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Comment on esd-2022-30

Anonymous Referee #2

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-RC2, 2022

This manuscript essentially details the study of a very simple model of monsoons that is the basis of one section of Boos and Storelvmo (2016)'s rebuke of Leverman et al (2009)'s model which produces abrupt transitions between regimes with and without monsoon. The manuscript provides more information on the structure of the dynamical system than what Boos and Storelvmo (2016) provided, and this might be worth publication if developed further. Section 3.3 on the pitchfork bifurcation is the most novel part of the manuscript; its interest lies in relation to Leverman et al (2009)'s study. It is otherwise an essentially mathematical exercise since it relies on breaking the physical consistency of the model. There are also flaws in the original model that should be addressed, a section (3.2) should be shortened and a lot of technical details should be improved before it can be published.

Main comments:

1. This simple model presents monsoons as a large-scale sea breezes. It neglects rotation and does not simulate the reversed trade winds. But, from the early definition of monsoons (Ramage 1971) to recent work on global monsoon (e. g., Gadgil 2018, Geen et al. 2020), the reversal of the winds is an inherent part of the monsoon circulations that distinguishes them from breeze circulations. As a result, the amplitude of the meridional wind is about one order of magnitude larger than the observed wind (well, if v refers to the low-level wind and not to (minus) v_1 form the QTCM). I think it would be an improvement on Boos and Storelvmo (2016)'s model to include the effect of rotation by considering an f-plane. This would actually not change the number of equilibria, ε_1 would just have to be substituted by $\varepsilon_1 + f^2 / \varepsilon_1$, but it would change the amplitude of the meridional wind and precipitation response and maybe modify the stability of the equilibria.

2. In the QTCM, by construction, $M_{qp} = - \langle b_1 V_1 \rangle$, which is imposed by the conservation of

water- vapor mass by transport (in the absence of phase change): the integral of the terms of transport overt the whole horizontal domain has to be zero. Mathematically, it makes Equation 4 quadratic. There is no pitchfork bifurcation if the physical basis of the model is respected. This should be stated clearly even before the first results. At first, I was wondering why there was no third solution shown for P>0 in figure 2, but that's because of the equality above, which is not highlighted until Section 3.3.

In Section 3.3, it would be worth providing clarification that making M_{qp} different from -$\langle b_1 V_1 \rangle$ amounts to disregarding mass conservation of moisture. In the current version of the manuscript, lines 276-282 do not make clear that this equality is a result of a fundamental law of physics. Lines 298-300 refer to a physical "interpretation" of the equality above. It is more than a physical interpretation, it is the expression of water-vapor mass conservation by transport. The interest of this section is to clearly show the unphysical assumption in Leverman et al (2009)'s model, this should be investigated further and the most interesting oints in the further analysis mentioned on lines 342-343 should be included in this section to enhance its content.

3. The equations could be simplified, clarified, and made easier to interpret physically.

In the supplementary material, the derivation of the reduced model from the QTCM equations is too long and a little confusing. First, the terms of meridional advection of zonal wind and zonal advection of meridional wind are wrong. Second, the symbol v_1 is used for both the QTCM variable of first-baroclinic meridional wind and the fixed vertical profile of wind associated to this mode; in the reference articles on QTCM, the latter is noted V_1. In the LSSS geometry, the imposed boundary conditions are incorrect: on boundaries B and D, the presence of zonal gradients of temperature (hence, of geopotential) precludes assuming that the zonal winds u_B and u_D are equal, and same for v_B and v_D in the presence of rotation. Actually, I think the LSSS geometry and the whole derivation are not necessary. By assuming zonal symmetry and a flat, constant-pressure surface, continuity imposes the barotropic meridional velocity to be constant and therefore zero. Neglecting the non-linear momentum transport essentially sets the barotropic zonal velocity to zero as well and reduces the equations of baroclinic wind to the Matsuno-Gill system (Matsuno 1966, Gill 1980). This system is sufficient to simulate the main features of monsoon circulations (Gill 1980, Bellon and Reboredo 2022).

