Review of Manuscript "A simple dynamical model linking radiative-convective instability, convective aggregation and large-scale dynamics" by Davison and Haynes

This is perhaps the most thorough study on convective aggregation and its potential effect on large-scale dynamics. It goes through meticulous discussions, both analytically and numerically, of the role of rotation (f and β- effects), nonlinear advection, thermal and dynamical damping and mean flow in convective aggregation and in the consequential large-scale zonal propagation. Its systematic treatment of convective aggregation in a variety of parameter space is a huge step forward from previous studies on this subject. This is a massive study with many mathematical details. It's impractical to discuss all these details in this review, although the devil is in them. Most of my comments are on the main objective of this study, which is to link convective aggregation to large-scale dynamics. My general sense is that the discussion on aggregation is fine, but its implication to the MJO is a stretch.

General comments:

1. There are so many simplifications and assumptions in the formulation of the theory that it is difficult for readers to see which ones are for mathematical convenience to get an analytical solution but bear minimal physical consequences, and which ones are key to the conclusion. It might be helpful to have a table that list all simplifications and assumptions and marks those that is at the center of this theory (e.g., must have to make the conclusion valid).
2. Sections 3 and 4 are unnecessarily long. The failure of aggregation on an f-plane has been documented before (e.g., Carstens and Wing 2022). Even though more detailed discussions on this subject add intellectual values and it might be nice to compare results on a β-plane to those on an f-plane, Sections 3 and 4 do not directly contribute to the main conclusion of this study, which can be made solely based on the β-plane discussions. Suggest substantially shorten this part and retain only the materials that are directly relevant to compare with those on a β-plane. This would make the manuscript more focused on tropical dynamics and easier to follow. The removed materials on an f-plan can be published elsewhere.
3. I found the interpretation of large-scale zonal propagation in terms of its dependence on latitudes very interesting. Based on the authors' interpretation, when Rossby (Kelvin) wave response is stronger, the propagation tends to be eastward (westward). This is counter-intuitive but not impossible. There are several issues that the authors may want to consider:

a) The Rossby wave may lead to eastward propagation without any convective aggregation (Hayashi and Itoh 2017; Rostami and Zeitlin 2019; Yano and Tribbia 2017).
b) The westward propagation due to stronger Kelvin wave responses in the context of convective aggregation should be discuss in the context of its natural (dynamical) eastward propagation. In other words, what is the net balance between the dynamical eastward propagation of the Kevin wave and its westward propagation due to convective aggregation?
c) The "<" shape structure is in the opposite direction of the "swallowtail" pattern observed for the MJO (Zhang and Ling 2012). How do the two reconcile with each other if the authors think their results are relevant to the MJO?
d) With a strength of convective heating associated with moisture anomalies, its Rossby and Kelvin wave responses are pretty much fixed based on the Gill model. Under what

circumstances the relative strengths of Rossby and Kelvin wave responses would vary? By the latitude location of convective heating?

4. The time it takes for the aggregation to reach a large-scale state (400 days) is much longer than the initiation of the MJO or any tropical large-scale disturbances. How should this inconsistency be reconciled?
5. In the conclusion section (6), the authors compared their results with those from previous studies of the MJO, all of them relying on the moisture-mode doctrine. It might be further enlightening if the authors walk out of the moisture-mode and aggregation camps and discuss what their results add to other simpler theories. For example, as mentioned above, several studies (Hayashi and Itoh 2017; Rostami and Zeitlin 2019; Yano and Tribbia 2017) proposed that the dry Rossby wave alone may propagate eastward to provide a propeller effect on the MJO. Kim and Zhang (2021) suggested that dry Kelvin waves alone can propagate eastward slowly at the MJO speed in the presence of dynamical damping. They reproduced the observed "swallowtail" pattern without Rossby waves. As the authors know very well, Adames and Kim (2016) provided a perhaps the most comprehensive MJO theory based on the moisture-mode thinking. All these studies reproduced fundamental MJO properties to various degrees without convective aggregation. The big question for this study under review is: What additional physical insights to MJO dynamics can be added to the previous studies or what fundamental MJO properties missing from the previous studies can be produced by including convective aggregation? Especially, the authors mentioned that more physics are needed to make the results more realistic. Is that so? Majda and Stechmann (2009) and Kim and Zhang (2021) included far less physics (assumptions) but produced no less fundamental properties of the MJO than other MJO theories. Sometimes less is better. I challenge the authors of this study to pursue the beauty of simplicity. If convective aggregation plays an important role in the MJO, there must be a way to demonstrate this in a simple, straightforward way without invoking numerous assumptions and massive mathematical procedures.

