Response to Reviewers: Sensitivity of tropical orographic precipitation to wind speed with implications for future projections

Summary for all of the Reviewers and the Editor:

We thank the reviewers for providing thorough and thoughtful comments. We were pleased to hear that they expressed appreciation for the manuscript. We have strived to address all of the reviewers' comments, making changes to the manuscript in response to most of them and giving a clear response and reasoning for any that did not result in manuscript changes. We give an overview of these changes below.

The reviewers' main comments related to our analysis of the changes in boundary-layer equivalent potential temperature induced by changes in the background wind speed. Specifically, they pointed out that our explanation of the change in the three-way balance between horizontal advection, vertical advection, and diabatic sources was unclear, that we were showing little evidence of the deepening of the mountain wave in the boundary layer, and that alternate explanations based on changes in diabatic sources needed to be explored. In order to address these comments, we have changed the text of section 3.3 (especially the 4th and 5th paragraphs) in depth. We have explained in more detail why we consider a simplified budget of θ_e with fixed horizontal wind and vertical θ_e gradient (and we have better shown that the residual term arising from these approximations is small upstream of the mountain). We have stressed that both horizontal and vertical advection increase, but that vertical advection increases faster than the background wind U, forcing a steepening in the horizontal θ_e gradient to maintain the balance. Finally, we have stressed that changes in diabatic sources of θ_e are unlikely to explain its change in response to increased U.

To complement these changes to the text, we have modified Fig. 3C in two ways. First, we have changed the range of the x-axis to zoom into the region (-500 km, 0 km), immediately upstream of the mountain top, which is where our discussion of the figure in the manuscript concentrates. Second, we have highlighted the magnitude of each term in the differential budget of θ_e in the region of interest (that is, where the increase in boundary-layer θ_e occurs, from -100 km to -50 km). We have also added a new figure which shows the deepening of the mountain wave at low levels, using the vertical motion field, in both simulations and theory. Finally, we have added a new section (section 3.4) discussing the changes in lower-free-tropospheric moisture in simulations. This section is accompanied by second new figure diagnosing the lower-free-tropospheric moisture budget.

Section 2 also saw some changes in order to clarify the presentation of the theory, giving more detail about the convective closure and underlining the role of the background moisture profile in setting the orographic precipitation perturbation and its sensitivity to U. One other important change to the manuscript arose in the introduction in a response to a comment from reviewer 2: we have added two paragraphs discussing the literature on midlatitude orographic precipitation relevant to the present study. The rest of the changes came in response to several minor comments and are scattered throughout the manuscript.

Below are our responses to the reviewer comments and, in **bold font,** our description of the corresponding changes to the manuscript. The reviewer comments are indicated in black and our responses are in blue font.

Reviewer 1

In this paper, the authors apply a theoretical framework they developed in previous work to evaluate the sensitivity of tropical orographic precipitation to changes in background wind speed. The theory predicts that precipitation increases with increasing wind speed by 20-30% per m/s, and find good agreement with this prediction in a numerical simulation using the Weather Research and Forecasting (WRF) model. They also find qualitative agreement with their prediction in observations.

The paper is well written. The result that tropical orographic precipitation doesn't scale in proportion to the horizontal wind speed is interesting and has important implications for climate change impacts given possible future changes in the tropical circulation. The agreement between theory, models, and observations is imperfect but mostly convincing.

The main room for improvement, in my view, relates not to the analysis per se but to the way it's explained. I like the authors' approach of combining linear mountain-wave theory with a convective closure to study orographic enhancement of precipitation over tropical mountain ranges like the Western Ghats. However, aside from the two previous studies published by the same authors, this is a novel approach, and one that I suspect many readers will not be very familiar with. I think the authors should do more to explain the physical basis for their approach, including, for example, why they assume a dry static stability for the linear mountain wave solution, and why the convective closure of Ahmed (2022) makes sense in this context. I also think the authors could hold the reader's hand a bit more on some of the more technical parts of the paper, especially when using their equations to draw physical conclusions. I note some specific areas of confusion for me below.

