Earth System Dynamics Discussions

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Comment on esd-2022-30

Jacob Schewe (Referee)

Referee comment on "Origins and suppression of bifurcation phenomena in lower-order monsoon models" by S. Krishna Kumar and Ashwin K. Seshadri, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2022-30-RC1, 2022

General comments:

This paper is motivated by previous studies of simple monsoon models, which either found bifurcation behavior or a quasi-linear behavior of monsoon strength in response to external forcing. The paper starts with a more complex and comprehensive model of the tropical circulation, the QTCM, and reduces it to obtain a low-order monsoon model comparable to those used in those previous studies. Importantly, the authors derive two separate sets of equations, one each for non-zero precipitation and zero precipitation. They then demonstrate that each of these sets exhibits bifurcation behavior. The physically relevant solution to the full model is comprised of the relevant portions of each of the two separate sets. The authors show that, using a standard set of parameter values, this full solution does not exhibit a bifurcation point in the physically relevant regime, explaining previous results that showed near-linear behavior of the full low-order model. On the other hand, the underlying bifurcations are still present, and perturbations in terms of parameter values can move bifurcation points into the physically relevant regime; explaining other previous results that showed bifurcation behavior in a similar low-order model.

The paper is well written and highly useful as it reconciles opposing findings of previous studies. It also reveals the underlying dynamical structure of the model across a broader range of parameters, showing how a pitchfork bifurcation emerges when the effects of gross moist stratification and moisture advection on horizontal velocity are no longer assumed to cancel each other. The authors also illustrate, by gradually reducing the dry thermal stratification parameter, the transition from the case with near-linear physical solution to a case with a physically relevant bifurcation point.

I recommend publication of the paper subject to some technical corrections and consideration of a few comments and suggestions, as listed below.

Specific comments:

- Some more discussion is needed of the change in sign of a_T. What does it mean, why is it necessary, and how is the point at which the sign should change determined? There is some discussion of this in the SI, but it is not clear how arbitrary this choice is and how a deliberate change in sign of one quantity affects the self-consistency of the overall model.
- Regarding the model equations: The step-by-step derivation in the SI is useful, as is Table 1. I think it could be even more helpful if the authors could provide a bit more interpretation of the different terms in equations 2 and 3 (e.g. horizontal temperature gradient, heat advection, moisture advection etc.). And potentially provide some introduction/references for the concepts of static energy and static stability, which are of central importance.
- An optional, but very interesting addition to the paper would be some discussion of the plausible ranges of the relevant parameters. For instance, the authors show that a physical bifurcation point emerges already well before the dry thermal stratification parameter reaches zero. Can the transition point be pinned down, and how far away is it from realistic values of the parameter for either modern or paleo climates (where those are known)? Similarly, given that the solution structure changes strongly for small deviations from the balance between gross moist stratification and moisture advection, how prevalent or relevant are such deviations for the real large-scale circulation? Finally, indicating typical or expected values of the radiation parameter R might help readers interpret the figures with respect to plausible regimes.
- Related to this, and also optional, would be some discussion of what the solutions would look like in terms of a different parameter than the insolation R, such as the moisture at the sea boundary q_s.

Technical corrections:

Main paper:

- Equations 4 and 5: Indices c and q are not explained – do they stand for cubic and quadratic, respectively? Important to clarify, since q could also correspond to moisture.
- line 205: “comparing Fig. 2” – compare to what? Do you mean comparing the two

curves in Fig. 2?

- line 277: a_q is not explained, I think it is the notation used by Boos & Storelvmo – please clarify. Perhaps alternative notations (also a_T for the temperature advection coefficient) could be indicated directly in Table 1.
- line 303: “partially nullify” – should this read “fully nullify”? I understand the two terms need to cancel each other exactly in order to remove the cubic term.
- line 357: delete “they occur in”
- Figure 6: Please indicate the (relative) values of M_s chosen for each panel, e.g. in the caption or in a legend.
- Figure 7, caption: “M_qp is reduced from its standard value.” – this sentence seems superfluous given the following sentence. Delete?

SI:

- Figure S1: the grey bar connecting “Galerkin expansion” and “Tailored basis functions” is not explained. It is also unclear which of the blue boxes the expressions “Convective” and “Non-convective regions” refer to.
- Equations (1): Some symbols are not explained, e.g. f (Coriolis frequency?), or the indices of epsilon (01, 10 etc.). Please explain all symbols used.