Detailed comments:

1. Near line 20: "The key overall properties of the propagating disturbances, the spatial scale and the phase speed, depend on nonlinearity in the coupling between moisture and dynamics and any linear theory for such disturbances therefore has limited usefulness." I beg to differ. See major comment 4 regarding simple linear theories.
2. Line 28: "Much theoretical and modelling work over the past few decades has focused on the coupling between dynamics and moisture in the tropical atmosphere, which it is clear must be taken into account at leading-order to explain many tropical phenomena." Ditto. Look around outside the moisture-mode camp and you would find theories with equal or greater success in comparison to the moisture-mode ones. This would help to put this theory in a broader context.
3. The origin of the moisture equation (3) needs to be scrutinized. In the text, it is "assumed", which is not good enough. In most of the moisture-mode studies, such a moisture equation is thrown to the well-established shallow-water equation as a second thought. This has been one of the most severe inconsistencies in the moisture-mode theories that very few have realized. Neelin and Zeng (2000) derived a set of horizontal structure equations for wind, temperature

and humidity (their Eqs 5.1 – 5.4) after applied variable separation and vertical base functions. Can the moisture equation (3) in this study under review be vigorously derived from Neelin and Zeng's humidity equation (5.4)? If not, we have a problem.

4. What is the physical reality of $\frac{d\bar{h}}{dt}$ (Eq 5)? Global mean temperature fluctuation? Is there an example for this? Please forgive my ignorance if this is a common knowledge.
5. Please provide justifications for the values of α, λ, κ chosen in Table 1 or point out where in the text such justifications are given if I missed them.
6. The domain integrations (Eqs 42 – 43) need explanations. If it covers the entire longitudinal circle, it mixes the convective part (Indian Ocean and Western Pacific) where zonal speeds tend to be lower than the dry part of the tropics. Otherwise, do they distinguish the faster and slow speeds when the domain covers the moist and dry zones separately?
7. The linear friction coefficient α plays a central role on a β-plan but not on an f-plan. The parameter $c\beta/\alpha^{3/2}\lambda^{1/2}$ suggests that α must not be zero on a β-plan. All $\alpha = 0$ cases are on an f-plan. Linear friction plays critical role in some theories (e.g., Adames and Kim 2016; Wang et al. 2016; Kim and Zhang 2021), but not in others (e.g., Majda and Stechmann 2009; Hayashi and Itoh 2017; Yano and Tribbia 2017; Rostami and Zeitlin 2019; Emanuel 2019). Can the author explain why linear friction must exist in this theory and what exactly it does to make the theory work on a β-plan? Eqs 36 and 37 clearly demonstrate that α and f (or βy) play equivalent roles and they cannot be zero at the same time. This deserves a physical explanation: Why must friction exist at the equator but not away from the equator? Go back to the starting point (Eqs 14 – 17), there must be a solution with $\alpha = 0$. What is that solution on a β-plan?
8. Figs. 14, 15, 16, and 19: Does it take the equal length of integration to reach the states shown in the panels? What is the integration length? 400 days as in Fig. 18? It should be mentioned in the captions.
9. In all the parameters used, which one control the speed at which aggregated perturbations grow? Fig. 9 suggests it's κ when $\alpha = \lambda = f = 0$. How about on a β-plan when α and λ are not zero?

References

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