We thank the reviewers and co-editor for further careful reading of the paper and for their helpful comments. These comments have been considered carefully during the revision of the paper, and we will respond to each in turn below. The reviewers comments are shown in black and our responses in blue.

Response to Report #1

The long lead time (>100 days) needed for a large-scale convective system to grow out of aggregation cannot by any means be related to the MJO. The initiation timescale of the MJO is about 30 days. By that time, a large-scale circulation pattern would have already formed and interacted with convection. Any convective growth from then on cannot be explained by aggregation.

We accept that this comment is fair however we feel that it has been sufficiently covered in the current manuscript. In the previous revision of the paper we included additional notes at the end of the introduction (lines 107-112) that the model does not reach the stage of direct relevance to the MJO. We discussed this further in the conclusions (lines 957-975), emphasising the difference between our model and the MJO in both time-scale for the aggregation to form and in the spatial structure and propagation characteristics of the aggregated moist and dry regions on the beta plane. In particular, in the paragraph at lines 957-966, we specifically acknowledge that aggregation alone is unlikely to account for the observed behaviour of the MJO where a well-organised disturbance appears on a time scale of a few days at a spatial scale of 10^4 km.

Response to Report #2

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.

The text and one equation have been altered so that A_- and A_+ are now actual areas, not fractional areas.

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).

We feel that panel (a) does provide useful extra information, in particular on the evolution of the q_+ and q_- values with time and the agreement of the observed moisture distribution with the predicted values of q_+ and q_-, but accept that the caption did not make this clear. We have added detail to the caption to make it clearer what can be seen in panel (a).

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.

The referee raises an interesting point regarding the possible role of the numerical grid in determining the steady-state solutions shown in Figures 4(b) and 4(c). It is certainly the case the geometry of the domain, as distinct from the geometry of the numerical grid, is likely to be playing a very important role. We have changed the text to emphasise this point further. The role of the domain shape in nonlinear reaction-diffusion type problems has been established in the relevant literature through analytical work which does not depend on any particular grid. Therefore we think that it is more likely that it is the shape of the domain rather the shape of the numerical grid that is relevant here and we have not mentioned the latter in the revised text. (But we accept that there cannot be absolute certainty on this point.)

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.

We have now noted the value of M in the text immediately after the specification of parameter values in Section 3.1, line 362, so that readers are made aware of it.

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

We have re-ordered the sentence to make the meaning more clear.

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

This error has been corrected in the text.

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

The figure has been updated by colour coding the lines and giving their equations in the legend, and the caption has been simplified correspondingly.

Line 899: Do Sugiyama's physically derived forms for Fq and Fh 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. 1

Sugiyama (2009a) indeed notes bistable behaviour in a certain limit of the model considered and we have added a comment this effect. However we have also reminded the reader that the bistability is motivated more generally by other work cited earlier in the paper and have repeated those citations.