Review response for "Origins and suppression of bifurcation phenomena in lower-order monsoon models"

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Reviewer 1

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:

1. **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.**

   Response 1: We thank the reviewer for the question. The thermodynamic equation relates the thermal advection and thermal stratification to the diabatic energy input into the column, *i.e.*, precipitation and vertically integrated radiative fluxes. The steady state thermodynamic equation for the case of precipitation $P > 0$ is,

$$\langle a_1 V_1 \rangle v \frac{\partial T}{\partial y} + M_s \frac{\partial v}{\partial y} = P + \frac{Rg}{p_T} \quad (1)$$

   Typically, the thermal stratification term- also representing the effect of adiabatic cooling- is the leading order term in the equation[Boos and Storelvmo, 2016]. The stratification coefficient $M_s = M_{sr} + M_{sp}T_{1L}$ is designed to be positive [Neelin and Zeng, 2000], leaving the sign of the stratification term to depend only on the sign of the meridional velocity gradient. With the assumption that meridional velocity vanishes on land boundaries, $\partial v/\partial y = (v_{1L} - v_{1s})/L$ means that the sign of the meridional velocity gradient is opposite to that of the meridional velocity value at the sea boundary, v_{1s}. For a landward monsoonal wind flow, as expected when the column energy input $R > 0$, $v = v_{1s}$ is negative (upper tropospheric flow as in [Neelin and Zeng, 2000], approximation $v = v_{1s}$ as in Boos and Storelvmo [2016]). As a consequence, the adiabatic cooling term is positive for non-zero precipitation. Using the momentum equation to replace the temperature gradient in the advection term, the product of v_{1s} and the temperature gradient turns out to be a quadratic term in v_{1s} with a negative sign. This means that the sign of the advection term $\langle a_1 V_1 \rangle v_{1s} \frac{\partial T}{\partial y}$ is positive if $\langle a_1 v_1 \rangle$ or a_T is negative and together with the stratification term, it can balance the precipitative and radiative forcings. If the stratification term were too large compared to the advection term, it might also be the case that positive a_T, and consequently negative advection term could still be mathematically permissible so that the net effect of the two terms still balances the forcings. However, if $M_s = 0$, then evidently a_T is bound to be negative for the balance to be intact. Thus, a positive a_T could work well for the case with dominant stratification but not so for the no-stratification case. A negative value of a_T should work well for both the cases, on the other hand. As seen in the analysis ahead, a similar scenario appears to be the case with the model by Boos and Storelvmo [2016] with $a_T > 0$ guaranteeing a positive advection term.

Let V_s be the lower tropospheric meridional velocity used in the model by Boos and Storelvmo [2016]. Effectively, $V_s = -v_{1s}$, for the same parameters and assumptions. The thermodynamic equation for the corresponding model, after replacing the derivatives with finite difference approximations is

$$\frac{a_T \epsilon_1 + M_{sp}\epsilon_1}{\kappa} V_s^2 + (M_{sr} + M_{sp} T_s)\frac{V_s}{L} = P + R. \tag{2}$$

The corresponding equation for the lower order model from QTCM, formulated in this study is,

$$\frac{-a_T \epsilon_1 + M_{sp}\epsilon_1}{\kappa} v_{1s}^2 - (M_{sr} + M_{sp} T_s)\frac{v_{1s}}{L} = P + \frac{Rg}{p_T}. \tag{3}$$

For Eq.3, consider the case of $P = 0$. The resulting quadratic equation in v_{1s} has roots which are real if,

$$\frac{(M_{sr} + M_{sp} T_s)^2}{L^2} - 4\frac{(-a_T + M_{sp})\epsilon_1}{\kappa}(-Rg/p_T) > 0. \tag{4}$$

