Response to Reviewers for
"The Non-monotonicity of Moist-Adiabatic Warming"

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I thank all three reviewers for their thoughtful comments and feedback, which helped improve the manuscript. My responses to reviewer comments are in **bold**.

Reviewer #1

This manuscript provides an explanation for why moist-adiabatic warming is non-monotonic as a function of the initial surface temperature. In addition, it shows results for the implications for buoyancy and updraft velocity in convection. I found the results interesting and intuitive, but there are some issues with the manuscript that would have to be addressed for publication.

Major comments

line 52: Need to clarify that you are assuming surface saturation throughout the paper (i.e. surface relative humidity is 100%). Also, I think you may be neglecting latent heat of fusion. If so, would this affect the results significantly?

I now clarify that the air is saturated from the surface throughout the paper (line 45, 72, 106). Indeed, I neglect latent heat of fusion in the main analysis. Including the latent heat of fusion introduces a secondary peak in warming due to the additional heat released (new Fig. B1a). This effect is negligible in the upper troposphere (< 500 hPa) but can be important in the lower troposphere (> 500 hPa, new Fig. B1b). The impact of fusion is largest (T_s of peak moist-adiabatic warming shifts 6.01 K) at 727 hPa. I include these discussions in the new Appendix B (line 55–56, 410–431). Since the role of fusion is minimal in the upper troposphere and significantly complicates the analysis, I choose to neglect it in the main derivation.

equation 9: You are trying to find an expression for gamma_m, but the result involves gamma_m which is thus not satisfactory! Furthermore, when you take a T_s derivative, the results will involve dgamma_m/dTs which is what you are trying to find. You could instead bring all cases of gamma_m to the left hand side. This will result in a standard expression for gamma_m that involves q_s in the numerator and denominator (see e.g. the expression for the pseudo moist adiabatic lapse rate in the moist variables appendix in the Holton and Hakim textbook). Presumably then the non-monotonicity results from a competition between q_s in the numerator and denominator.

I now explain the source of the non-monotonicity starting with the standard expression for Γ_m (new Eq. 13, new Section 2). The terms α_L and c_L in this equation include the derivatives of q^* that you mentioned (new Eq. 11 and 12). I explain why Γ_m is non-monotonic with T_s starting from the functional form of the moist-adiabatic lapse rate (new Eq. 17 and lines 128–134). I then systematically derive the condition for the extremum in $\partial \Gamma_m / \partial T$ (line 135–163). With the new derivation I find the non-monotonicity emerges from a competition between two limiting factors of the condensation rate of rising air: the availability of water vapor and adiabatic cooling. At low T_s, warming increases with T_s because the increase in q^* at the surface is realized as latent heating. At high T_s, warming decreases with T_s because there

is a diminishing fraction of adiabatic cooling that remains available to be offset from latent heating. This competition leads moist-adiabatic warming to peak when these two factors are equally limiting, i.e. $L_v \partial_T q^* = c_{pd}$.

line 202: The dependence on T^2 is emphasized but you also found a dependence on gamma_m. You should show that the T^2 term is more important than the gamma_m term to support this.

The new explanation no longer relies on the $1/T^2$ dependence or the circular dependence on Γ_m (see above).

line 206: Can you provide or cite evidence from convection resolving simulations or observations that buoyancy and updraft velocity are non-monotonic functions of initial surface temperature? For example, Singh and O'Gorman, QJRMS, 2015 show a hint on non-monotonicity of updraft velocity but it is confined below 900hPa. If there is not support for this, then you need to say that with a number of major approximations you find non-monotonic behavior of B and w but this needs to be confirmed.

I analyzed updraft velocities exceeding the 99.9th percentile at each level in 9 RCEMIP simulations to evaluate the robustness of the variation in updraft velocity to surface temperature. Cloud-resolving models qualitatively support the findings from the zero-buoyancy plume model. All but one model (SAM) exhibit non-monotonicity in updraft velocity with surface temperature (new Fig. 8). The presence of a non-monotonicity and a shift toward increasingly top-heavy updraft velocity profiles in RCEMIP models suggest that the mechanism controlling non-monoticity in moist-adiabatic warming is playing a role in shaping the sensitivity of updraft velocity to surface temperature in models that explicitly resolve convective storms. I discuss this in the revised manuscript (line 306–322).

