Review of „Effects of Intermittent Aerosol Forcing on the Stratocumulus-to-Cumulus Transition“ by Prabhakaran et al.

This paper assesses how conceivable MCB strategies may influence stratocumulus-to-cumulus transitions (SCT). Different seeding strategies are explored here. The study is scientifically sound and includes findings of great interest to the community. My main concern with respect to their methodology is the use of a climatological forcing, which linearises the system to an extent, that may not be valid. However, this limitation is clearly acknowledged by the authors. Remaining concerns listed below are with respect to the interpretation of their Na=50mg-1 experiments, and the occasional lack of transparency of presented arguments.
However, overall it is a well-written manuscript and I recomment publication with minor revisions.

General:

You chose your experiment with a fixed seeding geometry with respect to the domain. However, this means that the orientation of the seeding source will constantly change with respect to the wind direction inside the domain, right? Is there any impact of large-scale advection on top of the mesoscale organisation driving lateral mixing of aerosol?

Section 3.2: I would like the authors to add more context ot this set of experiments. Currently, the results are presented as investigating the sensitivity of an SCT under lower background aerosol concentrations. This is also consistent with the manuscript title. However, Fig. 6 suggests, that you are looking at this in a quite different system to start with. The transition seems to have already occurred before the analysis and indeed the first aerosol injection. Was there even a transition prior to hour 10? I am also not sure how good an analogue it is for a post-transition type of experiment, since the simulated boundary layer you start with is quite shallow. This is also neccessary for context of interpretation. How often do such shallow cumulus layers occur so close to the coast and thus how relevant is it for understanding the effect of potential MCB applications?

Section 3.2.1: Following on from my previous comment. While I scientifically do not object to the analysis and mechansim presented, it is shown in a boundary layer, whichs representativeness is questionable (previous comment). Would you expect to see the same in deeper cumulus-topped BLs as well? Once again, this is pointing towards relevance and generalisability of your finding. Also, please clarify in the text the novelty/added value of your findings to the previous mechanism presented in Wang & Feingold 2009b?

Please explain why different sprayer configurations and specificiations were performed in Na=150mg-1 and Na=50mg-1 experiments. Why was 8.6x-100 introduced but 5x-220 omitted?

Specific:

L25ff: I don’t disagree with the reviewers that precipitation may play a role in accelerating the Stratocumulus-cumulus transition. Howevever, observation-based assessments also support the notion that stratocumululs-cumulus transitions are predominantly driven by entrainment rather than precipitation (e.g. Bretherton et al. 2019, Eastman et al 2021). It seems misleading to attribute these differences simply to the neglect of aerosol-cloud interactions in numerical models.

Reference: Eastman, R., I. L. McCoy, and R. Wood, 2021: Environmental and Internal Controls on Lagrangian Transitions from Closed Cell Mesoscale Cellular Convection over Subtropical Oceans. *J. Atmos. Sci.*, **78**, 2367–2383, https://doi.org/10.1175/JAS-D-20-0277.1.
Bretherton, C. S., and Coauthors, 2019: Cloud, Aerosol, and Boundary Layer Structure across the Northeast Pacific Stratocumulus–Cumulus Transition as Observed during CSET. *Mon. Wea. Rev.*, **147**, 2083–2103, https://doi.org/10.1175/MWR-D-18-0281.1.

L120: suggest rephrase „during each pulse“ -> „during each of the 3 pulses“. Or something like this. It took me a minute to understand why there are 3 numbers behind the hyphon.

L127: At which altitude to you spray?

L137: Please explain why the TKE is used here as a metric for lateral dispersion. I would have thought that the length-scale of mesoscale organisation may be a better constraint?

L145ff: I agree with the authors on this. The fact that you reach an area fraction of 1 suggests, that your results in the 5x scenarios are likely limited by domain size. This reduction of the analysis to ship track scales, should be made more clearly in the abstract. The current first sentence is ambiguous in this regard. This holds especially for all conclusions based on the Na=50mg-1 background experiments discussed in the following section.

L157: „weakening of the updrafts (Fig. 3). Where do I see this in Fig. 3? What is included in your metric of TKE (i.e. subgrid and grid-scale components)

L166ff: Why is the deepening during the first days a function of the difference in Na across the inversion in your simulations? This is not clear. Is it not rather that incloud Nd is not increased that much in the 1x experiments, such that entrainment feedbacks are not effective?

L175ff: Your explanations of the observed behaviour in LWP and fc in seeded experiments lack evidence. Please provide this (at least as part of this review).

L184: I agree with your statement, but please define the „onset of the transition“.

Fig4: Please add injection times like in Fig. 2. Hours 5, 13, 21 are unconventional. Why were they chosen? Also maybe you could add a timeline for shortwave absorption here to also provide partial proof for your arguments presented earlier (i.e. comment on L175ff).
Your time labels in Fig. 4b (i.e. starting at day 0) are different to the rest of the paper. Please adjust. Generally, each figure caption showing temporal slices with a periodic time axis should state the starting day for clarity. I also believe the day0 label in Fig.4b refers to the period marked under „day 1“ in Figure 2 and throughout the manuscript.

L189ff: I am not sure that your metric of BFI is really so usefull to diagnose decoupling in this case. For instance you simulate low BFI in periods following day 2 h12 (as low in the first 24 coupled hours) while the profile of Na+Nd clearly indicate decoupling. Other metrics have previously been suggested in the literature that may show this more clearly?

Fig5: The shift im time-axis from day-to-day (i.e. first 10, 20, 6h,16,02,...) makes it hard to look at the periodicity of the cloud-radiative effect. I would suggest a uniform periodic time axis.

L288: „changes in fc and are proportional to net injected aerosol concentration“ The term „net injected“ I find misleading. There is no sink in an injection. I would rephrase. Also, where do I see this?

Fig. 8. Same questions and comments apply as in Fig. 4.

L348: rephrase „affected mainly by the total number of injected particles“ to „ affected mainly by the total number of injected particles prior to the transition“. I beleive your statement only holds under these conditions, as only then the number of injected particles is the same. Otherwise, this statement cannot be true, as temporarily the number of injected aerosol will by design be different between 1x-101 and 1x110.

L367: „earlier transition“ what transition? It already starts out in the cumulus state in 0x-000 right? I think the suggested definition of the transition in an earlier comment can help clarify this point.

L391: According to table 1 there is no saturation in the Twomey effect though. There is a ~20 W/m2 difference between 5x-200 and 5x-220 on day 3.