Considering that we seek real roots of v_{1s} for all $R > 0$, the second term in the discriminant is always positive if $a_T < M_{sp}$, and this guarantees that real roots occur for all $R > 0$. However, depending on the magnitude of M_{sr} and M_{sp}, $a_T > M_{sp}$ also can yield real roots, affirming the discussion above. Yet, in that case, there will always be a higher value of R for which the second term in the discriminant has an absolute value higher than $(M_{sr} + M_{sp})^2/L^2$, and the discriminant would be negative. Thus, real roots for all $R > 0$ occur only if $M_{sp} - a_T > 0$, or in other words, the quadratic coefficient in Eq.3 is positive. As M_{sp}, along with M_{sr}, is reduced from their standard value to 0, the need for change in sign of a_T arises when the condition $M_{sp} - a_T = 0$ is satisfied. For the present study, this critical condition is met when M_{sp} is reduced to 73% of its standard value.

Note that if value of M_{sp} is markedly smaller (say, by an order) than a_T, than the sign of a_T decides the existence or otherwise of real roots for $R > 0$. For the QTCM based low order model studied in this paper, this means that negative a_T almost always yield real roots for all $R > 0$. Thus, a change in sign when M_s is reduced is not required if $a_T >> M_{sp}$. When a_T and M_{sp} are comparable, as in the values suggested in Neelin and Zeng [2000] (where $a_T > 0$, $a_T < M_{sp}$), the existence of real roots can change with slight changes in value of a_T. Boos and Storelvmo [2016] remark that the changing of sign of a_T with reduction of M_{sp} is only optional and serves to relate their results with those of Levermann et al. [2009]. This possibly follows the fact that in their study, $|a_T| >> M_{sp}$ (supplementary to [Boos and Storelvmo, 2016]).

Notably, Neelin and Zeng [2000] use an $a_T > 0$, and the same is used in the present study for conformity with Neelin and Zeng [2000] and Boos and Storelvmo [2016]. The preceding discussion indicates that a different choice ($a_T < 0$) should avoid the requirement of change in sign of a_T as stratification is suppressed. However, note that the temperature basis vector is positive across the pressure variable (in QTCM) and the baroclinic velocity basis is positive with a large value at the top. The basis functions as used in Zeng et al. [2000] indicate that $a_T = \langle a_1 V_1 \rangle$ would be positive. Thus, changing the sign of this coefficient would require modifying the basis functions themselves. This is beyond the scope of the current study and the present study follows the a_T sign change practice as followed in Boos and Storelvmo [2016], in accordance to Neelin and Zeng [2000].

2. **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.**

   Response 2: We thank the reviewer for the suggestion. A brief remark on the interpretation of the terms in the non-steady equations for the non-zero precipitation and precipitation cases is now included in the manuscript. Also, relevant literature for dry and moist static energy as well as static stability have been included in Section 2 of the manuscript.

3. **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.**

   Resonse 3: We thank the reviewer for the suggestion. A discussion of the range of parameters appearing in the equations would certainly be useful to the reader in interpreting the feasibility of these mechanisms. The parameters used are appropriate to the tropics and are the same as those used in Neelin and Zeng [2000] and Boos and Storelvmo [2016]. While a detailed discussion on the ranges of parameters would certainly add value to the discussion, especially in the light of global climate data including paleoclimatic observations, such an inquiry is expected to be a sizeable discussion on its own. Given the importance of the conclusions of such a study to paleoclimatic monsoon dynamics and rapid changes observed therein, we intend to follow this study up with a manuscript on its relevance to global contemporary climate data and paleoclimatic observations. Hence, we would prefer to relegate a detailed and systematic discussion of these aspects to a future study. The critical point at which physical bifurcation starts to appear as stratification is reduced can be estimated simply by comparing Eqs. 4 and 5 of the manuscript. The equilibrium value at the bifurcation point of a saddle node curve for a quadratic equation $Ax^2 + Bx + C = 0$ is $-B/2A$, and the corresponding critical value of bifurcation parameter can be obtained using the fact that the discriminant $B^2 - 4AC$ vanishes at this point. In our case, the critical R value, corresponding to the bifurcation point, can be obtained from Eq. 4 and Eq. 5 of the manuscript for the $P > 0$ and $P < 0$ cases respectively, in this fashion. The critical R value (R_c) from Eq. 4 (corresponding to non-zero precipitation curve in Figure 2 of manuscript) has to be smaller than the critical value R_q from Eq. 5 (corresponding to zero precipitation curve). Note that R_c results from $C_c^2 - 4B_cD_c = 0$, considering that $A_c = 0$ for the case considered in this study as well as Boos and Storelvmo [2016] and Neelin and Zeng [2000] (following from the equality between $a_q = \langle b_1V_1 \rangle = -M_{qp}$ values). R_q results from $B_q^2 - 4A_qC_q = 0$. Equating R_c and R_q, yield the critical values of the parameters at which bifurcation happens. A remark on this aspect is now included in the Supplementary Information in Section 2.