In addition to the new analysis above I now reference Singh and O'Gorman (2015) in the discussion of the non-monotonicity of vertical velocity (line 289–292) and Seeley and Romps (2015) who show that buoyancy varies non-monotonically with surface temperature (line 287–289).

Minor comments

line 8 and throughout the paper: "change in temperature along a moist adiabat is surprisingly non-monotonic with surface temperature". This is unclear as stated. What you mean is that it is non-monotonic with variations in initial surface temperature (or control-climate surface temperature). This should be clarified here and throughout the paper. Otherwise the reader may naturally think you mean non-monotonic as the surface temperature increases from control temperature to +4K which doesn't make sense.

I replaced all instances of "surface temperature" that appear in the context of describing the non-monotonicity in moist-adiabatic warming to a 4 K surface warming to "initial surface temperature" throughout the paper (e.g., line 8, 12, 52, 53, and so on). For discussions involving non-monotonicity of derivatives (e.g., $d\Gamma_m/dT_s$ and $\partial\Gamma_m/\partial T$) and dynamic quantities (buoyancy and vertical velocity) I continue to use "surface temperature" since there is no ambiguity in these contexts.

line 20 "The Clausius-Clapeyron relation describes...": change "describes" to "implies" (The CC relation in itself is not a property of air or the atmosphere)

I changed "describes" to "implies" as suggested (line 22).

line 24: Fig 1 shows the column water vapor which is not in any way the "total latent heat released from convection".

Thank you for catching this error. In a moist-adiabatic atmosphere, the total latent heat released from convection is $L_v(q_s^* - q_{top}^*)$ where L_v is the latent heat of vaporization, q_s^* and q_{top}^* are surface and cloud-top saturation specific humidity. In a purely moist-adiabatic atmosphere $q_{top}^* \to 0$ because $T \to 0$ so total latent heat release to first order scales as q_s^*, not column water

vapor as I originally stated. I now explain the assumptions involved in using q_s^* as a proxy for total latent heat release (line 45–50) and plot q_s^* vs T_s in the new Fig. 1a.

line 34: Again "total latent heating" is not appropriate here

This is now revised to surface specific humidity (line 51).

line 39: Interestingly, this non-monotonicity of the moist adiabat also affects the land-ocean warming contrast: see page 4003 of Byrne and O'Gorman, 2013 (DOI: 10.1175/JCLI-D-12-00262.1)

Thank you for sharing this. I now reference Byrne and O'Gorman (2013) in the introduction when pointing out that previous work has noted the existence of this non-monotonicity (line 57–59).

line 46: "as a function of initial surface temperature" or "as a function of control climate surface temperature" (and similarly throughout paper)

I now refer to "initial" surface temperature here and elsewhere in the paper (as I discussed in my response to your previous comment).

line 81: "increase in water vapor"→ "increase in specific humidity" (whether there is an increase depends on how water vapor is measured)

This sentence was removed in the revised manuscript.

line 145: I take it the assumption of the environment follows an entraining lapse rate whereas the parcel does not is following the general approach of the zero-buoyancy plume (e.g. Singh and O'Gorman, GRL, 2013)

Yes. I now reference the zero-buoyancy plume model (Singh and O'Gorman, 2013) when assuming the environment follows an entraining plume to understand updraft buoyancy (line 215–217).

line 173: Eq 27 seems to be a linear equation in w^2 rather than a nonlinear equation. You could instead say that solutions to Eq 27 depend nonlinearly on B.

This sentence was removed in the revised manuscript as a result of the new derivation of the non-monotonicity.

appendix: The text of the appendix should be cited somewhere in the main part of the paper

I now reference all appendices in the main text (line 56–57, 370).

Reviewer #2

Summary

In this manuscript, the author discusses how tropospheric temperature responds to change in surface temperature non-monotonically, when driven by deep convection. The author then discusses the implication of these changes on the buoyancy and vertical updraft energy of air-parcels lifted by deep convection.