Also, in the main text, the coefficients $\langle a_1 \rangle$ and $\langle b_1 \rangle$ resulting from vertical averaging should appear, respectively, in front of the time derivatives of T_{1L} and q_{1L} in Equations 2 and 3. The authors should check that they are taken into account in the computations of the stability of the equilibria. Equations 2 and 3 need to be clarified, for ease of undertanding: terms corresponding to the same physical contribution (horizontal transport, vertical transport, diabatic sources) should be factorized as much as possible and regrouped; most parentheses are currently not necessary (and one is not opened in 3b), the diabatic terms Hg/p_t , Rg/p_t , Eg/p_t could be written $\langle H \rangle$, $\langle R \rangle$, $\langle E \rangle$ for simplicity, and a notation for $R+H$ (the source of dry static energy) would also simplify the equations.

Finally, it seems to me that the expression of B_c after Equation 4 is missing a factor τ_c in its second term on the right hand side, and I have doubts about the sign in front of $<a_1V_1>$ in the first term on the right-hand sign.

4. As I understand it, R is imposed and varied systematically, but there is no mention of how H and E are set for the solutions presented in Section 3. And if all the QTCM parameters can be found in the reference article, it would be worth giving the values of these parameters. Also, T_{1s} and q_{1s} should be specified and, if they are set to zero (i.e., the oceanic surface is at the reference state of the QTCM), it could be specified early in the manuscript so as to simplify the equations.

5. If Boos and Storelvmo (2016) listed the physical misconceptions in Leverman et al. (2009), it would be worth mentioning that the model with no stratification does not simulate a non-precipitating equilibrium for $R< 0$, which can be considered as winter conditions. Indeed, without adiabatic warming due to subsidence, there is no term that can compensate diabatic cooling. Leverman et al. (2009) considered only horizontal advection in the lower troposphere, which can be a cooling term over the continent for onshore low-level flow but can hardy be a warming term (except if the advection by the returning upper-tropospheric flow is included). Arbitrarily changing the sign of $<a_1V_1>$ is really not physically relevant and does not really show any particularly interesting behavior of the system. I think Section 3.2 should investigate only the sensitivity to M_s, which can be considered to depend on the reference stratification of temperature T_r and therefore changed to some extent. To better document the sensitivity of the system, the authors could investigate the sensitivity to the profile of temperature perturbation a_1(p) (more or less similar to a perturbation of the moist adiabat, similarly to Section 3.c of Bellon and Sobel 2010), which would modify V_1(p) and multiple other parameters ($<a_1>$, $<a_1V_1>$, M_{sp}, $<b_1V_1>$, M_{qp}) in a pÄ¥ysically consistent framework.

Minor edits:

a. Some references are supposed to be in line in the text but appear in parentheses.

b. In many instances, the text and captions refer to thick and thin solid lines in the figures. The figures have obviously been changed since these descriptions have been written.

c. Readers should not have to read Boos and Storelvmo (2016)’s article to know what α_T and α_q mean.

d. On line 58, moist static energy (MSE) does not increase with altitude. It has a minimum in the middle troposphere. Overall, the gross moist stability is positive because in average the upper-tropospheric MSE (above the minimum) is larger than the lower tropospheric MSE.

Additional references:

Bellon, G., & Reboredo, B. (2022). Scale sensitivity of the Gill circulation. Part II: Off-equatorial case. *Journal of the Atmospheric Sciences*, *79*(1), 19-30.

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Gill, A. E. (1980). Some simple solutions for heatâ□□induced tropical circulation. *Quarterly Journal of the Royal Meteorological Society*, *106*(449), 447-462.

Matsuno, T. (1966). Quasi-geostrophic motions in the equatorial area. *Journal of the Meteorological Society of Japan. Ser. II*, *44*(1), 25-43.

Ramage, C. S. (1971). Monsoon meteorology, Academic Press.