4. **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.**

   Response 4: The system, as the reviewer points out, has a number of parameters which affect the bifurcation scenario. On a multi-parameter space, the potential bifurcation scenarios are certainly more complicated than the saddle-node or pitchfork scenario presented in this study. The choice of R as the bifurcation parameter is the first step in the analysis and follows the fact the it represents the dominant agent for possible abrupt changes over various timescales. While q_s didn't seem to cause a major qualitative change in the bifurcation scenario, other parameters might certainly do so. This study, to an extent, also discusses the effect of parameters a_T and M_{sp}. A rigorous discussion of effect of all the major parameters, in additions to modifications in the assumptions inherent in the parameterisations (e.g., rainfall), is expected to be a part of a ensuing paper.

***Technical corrections*: Main paper:**

1. **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.**

   Response: The indices indeed correspond to 'cubic' and 'quadratic' nature of the equations. This is now clarified besides the equations themselves.

2. **line 205: "comparing Fig. 2" – compare to what? Do you mean comparing the two curves in Fig. 2?**

   Response: We thank the reviewer for pointing out the mistake. The typo has now been corrected as 'illustrated in Fig.2' instead of 'illustrated by comparing Fig.2'.

3. **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.**

   Response: Table 1 in the manuscript is now edited to include that $a_q = \langle b_1V_1 \rangle$, and $a_T = \langle a_1V_1 \rangle$, as suggested.

4. **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.**

   Response: The moisture stratification term has a component involving the reference value M_{sr} and one involving moisture sensitivity M_{sp}. The former isn't nullified in the process in which cubic term vanishes. In fact, a term $M_{sr} + M_{sp}T_{1s}$ remains after the cubic term vanishes for $a_q = M_{qp}$ (please refer to Eq. 4 in the manuscript). Hence, we remark that the moisture advection and stratification terms partially nullify each other.

5. **line 357: delete "they occur in"**

   Response: We thank the reviewer for pointing out the typographical error. It is now corrected in the manuscript.

6. **Figure 6: Please indicate the (relative) values of M_s chosen for each panel, e.g. in the caption or in a legend.**

   Response: We thank the reviewer for pointing out the mistake. It is now corrected in the manuscript.

7. **Figure 7, caption: "M_{qp} is reduced from its standard value." – this sentence seems superfluous given the following sentence. Delete?**

   Response: The phrase is indeed redundant has been removed from the manuscript.

SI:

1. **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.**

   Response: The grey bar refers to the fact that the Galerkin expansion is in terms of the basis functions, which are tailored to specific approximation in the QTCM, *i.e.* the solution for the convective regions. This is now clarified in the discussion on the flowchart in Figure S1.

2. **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.**

   Response: We thank the reviewer for pointing this out. These are now explained in the Supplementary Information in a table.