Specifically, the amount of temperature increase at various levels of the troposphere and in response to a unit change in surface temperature is found to be non-monotonic with surface temperature. This sensitivity is such that tropospheric temperature increase in response to a unit surface temperature change is maximum at a specific surface temperature, with the response being smaller at colder or warmer surface temperature values. The author also shows that the surface temperature value that maximizes the tropospheric temperature response is different for different tropospheric levels. The author then concludes by demonstrating that, in ideal scenarios, the non-monotonic changes in the lapse rate implies non-monotonic changes in other important properties of convective air parcels, namely their buoyancy and the kinetic energy of their updraft.

Apart from some minor issues, I find this study interesting and worthwhile publishing. While the non-monotonic changes in the lapse rate is not a novel finding, as pointed out by the author, the mechanism put forward by the author to explain it (i.e., linking this non-monotonic behavior to a compensation between the temperature and pressure dependence of saturated specific humidity) is novel. Furthermore, its implications on the buoyancy and energetics of convective air parcels is original and, in my opinion, a substantial departure from previous mechanisms proposed to explain those properties.

Based on the feedback from reviewers 1 and 3, I rederived the source of the non-monotonicity in moist-adiabatic warming based on an explicit equation for the moist-adiabatic lapse rate (new Eq. 13). I systematically derive the condition for the extremum in $\partial \Gamma_m / \partial T$ (line 135–163). With the new derivation I find the non-monotonicity emerges from a competition between two limiting factors of the condensation rate of rising air: the availability of water vapor and adiabatic cooling. At low T_s, warming increases with T_s because the increase in q^* is realized as latent heating. At high T_s, warming decreases with T_s because there is a diminishing fraction of adiabatic cooling that remains available to be offset from latent heating. This competition leads moist-adiabatic warming to peak when these two factors are equally limiting, i.e. $L_v \partial_T q^* = c_{pd}$.

Some minor comments

L. 22, 24, 34: I don't think that 'latent heat release' is the correct term to use here. Rather, I think that 'saturated water vapor content' or perhaps 'precipitable water' would be appropriate. The term "latent heat release" should be reserved to the process of phase change itself, which is not what is shown on Fig.1 and isn't even directly discussed in this paper. Changes in latent heat release are more directly connected to changes in the radiative balance of the atmosphere, which is not the focus of this paper.

Indeed, the original Fig. 1a was not well connected to the latent heat released by the moist adiabat. I now show surface specific humidity (q_s^*) as a function of surface temperature in Fig. 1a. I use Fig. 1a to set up the first-order expectation of how total latent heating in a *purely moist-adiabatic* atmosphere scales following Clausius-Clapeyron through q_s^* and as a result of this one might expect moist-adiabatic warming at fixed levels to also be a monotonic function of surface temperature. I now explain the assumptions involved in using q_s^* as a proxy for total latent heat release and more explicitly connect this to moist-adiabatic warming (line 45–50).

L. 31: "(...) because it increases [dry] atmospheric static stability" I suggest precising 'dry', in contrast to 'moist static stability', which does not increase monotonically with height.

I now specify dry static stability (line 39).

L. 32: "(...) which [whose spatial structure] influences convection the organization of the tropospheric dynamics [in the tropics] (Neelin and Held 1987)." I'm suggesting changes that may be more in line with

NH87's argument. Note however that NH87 argument is based on the gross moist static stability, which differs from dry static stability.

Dry static stability still plays a role because it is a part of the definition of gross moist stability (Eq. 3.2 in Neelin and Held 1987). Spatial variations in the dry static stability response influence the position of convergence zones because horizontal free-tropospheric temperature gradients, while weak, exist (Bao et al. 2022). I revised the main text to clarify this point (line 39–41).

L. 66: "the first law of thermodynamics for a [adiabatic, non-precipitating, reversible] saturated, ascending air parcel, which is equivalent to the conservation of Moist Static Energy (MSE)". I think it is important to specify that we're dealing with an idealized set of conditions.