We thank the reviewer for raising to our attention these points of confusion about our orographic precipitation model. Many studies focusing on midlatitude orographic precipitation have used moist static stabilities, because they considered the atmosphere to be fully saturated in the vicinity of orography on the short time scale of a midlatitude storm. Over the seasonal time scales considered here, the atmosphere is not saturated and the use of a dry static stability proves to be more appropriate: for example, the temperature perturbations obtained using a dry static stability in Fig. 1B are qualitatively similar to the simulations, whereas a moist static stability would imply much weaker perturbations with a much larger vertical wavelength. **We have added a sentence clarifying why the use of a dry static stability is appropriate here (after equation 2).**

As for the Ahmed et al (2020) convective closure, **we have modified the second paragraph of section 2 to provide more justification for its use in the context of orographic**

precipitation. This closure relies on an empirical relationship between tropical precipitation and a bulk measure of convective plume buoyancy. This relationship was shown to hold across all tropical ocean and land regions, including in regions of mechanically forced orographic precipitation.

Sections 2 and 3: I may have missed it, but I couldn't find any mention of the vertical humidity profile in either the theory or numerical modeling. Isn't that important given that the lower tropospheric humidity perturbation is based on the vertical humidity gradient (Eq. 2)?

The answer to this question is that the lower-free-tropospheric vertical humidity gradient is important in setting the orographic precipitation rate P', but not in setting its ***relative*** sensitivity dln(P')/dU. To understand this, let us first recall that P' is the sum of a precipitation perturbation due to the adiabatic mountain wave P_a, and a precipitation perturbation due to the convective feedback P_m. P_a dominates P' and its sensitivity to U. In equation 2, one sees that P_a strongly depends on the vertical humidity gradient. However, the only term in the right-hand-side of equation 2 that depends on U is the lower-tropospheric vertical displacement η_L. Thus, changing the vertical humidity gradient will affect P_a, but not its relative sensitivity d $\ln(P_a)$/dU. The moisture lapse rate does have a small influence on the convective feedback P_m (it is hidden in the moisture relaxation length scale L_q in eqn. 3, via the gross moist stability term), but this is dwarfed by changes in P_a.

The above explanation has been added to the revised manuscript (section 2). In the initial submission, as pointed out by the reviewer, we had forgotten to quote the value of dq_0/dz that we used in the theory. We had also not made any mentions of the vertical gradient of specific humidity in the simulations. **In the revised version, the values of the vertical gradient of specific humidity (and static stability) in theory and simulations have been included at the end of Appendix A. We have stressed the important point that they do not change when the background wind U is changed in simulations.**

Line 93 and Fig. 1A: I understand why the depth of ascent would increase from a change in the vertical mountain wavelength, but I'm surprised there's not an increase in the *magnitude* of eta (and thus T') from an increase in U, given the lower boundary condition w=u*dh/dx (Fig. 1b). Later the authors note that the ratio of w/U increases in response to an increase in U, which I would expect to further amplify the T perturbation. What am I missing?

The answer to the reviewer's question lies in the difference between vertical velocity (w) and vertical displacement (η). Both quantities have a wave-like structure in the vertical, with the same wavelength $\lambda_z = 2\pi U/N$. Their amplitude (or magnitude, as put by the reviewer) is set by the lower boundary condition. That of w reads w(z=0)=U*dh/dx, hence it increases proportionally to U. That of η is η=h, which does not depend on U. Hence, the amplitude of η does not change with U (in contrast to its vertical wavelength, which does change with U). One way to reconcile these two results is to note that η obeys dη/dx = w/U. While the amplitude of w increases proportionally with U, that of w/U does not, and neither does that of η.

The second part of the reviewer's question concerns our later point that w/U increases when averaged over the boundary layer. While the amplitude of w/U does not change with U, its wavelength $\lambda_z = 2\pi U/N$ increases with U. Over the mountain's upwind slope, this leads to an increase in w/U in the boundary layer (see the new Fig. 4 in the revised manuscript). As hinted at by the reviewer, the same effect occurs in the lower free troposphere. This does lead to a strengthening of the T perturbation in the lower free troposphere, but this is in fact the exact same process as explained in section 2. In section 2, we write $T'_a = - \eta\, ds_0/dz$, and invoke an increase in η in the lower-free-troposphere. Equivalently, one can write $dT'_a/dx = -(w/U)ds_0/dz$, an invoke an increase in w/U in the lower-free-troposphere. Both describe the same physical process.

We have added a sentence in the revised manuscript to stress that the amplitude of η does not change with U (in the second-to-last paragraph of section 2). The revised manuscript also features a new section (3.4) that clarifies the link between our argument for the increase in the T & q perturbations in the lower free troposphere, and that for the increase in θ_e in the boundary layer (both based on the vertical stretching of the mountain wave).