Reviewer 2

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 from 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 $\epsilon_1 + f^2/\epsilon_1$, but it would change the amplitude of the meridional wind and precipitation response and maybe modify the stability of the equilibria.**

   Response 1: We thank the reviewer for the suggesting improvements to the model. The presented model indeed has restrictions and accounts for only the nonrotating baroclinic mode of variation in the system variables, and it would serve well if it could capture as many aspects of monsoon circulations as possible. The present study is a part of a broader attempt by our group to form a hierarchy of simple monsoon models in the QTCM framework and the present model is only a first step in it. The broader objective is to understand the dynamics, in light of bifurcations as well as otherwise, of different models that can be obtained by relaxing the assumptions inherent in the present study, which follows the assumptions made in Boos and Storelvmo [2016]. Permitting rotation, and as a result a non-zero zonal baroclinic velocity, is one among the relaxations possible in this regard. A thorough analysis into the effects of different assumptions (including rotation) on the bifurcation scenario, as well as variations within the permissible range of the parameters, is currently underway as the next phase of the work. The inclusion of rotation is indeed expected to reduce the intensity of circulation as opposed to the non-rotating case [Reboredo and Bellon, 2022]. However, we expect the effect to be quantitative with respect to the equilibria and their stability, and hence leave a study of effect of rotation for the subsequent paper.

   The present study essentially attempts to present a unified explanation for both appearance or otherwise of physical bifurcations in simple monsoon models. To this extent, keeping the model similar to the ones in literature, *i.e.* Boos and Storelvmo [2016], helped focus on the question of appearance of bifurcations from a dynamical systems perspective.

   The inclusion of rotation also means the inlcusion of zonal baroclinic velocity and zonal asymmetry (*i.e.*, non zero zonal temperature gradient). For the single column framework used in this study to obtain reduced model from QTCM, this may not result in change in number of equilibria or their stability, but would still increase the number of prognostic variables (Temperature, velocity variable at each of the four boundaries). While this would be certainly interesting to undertake, we feel it would be beyond the scope of present manuscript. Please note that the four-boundary framework has been assumed initially before reducing it to a simpler form mimicking the model in Boos and Storelvmo [2016], precisely to pave way for further models with relaxed assumptions.

   A remark on this aspect, along with references to the role of rotation on monsoonal circulations is now included in the manuscript in Section 2, for the sake of clarity.

2. **In the QTCM, by construction, $Mqp = -\langle b1V1\rangle$, 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 - $\langle b_1V_1\rangle$ 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 points in the further analysis mentioned on lines 342 – 343 should be included in this section to enhance its content.

   Response 2: We thank the reviewer for pointing out this fundamental aspect. Consider the steady state moisture equation for the simple model in QTCM framework used in this study-

$$\langle b_1V_1\rangle v\frac{\partial q}{\partial y} + M_q\frac{\partial v}{\partial y} = -P \tag{5}$$

   Expanding this equation using $M_q = M_{qr} + M_{qp}q$, before substituting the finite difference approximations, we get,

$$\left(\langle b_1V_1\rangle v\frac{\partial q}{\partial y} + M_{qp}q\frac{\partial v}{\partial y}\right) + M_{qr}\frac{\partial v}{\partial y} = -P. \tag{6}$$

   Here E is taken to be zero, as in the manuscript. When the moisture advection coefficient $\langle b_1V_1\rangle$ (or a_q) is equal to $-M_{qp}$, this equation simplifies to

$$\left(\langle b_1V_1\rangle v^2\frac{\partial(q/v)}{\partial y}\right) + M_{qr}\frac{\partial v}{\partial y} = -P, \tag{7}$$

   which indicates that there may not be any cancellation of terms due to the equality $a_q = -M_{qp}$, though the equation may be considered to have been simplified. Following the finite difference approximations used in the study- $v = v_{1s}$, $q = q_{1s}$, $\partial v/\partial y = (v_{1L} - v_{1s})/L$, $\partial q/\partial y = (q_{1L} - q_{1s})/L$ and $v_{1L} = 0$,

$$\cancelto{0}{(\langle b_1V_1\rangle - M_{qp})}\ \frac{q_{1L}v_{1s}}{L} - (M_{qr} + M_{qp}q_{1s})\frac{v_{1s}}{L} = -P. \tag{8}$$

   Note that all the finite difference approximations are required for the above simplification to take place, and such a cancellation of terms doesn't appear to be intrinsic to the conservation equation (Eq.5) itself.

