tune the adjustable parameters so that the rates are comparable. Used properly (which means tuning the adjustable parameters to reasonably match the heating rates given the conditions input to a real radiative transfer model), the only disadvantage of a Beer's law formulation in this context is that it is not set up to readily compute solar heating. It should be stated that such an extension would not be difficult, and the reasoning for not doing so provided.

27. The authors' claim that there may be some biases for the Beer's law formulation for broken clouds, even though it is being used with the independent column approximation. But RRTM is also being used with the same approximation. Why would there be any bias if both approaches are using the same treatment to treat horizontal heterogeneity?

28. Instead of, or in addition to, stating the specific humidity used for the free troposphere in RRTM, it would be helpful to provide the overlying column of water vapor, which is more physically relevant.

29. Appendix: "Grid size" should be "grid spacing" or so. Also, it should be stated whether or not the domain size is fixed for these tests.

30. Panel labels are far too small in fig 2.

31. The surface precipitation rate shown in fig 3 is about a factor of five smaller than the average value measured in the open-cells for this case. This discrepancy should be noted and the implications discussed.

32. There is a units problem in the equation provided in the fig 6 caption.

33. The "domain and boundary-layer average" mentioned in the fig 7 caption is confusing. Surely the domain is deeper than the boundary layer, so this description does not make sense.

34. The legend, which appears to show grid sizes (numbers without units would seem to indicated that what is referred to is the number of grid cells), evidently conflicts with the description in the main text. A more complete figure caption might help.

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