

The authors present a comprehensive study of convective aggregation in the relatively simple dynamical framework of the shallow water equations. At the core of this simple model of convective aggregation is an assumed bistability of the RCE state which tends to be driven towards two different stable equilibrium states, one moist and one dry. Having been brought on as a reviewer in this second round, I feel that the authors have adequately addressed the concerns over framing (and in particular the relevance of the study to the MJO) raised in the first round, and the main body of the manuscript now presents a coherent picture of convective aggregation within this idealized model. As such, I feel the manuscript is nearly ready for publication, and have only minor suggestions that I feel will bring further clarity to the presented results.

Specific Comments

Equation 11: The quantity A_+ is defined in line 267 as a fractional area, but then the LHS of this equations has dimensions $1/T$, and the RHS dimensions L^2/T . Presumably multiplication of the LHS by the square of the domain scale will rectify this inconsistency.

Figure 3: The information presented in panel (a) and panels (b)-(d) seems redundant. I find panel (a) to be a poor visualization and hard to glean information from, and so would recommend its removal. The caption also does not indicate the significance of the red curves in panel (a). The main points, i.e. the bimodal character of the moisture distribution and its achievement of a steady state, are adequately shown just with panels (b)-(d).

Figure 4: The fact that the aggregated regions in the nonlinear simulations organize along the directions of the underlying discretized grid seems like a numerical artifact (i.e. why wouldn't the aggregated line have some arbitrary orientation in the horizontal plane?). This should be briefly commented on in Section 3.2.

Line 425: With the chosen parameters $Q/H = 0.5$, $\mu_2/\mu_1 = 3$, the normalized gross moist stability (GMS) is $M = -0.5$. While I have no issue with the requirement of negative GMS, this gives quite a large magnitude relative to what is assumed in linear models for convectively-coupled waves. The authors have noted above that their model is insensitive to specific parameter choices, but I still feel this strongly negative value should be noted explicitly when introducing the GMS as an important parameter for the model.

Line 520: This wording in this sentence is poor, I would recommend re-wording to make it more clear.

Line 588: Should this read $\alpha = \lambda = f = 0$, rather than simply $\alpha = \lambda = f$?

Figure 10: The figure and corresponding caption are hard to follow. I would recommend colour-coding the lines or labelling them on the figure itself.

Line 899: Do Sugiyama's physically derived forms for F_q and F_h exhibit the kind of bistable behavior that is central to the results of this study? This seems like an important point in connecting the authors' ansatz to reality.