   Also, Eq.8 indicates that $\langle b_1V_1\rangle = M_{qp}$ renders the precipitation P to be a linear function of v_{1s}. It can also be shown that, similar to P, the moisture variable q_{1L} also varies linearly with variations in v_{1s}. Consequentially, substituting these linear relationships of P and v_{1s} in the simplified temperature steady state equation,

$$\frac{-a_T\epsilon_1 + M_{sp}\epsilon_1}{\kappa} v_{1s}^2 - (M_{sr} + M_{sp}T_s)\frac{v_{1s}}{L} = P + \frac{Rg}{p_T}, \tag{9}$$

we get,

$$\frac{-a_T\epsilon_1 + M_{sp}\epsilon_1}{\kappa} v_{1s}^2 - (M_{sr} + M_{sp}T_s)\frac{v_{1s}}{L} = (M_{qr} + M_{qp}q_{1s})\frac{v_{1s}}{L} + \frac{Rg}{p_T}. \tag{10}$$

This further reduces to

$$\frac{-a_T\epsilon_1 + M_{sp}\epsilon_1}{\kappa} v_{1s}^2 - ((M_{sr} + M_{sp}T_s) - (M_{qr} + M_{qp}q_{1s}))\frac{v_{1s}}{L} = \frac{Rg}{p_T}, \tag{11}$$

which differs from Eq.9 with $P = 0$ only in the linear term's coefficient. In fact, it can be shown that taking $P = 0$, even without taking $\langle b_1 V_1 \rangle = M_{qp}$, leads to the moisture value being just a diagnostic term independent of time too. Taking $\langle b_1 V_1 \rangle = -M_{qp}$ with $P > 0$, in turn, leads to a diagnostic moisture variable with q_{1L} varying linearly with v_{1s}. Thus, $\langle b_1 V_1 \rangle = -M_{qp}$ reduces the effect of moisture balance and non-zero precipitation on the dynamics to merely a change in the location and stability of the equilibria of the zero-precipitation case. When this condition is relaxed, it permits a higher order interaction between the moisture field and velocity, temperature fields. Both in the steady state form (Eq. 7) as well as in the finite difference simplified form as in Eq. 8, $\langle b_1 V_1 \rangle$ and M_{qp} are the coefficients of the only nonlinear terms in the moisture balance. These terms cancelling out effectively neglects the nonlinear contributions to moisture dynamics. This leaves the thermal balance to contain the only nonlinear terms in the three equation system. For this reason too, it appears to be useful to relax $a_q = -M_{qp}$ condition and investigate the dynamics.

Consider the expressions for $a_q = \langle b_1 V_1 \rangle$ as in Neelin and Zeng [2000],

$$a_q = p_T^{-1} \int_{p_{rt}}^{p_{rs}} b_1(p) V_1(p) \mathrm{d}p. \tag{12}$$

Here p_{rs} and p_{rt} are the reference pressure values at the surface and top of the column. Integrating this expression by parts,

$$a_q = p_T^{-1} \left(b_1 \int V_1 \mathrm{d}p \right)_{p_{rt}}^{rs} - p_T^{-1} \int_{p_{rt}}^{p_{rs}} \left(\int V_1 \mathrm{d}p \right) \frac{\partial b_1}{\partial p} \mathrm{d}p \tag{13}$$