Section 3.3: I found this discussion of the role of theta_e' to be confusing. The basic argument, as far as I can tell, is that there's a theta_e perturbation that contributes to precipitation enhancement, and that this is tied to the greater vertical penetration of the mountain wave when U increases, since an increase in the vertical wavelength causes w/U to increase above the surface, and this in turn increases the vertical advection of theta_e. First, I didn't find the evidence in Fig. 3c to be especially convincing given the large magnitude of the residual term.

The reviewer's interpretation of the basic physical explanation is correct: in the WRF simulations, about half of the increase (see Fig. 2C) in P' is due to an increase in the boundary layer θ_e perturbation, over the mountain, with increased background wind (Fig. 2C shows, specifically, that this θ_e perturbation is just as important as the T_L' perturbation). In Fig 3C, we diagnose the change in horizontal gradients of boundary layer θ_e as a sum of changes in vertical advection divided by the background wind, diabatic sources divided by the background wind, and a residual (due to horizontal variations in the horizontal wind, and in the vertical gradient of θ_e). The residual term is small in the region of interest, where the increase in θ_{eB} occurs (upstream of x=-50 km). In order to stress this fact, **we have modified Fig. 3C to zoom into the region of interest, upstream of the mountaintop, and included a small histogram showing the magnitude of each term in the region where θ_{eB}' increases**. It is now more readily apparent that the residual is small in the regions discussed in the text. Downstream, the residual term is higher because the horizontal wind u deviates strongly from the background wind U when approaching the mountaintop.

Also, shouldn't the same increase in vertical advection also apply to T and q? In reality, the WRF simulations show no contribution from q' and a relatively modest contribution from T'. I think this disagreement between the theory and WRF simulations should receive more attention.

The reviewer is correct in pointing out that this increase in vertical advection applies to T and q (although, for these two quantities, the increase needs to occur in the lower free troposphere, and not in the boundary layer). This is in fact the argument made in section 2. The contribution from T' is not modest: it accounts for half of the increase in the orographic precipitation anomaly (see Fig. 2C). Moreover, the increase in T' is well captured by the theory (see Fig. 1B which illustrates the deepening of the mountain wave in the T' field in theory and simulations).

However, the increase in q' is indeed almost non-existent in the lower free troposphere. **We have added a new section (section 3.4) to the manuscript to diagnose the reason for this stagnation in q_L' when the background wind is increased**. In this new section, we discuss the parallel between the arguments made for the increase in q_L' in section 2, and for the increase in θ_{eB}' in section 3 (both related to the deepening of the region of ascent in the mountain wave). **We also discuss a new figure (Fig. 5). This figure decomposes, in the same spirit as Fig. 3C, the changes in horizontal gradients of q_L' with increased U into changes in vertical advection, convective sources, and a residual.** The outcome of the analysis is that the vertical advection term does increase faster than the background wind in simulations (as predicted by theory), but that an increase in the convective moisture sink counteracts this change in vertical advection.

In summary, I think this is an important contribution, but I wish it provided a clearer physical explanation for why tropical orographic precipitation is so sensitive to perturbations in U. The simple theory that involves q and T seems relatively straightforward (aside from assumptions about the vertical profile of q). However, while this theory gives an accurate prediction for the precipitation increase simulated by WRF, it seems to do so for the wrong reasons, since WRF shows a negligible contribution from q' and a large contribution from theta_e' (Fig. 2c). **I think the authors should better explain how their theory can be reconciled with the WRF results.**

We thank the reviewer for their appreciation of the manuscript, and for their very helpful comments. We agree with the fact that the quantitative agreement between the theory and the simulations on the precipitation increase with U is in part fortuitous, and had noted so in the original version of the manuscript. However, we emphasize that half of the precipitation increase in the simulations comes from a cooling of the lower free troposphere which is well captured in the theory; it is thus not accurate to make the blanket statement that the theory gives an accurate prediction “for the wrong reasons”.