I rephrased this sentence following your suggestion (line 105–106).

L. 67: In equation 4 and other places in the text, I would suggest using a different subscript than 's' for saturated specific humidity, to differentiate it better from the surface subscript used for surface temperature. For instance, you could use subscript 'sat' (qsat), or a superscript '*' (q*).

I now use the superscript $*$ for saturation and subscript s for surface (e.g., q_s^* is surface saturation specific humidity, see line 42, Eq. (8), and elsewhere in the revised manuscript).

L. 79: "the sum of a Cooling [Temperature] Term and a Pressure Term". For consistency, I would either use the term pairing 'Cooling'-'Warming' or 'Temperature'-'Pressure'.

This sentence was removed in the revised manuscript as I rewrote the explanation for the non-monotonicity.

L. 80: "The Cooling Term represents the decrease in water vapor due to the parcel cooling as it rises and expands". Here, you're referring to a partial derivative evaluated at constant pressure.

This sentence was removed in the revised manuscript as I rewrote the explanation for the non-monotonicity.

Suggestion for further analysis:

L. 232: There are other empirical definitions for saturated vapor pressure that are considered to be more accurate than Bolton, in particular Goff–Gratch and Murphy-Koop formula. I'd be curious to know whether using a different definition would impact the non-monotonic behavior in a significant way (e.g., is the peak warming occurring at the same temperature?)

I analyzed the sensitivity of the non-monotonicity to the choice of the saturation vapor pressure formula by comparing the results using Bolton (1980), Goff-Gratch (1946), and Murphy-Koop (2005). The surface temperature where warming peaks varies by less than 0.16 K across different e^* formula (new Fig. C1, new Appendix C). Thus the choice of saturation vapor pressure formula does not significantly impact the non-monotonicity. I now mention this in the revised introduction (line 54–57).

Reviewer #3

This short paper notes that the moist adiabatic amplification of tropospheric warming relative to the surface is non-monotonic in surface temperature Ts. The paper furthermore seeks to explain this in terms of the basic thermodynamics of the moist adiabat, and in particular the separate pressure and temperature dependence of saturation specific humidity qs.

This is a nice idea, and I think there is room in the literature to deepen our understanding of the moist adiabat. At the same time, I felt that the submitted manuscript fell short of this goal in a few ways, which I detail below. If it is possible to address these concerns, however, this paper could then make a nice addition to the literature.

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Based on the your feedback and similar feedback I received from reviewer 1, I rederived the source of the non-monotonicity in moist-adiabatic warming starting from an explicit expression of the moist-adiabatic lapse rate (new Eq. 13). I now derive the condition for the extremum in $\partial \Gamma_m / \partial T$ (line 111–148). I describe the new derivation and its physical interpretation in more detail below.

Major Comments

1. The author is trying explain how the moist lapse rate Gamma_m depends on Ts, and does this by writing d Gamma_m/dTs in terms of dqs/dz, which is then itself written in terms of Gamma_m in Eqs. (10) and (15)! This feels somewhat circular.

I rewrote the explanation of source of the non-monotonicity starting with an explicit expression for Γ_m (new Eq. 13, Section 2). I explain why Γ_m is non-monotonic with T_s starting from the functional form of the moist-adiabatic lapse rate (new Eq. 17 and lines 128–134). I then systematically derive the condition for the extremum in $\partial \Gamma_m / \partial T$ (line 135–163). The non-monotonicity emerges from a competition between two limiting factors that control the condensation rate of rising air: the availability of water vapor and adiabatic cooling. At low T_s, warming increases with T_s because the increase in q^* is realized as latent heating. At high T_s, warming decreases with T_s because there is a diminishing fraction of adiabatic cooling that remains available to be offset from latent heating. This competition leads moist-adiabatic warming to peak when these two factors are equally limiting, i.e. $L_v \partial_T q^* = c_{pd}$.