Since,

$$M_{qp} = p_T^{-1} \int_{p_{rt}}^{p_{rs}} \left(- \int_{p}^{p_{rs}} V_1(p') \mathrm{d}p' \right) \frac{\partial b_1}{\partial p} \mathrm{d}p, \tag{14}$$

we get,

$$a_q = p_T^{-1} \left(b_1 \int V_1 \mathrm{d}p \right)_{p_{rt}}^{rs} - M_{qp}. \tag{15}$$

Thus,

$$a_q = \langle b_1 V_1 \rangle = \langle V_1 \rangle (b)_{p_{rt}}^{p_{rs}} - M_{qp}, \tag{16}$$

which means that $a_q = -M_{qp}$ if $\langle V_1 \rangle$ vanishes. In other words, $a_q = -M_{qp}$ if the vertical integral of the baroclinic velocity is zero. In QTCM [Neelin and Zeng, 2000], for a temperature basis function $a_1(p)$, the baroclinic velocity basis function is taken to be

$V_1(p) = a_1^+ - \langle a_1^+ \rangle$, where $a_1^+ = \int_p^{p_{rs}} a_1(p^{'})\mathrm{d}\ \ln(p^{'})$. Hence, the vertical integral of the basis function,

$$\langle V_1 \rangle = \langle a_1^+ \rangle - \langle a_1^+ \rangle = 0. \tag{17}$$

Thus, Eq.16 reduces to $a_q = -M_{qp}$. A departure from the condition $a_q = -M_{qp}$ indeed signifies a break in the QTCM formulation. However, the resultant cancellation in terms in Eq.8 is evidently an effect of the finite difference approximations and not inherent to the conservation equations(Eq. 5). Thus, we intend the relaxation of $a_q = -M_{qp}$ only as a proxy measure to avoid the cancellation of terms in Eq.8 and permitting nonlinear interaction of moisture and other fields. This helps keep the analysis simple and aligned with the literature. We agree that this is a higher order approximation and needs to be clarified to the readers. We now include remarks in Section 3.3 of the manuscript and in the supplementary information to clarify these aspects. Since the requirement of such an adjustment would be waived if the finite difference formulation was modified accordingly, we also include a remark in the Supplementary Information briefly indicating possible modifications. The four-boundary finite difference formulation has been presented initially in this study to enable such modifications in the ensuing extension of this study.

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 v1 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 nonlinear 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 understanding: 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 $\langle a_1 V_1 \rangle$ in the first term on the right-hand sign.

Response 3: We thank the reviewer for suggesting corrections to the text. We have now simplified the derivations in the Supplementary information, along with the corrections to notations and advection terms. The LSSS geometry in itself doesn't require $u_B = u_D$ and $v_B = v_D$, and such a choice in this study is in line with assumed zonal symmetry. As stated earlier, the four-boundaried column framework for finite difference is framed keeping further extensions of the work in mind. The assumption of zonal symmetry is also to keep the discussion simpler and in line with the lower order models we are comparing the present model to. The LSSS geometry can permit non-zero temperature gradients and rotation as well. We now include a detailed remark on these aspects in the supplementary information for clarity. Also, the derivations are simplified, shortened and the terms are elaborately explained. We have corrected the missing $\langle a_1 \rangle$ and $\langle b_1 \rangle$ coefficients in the equations. The missing coefficients do not affect the steady state equation and the resulting equilibria and it is only a typographical error. Also, the equations 2 and 3 in the manuscript are corrected and clarified. The variables H and E are taken to be zero following Boos and Storelvmo [2016]. This is again to keep the discussion simple, since the main intent of the study is to focus on the bifurcation scenarios in the QTCM lower order models. Indeed, there are limitations to this approach and the considered model could be further extended and improved, as suggested by the reviewers. Such efforts are already underway as a part of the larger study. The terms Hg/p_T *etc.*, indeed result from vertical averaging and are better represented as vertical averages $\langle \cdot \rangle$. This is mentioned so in the revised manuscript. The equations 4 and 5 are also corrected and clarified. Also, additional references have been added to the Introduction section in the manuscript to place the present lower order model in context, with respect to other simple monsoon models.