In the revised version of the manuscript, we have clarified our discussion of the changes in boundary layer θ_e, and modified figure 3C to stress that the residual term is small in the region of the peak precipitation on the windward slope. We have also added a new section that shows the connection between the argument made in section 3.3 for the increase in boundary layer θ_e, and the theoretical argument behind the strengthening in lower-free-tropospheric T' and q' with increased U – both are due to an increase in the vertical mountain wavelength. This new section also explains why simulations behave differently from the theory when it comes to the change in q_L (a larger change in the

convective moisture sink occurs in simulations than in the theory), thus helping to reconcile these two parts of the manuscript.

Reviewer 2

Summary: in this manuscript, the authors apply their new theory of mechanically forced orographic convection (Nicolas and Boos 2022) to evaluate the sensitivity of orographic precipitation to background wind speed. The theory is augmented by convection-permitting WRF simulations and observations from selected mountainous tropical regions. In the end, the authors conclude that small increases in winds in current and future climates may give rise to disproportionate increases in orographic precipitation.

I found the manuscript to be mostly well written and scientifically rigorous. The figures are clear but also very densely packed with information, and the authors dutifully explain each detail in their very long figure captions. In terms of the methodology, the theoretical model is already vetted and on solid footing and the complementary simulations and observational analyses add substantial value. The results are scientifically interesting and mostly convincing. Overall, this is a strong manuscript, and my feedback consists of one major comment along with a number of minor and/or technical comments.

Major comment

1. The physical explanation for the enhanced \theta_e over the windward slope in the U=12 m/s case in Figs. 3A-B is not convincing to me. The authors attribute this enhancement to increased vertical advection of \theta_e in Fig. 3C. They argue that this increase must be associated with a deeper mountain wave, which is evident at upper levels (well, above 800 hPa) in Fig. 1B. However, the clear enhancement in \theta_e in Fig. 3A is confined to the 900-1000 hPa layer, not above 800 hPa. Moreover, some of this enhancement reaches all the way down to the surface in Fig. 3A, where the authors concede that the mountain wave cannot explain the enhancement.

   This first point raised by the reviewer is that the deepening of the mountain wave is only evident above the boundary layer when looking at the T' profiles in Fig. 1B. This happens as the boundary layer has a dry neutral stratification, hence T'~0 there (due to vertical motion anomalies) irrespective of the background wind. We recognise that in the original version of the manuscript, we did not show any direct proof that the increase in vertical advection divided by the background wind (shown in Fids 3C-D) was due to a deepening of the mountain wave – we merely showed that it was captured by a theoretical model (Long's equation) which itself represents this deepening.

   In order to show this deepening within the boundary layer, we need a field other than T'. Hence, **we added a new figure (figure 4 in the revised manuscript) which shows vertical profiles of w/U over the mountain's upwind slope, in simulations and in theory** (using

Long's equation). It is apparent that both simulations and theory feature a similar vertical stretching of w/U that extends through the boundary layer and results in similar values of the change in w/U when averaged over the boundary layer (Fig. 3D).
The reviewer's last point is that our argument of increased vertical advection relative to the background wind cannot apply at the surface, to which we agree. We note that the enhancement at the surface in Fig. 3A is weaker than aloft in the boundary layer, but that another explanation would be needed there, possibly based on changing surface fluxes or changing convective transports. We have not delved into that question.

I'm skeptical of this interpretation because, although vertical advection is stronger at larger U, so is horizontal advection, which tends to reduce \theta_e. Also, if the air crosses the mountain in both cases (which it does), and \theta_e is in fact conserved, the \theta_e over the mountain top should be the same in both cases even though the terms in the \theta_e budget may differ in magnitude. Therefore, my sense is that more analysis is needed to properly interpret this enhancement, which is important because it is largely responsible for the disproportionate increase in P' with U. One possibility is that relative flow deceleration caused by the mountain wave over the windward slope (relative to the background wind speed U) may be smaller in the U=12 m/s case. If so, the near-surface W would be disproportionately enhanced.