2. Furthermore, it is noted in lines 107-108 that this Gamma_m dependence is important, but this is never quantified and is later ignored in lines 200-204 and the caption to Fig. 4. I would in fact suspect that the Gamma_m dependence is key to the sensitivity of the cooling prefactor to Ts in the upper troposphere in Fig. 4a, as Gamma_m will vary there from dry adiabatic values at Ts=280 to roughly half that at Ts=310, which is a factor of 2 difference, whereas T will only vary by a few tens of percent.

The new derivation no longer includes an implicit dependence on Γ_m.

3. The author notes that their perspective must be equivalent to that of Romps 2016, which explains the decline in upper tropospheric warming as due to warming parcels retaining more of their latent heat and thus not expressing their change in surface enthalpy through sensible heat. Could the author more explicitly (perhaps analytically?) show how these perspectives are equivalent?

The new criteria I derive for the non-monotonicity in moist-adiabatic warming $c_L = c_{pd}$, where $c_L = L_v \partial q^* / \partial T$, is the same criteria that Seeley and Romps (2016) and Romps (2016) found for explaining the top-heavy structure of buoyancy and the non-monotonicity of buoyancy with surface temperature. However, the same criteria emerge independently from two different equations (sensitivity of moist-adiabatic lapse rate to warming $\partial \Gamma_m / \partial T$ vs the temperature difference between an entraining and undiluted parcel derived from the difference in entraining and undiluted moist static energy). The derivation for the non-monotonicity of buoyancy that

I present in the revised manuscript is a generalization of Romps (2016). It includes the role of the entrainment parameter a on the criteria for peak buoyancy $c_L = c_{pd}\sqrt{1+a}$ (new **Fig. 6c**, line 231–263).

4. The author spends several figures (Figs. 5-8) on the implication that vertical velocities should change non-monotonically as a function of Ts, but I'm not sure this result warrants that much emphasis. Is there any evidence of such non-monotonicity anywhere else in the literature, e.g. from cloud-resolving simulations?

I analyzed updraft velocities (vertical velocity exceeding the 99.9th percentile at each level) in 9 RCEMIP simulations to evaluate of the robustness of the variation in updraft velocity to surface temperature. Cloud-resolving models qualitatively support the findings from the zero-buoyancy plume model. All but one model (SAM) exhibit non-monotonicity in updraft velocity with surface temperature (new Fig. 8). The presence of a non-monotonicity and a shift toward increasingly top-heavy updraft velocity profiles in cloud-resolving models suggest that the mechanism controlling non-monoticity in moist-adiabatic warming is playing a role in shaping the sensitivity of updraft velocity to surface temperature in models that explicitly resolve convective storms. I discuss this in the revised text (line 306–322).

In addition to the new analysis above I now reference Seeley and Romps (2015)and Singh and O'Gorman (2015) who show that buoyancy and updraft velocity vary non-monotonically with surface temperature (line 287–292).

As a result of the rederivation of the non-monotonicity and the decision to highlight new insights gained from the revision, only Fig. 7 out of the original Fig. 5–8 remains in this revised manuscript.

5. The regime in which d Gamma_m /dTs is decreasing with Ts seems perhaps related to the "moist greenhouse" regime, in which qs does not decline significantly with height (e.g. Wordsworth and Pierrehumbert 2013, ApJ, see their Fig. 2). If true this would be a nice connection to make.

The criteria that separates the non-monotonicity in moist-adiabatic warming and the moist greenhouse regime may appear similar but are distinct. The non-monotonicity of moist-adiabatic warming, buoyancy, and updraft velocity corresponds to where the *temperature sensitivity* of latent and sensible enthalpy are equal, i.e., $L_v\partial_T q^* = c_{pd}$. The transition to a moist greenhouse regime corresponds to where the *magnitude* of surface latent and sensible enthalpy are equal, i.e., $L_v q_s^* = c_{pd}T_s$ (from Eq. 18 in Wordsworth and Pierrehumbert 2013). The surface temperature of peak buoyancy at the tropopause (and so CAPE, following Romps 2016) and the transition to a moist greenhouse regime are coincidentally (similar ≈ 335 K) but there is no reason to believe a priori that this similarity should hold across a broad range of planetary climates. I include this discussion in the revision (line 361–372, Fig. D1, and Appendix D).