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.**

   Response 4: The values for the various parameters are now included in the supplementary information.

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 $\langle a_1 V_1 \rangle$ 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 Bel-**

lon and Sobel 2010), which would modify $V_1(p)$ and multiple other parameters ($\langle a_1\rangle$, $\langle a_1V_1\rangle$, M_{sp}, $\langle b_1V_1\rangle$, M_{qp}) in a physically consistent framework.

Response 5: We thank the reviewer for the remark and the suggestion. Part of this response is given in the response to Reviewer 1 and is reproduced here for ease.

The thermodynamic equation relates the thermal advection and thermal stratification to the diabatic energy input into the column, *i.e.*, precipitation and vertically integrated radiative fluxes. The steady state thermodynamic equation for the case of precipitation $P > 0$ is,

$$\langle a_1V_1\rangle v\frac{\partial T}{\partial y} + M_s\frac{\partial v}{\partial y} = P + \frac{Rg}{p_T} \tag{18}$$

Typically, the thermal stratification term- also representing the effect of adiabatic cooling- is the leading order term in the equation[Boos and Storelvmo, 2016]. The stratification coefficient $M_s = M_{sr} + M_{sp}T_{1L}$ is designed to be positive [Neelin and Zeng, 2000], leaving the sign of the stratification term to depend only on the sign of the meridional velocity gradient. With the assumption that meridional velocity vanishes on land boundaries, $\partial v/\partial y = (v_{1L} - v_{1s})/L$ means that the sign of the meridional velocity gradient is opposite to that of the meridional velocity value at the sea boundary, v_{1s}. For a landward monsoonal wind flow, as expected when the column energy input $R > 0$, $v = v_{1s}$ is negative (upper tropospheric flow as in [Neelin and Zeng, 2000], approximation $v = v_{1s}$ as in Boos and Storelvmo [2016]). As a consequence, the adiabatic cooling term is positive for non-zero precipitation. Using the momentum equation to replace the temperature gradient in the advection term, the product of v_{1s} and the temperature gradient turns out to be a quadratic term in v_{1s} with a negative sign. This means that the sign of the advection term $\langle a_1V_1\rangle v_{1s}\frac{\partial T}{\partial y}$ is positive if $\langle a_1v_1\rangle$ or a_T is negative and together with the stratification term, it can balance the precipitative and radiative forcings. If the stratification term were too large compared to the advection term, it might also be the case that positive a_T, and consequently negative advection term could still be mathematically permissible so that the net effect of the two terms still balances the forcings. However, if $M_s = 0$, then evidently a_T is bound to be negative for the balance to be intact. Thus, a positive a_T could work well for the case with dominant stratification but not so for the no-stratification case. A negative value of a_T should work well for both the cases, on the other hand. As seen in the analysis ahead, a similar scenario appears to be the case with the model by Boos and Storelvmo [2016] with $a_T > 0$ guaranteeing a positive advection term.

Let V_s be the lower tropospheric meridional velocity used in the model by Boos and Storelvmo [2016]. Effectively, $V_s = -v_{1s}$, for the same parameters and assumptions. The thermodynamic equation for the corresponding model, after replacing the derivatives with finite difference approximations is

$$\frac{a_T\epsilon_1 + M_{sp}\epsilon_1}{\kappa}V_s^2 + (M_{sr} + M_{sp}T_s)\frac{V_s}{L} = P + R. \tag{19}$$

The corresponding equation for the lower order model from QTCM, formulated in this study is,

$$\frac{-a_T\epsilon_1 + M_{sp}\epsilon_1}{\kappa}v_{1s}^2 - (M_{sr} + M_{sp}T_s)\frac{v_{1s}}{L} = P + \frac{Rg}{p_T}. \tag{20}$$