Thanks to the reviewer's comment, we now recognize that the discussion in our original manuscript was unclear. Our point is precisely that both horizontal advection and vertical advection increase at the same rate, but that vertical advection increases faster than the background wind U, requiring a steepening in horizontal θ_e gradients to maintain the balance. **We have modified the text of section 3.3 in depth (4th and 5th paragraphs) to clarify this point in the revised manuscript.**
The reviewer raises the point that the conservation of θ_e should imply, if streamlines don't intersect the mountain's surface, that θ_{eB}' should not change with U. It is important to keep in mind that this conservation is only valid along streamlines. Thus, we would agree with the reviewer's statement if we were averaging θ_e between two streamlines. Here, however, we are averaging between two fixed pressure levels that are being crossed by streamlines. Hence, neither $\theta'_{e[900\text{-}950]}$ nor θ_{eB}' need stay fixed even if θ_e is conserved along streamlines. In fact, one way to understand the increase in θ_{eB}' with increased U is the following: above the mountain, the boundary layer contains streamlines that originate at lower heights for U=12 m/s than for U=10 m/s, because the deepening of the mountain wave deflects the streamlines higher. In the basic state, θ_e is higher at low levels, hence its boundary-layer average increases when U increases.
The reviewer raises a second interesting point, which is that an increase in w/U may occur as the result of an increase in u/U (given that w = u*dh/dx, with u<U the near-surface horizontal wind), i.e., a weaker deceleration of the near-surface flow with increased U. We had not considered this possibility, but the new Figure 4 shows that it does not occur in simulations: the near-surface value of w/U stays approximately fixed, increasing much less than w/U in the middle or upper boundary layer or the lower-free troposphere.

Also, because this is an orographic precipitation problem, one can never rule out cloud microphysics as a driver of changes. Is it possible that the U=12 m/s case sees a larger share of the precipitation evaporating over the lee slope, as opposed to over the windward slope? Weak evidence of this is seen in the slight downstream shift of the precipitation peak for the U=12 m/s simulation in Figs. 3A-B. Such a change could reduce evaporative cooling over the windward slope, thereby enhancing boundary-layer \theta_e there.

While the evaporation of precipitation into an air parcel is a process which conserves θ_e, precipitation-driven downdrafts can indeed carry lower-θ_e air into the boundary layer. The reviewer suggests that a reduction in precipitation-driven downdrafts over the upwind slope with increased U (as a result of stronger hydrometeor drift) may explain the increase in θ_{eB}'. While this is an interesting point, we believe that such an effect would translate into a reduction in θ_e sources in Fig. 3C (cyan line in the revised manuscript). **In the revised manuscript, we have added a bar plot to the right of Fig. 3C that shows the magnitude of each term averaged over the mountain's upwind slope** (more specifically the -100 to -50 km region, where the strong increase in $\Delta\theta_{eB}'$ occurs). This bar plot shows that the apparent source of θ_e (which includes all transient and diabatic processes, including the effect of penetrative downdrafts and turbulent mixing) changes at about the same rate as the background wind U. This implies two things: first, θ_e sources increase with U, meaning that a decrease in precipitation-driven downdrafts either does not occur or is counterbalanced by a change in another process. Second, it means that changes in the sources of θ_e cannot explain the change in $\partial\theta_{eB}'/\partial x$ between simulations. This leaves the change in vertical advection, driven by changes in orographic dynamics, as the only viable explanation for the change in horizontal θ_e gradients, hence in θ_{eB}'.

Minor comments:

1. Abstract, L5-6: this sentence is confusing and misleading. First, it is said that precipitation is "enhanced", but the term "enhancement" requires a reference state for comparison. What is that reference state? This context is necessary in the current study, where the term "enhancement" could refer to (i) orographic enhancement relative to surrounding regions, (ii) enhancement relative to cases with weaker or stronger winds, or (iii) enhancement relative to the current climate. Moreover, the description of the model seems off the mark. It's a model for orographic precipitation, right? So why not describe it as such?

   We apologize for this phrasing being unclear. **The sentence has been modified to clarify that our model describes the seasonal-mean orographic enhancement of precipitation relative to upstream regions.**

2. L29 and L35-36: I agree that decades of research have facilitated progress in orographic "rainfall" (or "precipitation"). But here the authors only refer to studies of tropical orographic precipitation, and only a very small sampling of those. My concern is that the authors preemptively limit the scope of their literature review to a narrow topic. Doing so helps to shorten the discussion, but it also precludes the authors from benefiting from a huge tranche of relevant literature. There are numerous studies of midlatitude orographic precipitation,

ranging from theoretical, to numerical, to observational, many with ideas that clearly transfer to the current study. For example, using idealized simulations, Colle (JAS, 2004) found 20 years ago that stronger winds enhance orographic precipitation by deepening the mountain wave. His result seems quite relevant to the present study. Of course, the goal is not to cite as many studies as possible, but to use the existing literature to help frame and inform the scientific investigation.