For Eq.20, consider the case of $P = 0$. The resulting quadratic equation in v_{1s} has roots which are real if,

$$\frac{(M_{sr} + M_{sp}T_s)^2}{L^2} - 4\frac{(-a_T + M_{sp})\epsilon_1}{\kappa}(-Rg/p_T) > 0. \tag{21}$$

Considering that we seek real roots of v_{1s} for all $R > 0$, the second term in the discriminant is always positive if $a_T < M_{sp}$, and this guarantees that real roots occur for all $R > 0$. However, depending on the magnitude of M_{sr} and M_{sp}, $a_T > M_{sp}$ also can yield real roots, affirming the discussion above. Yet, in that case, there will always be a higher value of R for which the second term in the discriminant has an absolute value higher than $(M_{sr} + M_{sp})^2/L^2$, and the discriminant would be negative. Thus, real roots for all $R > 0$ occur only if $M_{sp} - a_T > 0$, or in other words, the quadratic coefficient in Eq.20 is positive. As M_{sp}, along with M_{sr}, is reduced from their standard value to 0, the need for change in sign of a_T arises when the condition $M_{sp} - a_T = 0$ is satisfied. For the present study, this critical condition is met when M_{sp} is reduced to 73% of its standard value.

Note that if value of M_{sp} is markedly smaller (say, by an order) than a_T, than the sign of a_T decides the existence or otherwise of real roots for $R > 0$. For the QTCM based low order model studied in this paper, this means that negative a_T almost always yield real roots for all $R > 0$. Thus, a change in sign when M_s is reduced is not required if $a_T >> M_{sp}$. When a_T and M_{sp} are comparable, as in the values suggested in Neelin and Zeng [2000] (where $a_T > 0$, $a_T < M_{sp}$), the existence of real roots can change with slight changes in value of a_T. Boos and Storelvmo [2016] remark that the changing of sign of a_T with reduction of M_{sp} is only optional and serves to relate their results with those of Levermann et al. [2009]. This possibly follows the fact that in their study, $|a_T| >> M_{sp}$ (supplementary to [Boos and Storelvmo, 2016]).

Notably, Neelin and Zeng [2000] use an $a_T > 0$, and the same is used in the present study for conformity with Neelin and Zeng [2000] and Boos and Storelvmo [2016]. The preceding discussion indicates that a different choice ($a_T < 0$) should avoid the requirement of change in sign of a_T as stratification is suppressed. However, note that the temperature basis vector is positive across the pressure variable (in QTCM) and the baroclinic velocity basis is positive with a large value at the top. The basis functions as used in Zeng et al. [2000] indicate that $a_T = \langle a_1 V_1 \rangle$ would be positive. Thus, changing the sign of this coefficient would require modifying the basis functions themselves. This is beyond the scope of the current study and the present study follows the a_T sign change practice as followed in Boos and Storelvmo [2016], in accordance to Neelin and Zeng [2000].

As the reviewer suggests, studying the sensitivity to the profile of temperature perturbation $a_1(p)$ is a more robust and physically consistent approach to the same issue. This is being conducted currently as a part of a larger study on the sensitivity of the model to changes in various parameters.

Minor edits:

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

   Response: The said references have been corrected in the revised manuscript.

2. **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.**

   Response: Figure 2 and Figure 3 in the manuscript have their captions mentioning ‘thick solid lines’. Though redundant and also not obvious in Figure 2, the solid lines are indeed thicker- as seen in Fig. 3 when compared against the dashed lines. However, since they don’t seem to add any new insight to the figures and are prone to confusion, we have replaced ‘thick/thicker solid lines’ with ‘solid lines’.

3. **Readers should not have to read Boos and Storelvmo (2016)'s article to know what a_T and a_q mean.**

   Response: We thank the reviewer for the suggestion. a_T and a_q are now defined in Table 1 in the manuscript.

4. **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.**

   Response: We thank the reviewer for the explanation. The remark regarding static energy variation across altitude has also been modified appropriately.

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