Our omission of the midlatitude orographic precipitation literature indeed came from an effort to keep the manuscript concise, given that we believe that the physics of winter-type orographic precipitation in midlatitudes (which have received vastly more attention than summer processes, especially on the theoretical end) are highly distinct from tropical orographic precipitation processes. Yet, we regret that we had not recognized the striking similarity between the conclusion of Colle (2004) and our study. **We have thus added a paragraph discussing studies of winter-type, convectively stable orographic precipitation relevant to this study, and another paragraph reviewing some numerical modeling studies of convective orographic precipitation that addressed its sensitivity to wind speed as part of their analysis.** We thank the reviewer for prompting us to add this content.

3. L72: By “adiabatic”, do you mean dry or moist adiabatic?

   We mean dry adiabatic, and **we have modified the text to include this clarification. At the end of this paragraph, we have also added a sentence justifying this choice** (in response to a comment by the other reviewer).

4. L85-86: “Thus, we expect Pm << Pa”: Where does this inference come from? It is not clear from eqn (3) nor the text of this sentence.

   This conclusion comes from a scale analysis of equation 3. **We have explained this scale analysis in the revised manuscript.**

5. L27: More information on these simulations would be helpful. Do they ever reach a quasi-steady state? How long does it take? If 250 days are required (as the text suggests), that would seem rather excessive.

   We have added a sentence specifying that the simulations are in a quasi-steady state beyond 250 days. We have also specified that it takes around 250 days for the temperature profile to fully equilibrate in the stratosphere.

6. L132-133: Here the authors do cite a midlatitude orographic precipitation study but the physical reasoning is flawed. Yes, hydrometeor drift is significant, but Smith and Barstad (JAS, 2004) were exclusively interested in stratiform precipitation. They were assuming that part of the cause for the downstream shift was slow falling (~1 m/s) ice and snow being carried downwind. This effect is weaker for the convective precipitation studied here, where

fall speeds are closer to 10 m/s. Therefore, I think the "10 km" estimate is overly generous.

We thank the reviewer for pointing this out, and recognize our mistake. **This sentence has been removed from the manuscript.**

7. L134: Does the theory even consider multiple levels? This isn't apparent in equation (1) or the Appendix. Anyway, shouldn't the precipitation amount scale with low-level specific humidity? If not, the theory would have little hope of capturing the sensitivity of orographic precipitation with respect to temperature variations in a changing climate.

   The theory considers averages of temperature and moisture perturbations between 900 hPa and 600 hPa (see first paragraph of section 2); this is the subscript 'L' in equation 1. However, one may consider a convective closure that weights temperature and moisture perturbations nonuniformly in the vertical: this is the approach of Kuang (2010), cited at the very end of section 2. The precipitation amount scales with the vertical gradient of moisture in the lower free troposphere (see equation 2). This gradient steepens at approximately the Clausius-Clapeyron rate with global warming. This is explained in the fourth paragraph of section 5. We have not made any changes to the manuscript in response to this comment.

8. L158: What is meant by "equally"? The two contributions don't seem precisely equal to me, so this is misleading.

   We have changed "equally" by "similarly". We have also added a brief clarifying phrase to this sentence (italicized here): "The increase in θ'_{eB} and the decrease in T'_L *induced by the change in U* contribute similarly to the increase in the P' maximum…"

9. L160-161: I don't follow this reasoning. You just said that qL' has minimal impact but θ_e does, so how can one compensate for the other? And why is there an "absence of free-tropospheric moistening"? Doesn't the mountain wave ascent produce this moistening?

   The reviewer is pointing out differences between the behavior of the theory and that of the simulations; our original sentence was not explaining that clearly. In the theory, q_L' has a large effect on the sensitivity, and θ_{eB}' is not considered. In the simulations, q_L' has a minimal effect on the sensitivity, while θ_{eB}' has a large effect. **We have changed the sentence to remove the word "compensate" and clearly laid out the difference between the behavior of the theory and that of the simulations. We have also added a new section (section 3.4) discussing why q_L' does not increase with U in the simulations.**

10. Equation (4): I don't think it's fair to claim that the \theta_e budget is equal to the highly simplified (4) without stating which simplifying assumptions were made.

    We apologize for taking this shortcut, and **we have laid out the assumptions behind equation (4) in the revised version.**

11. L174: “Little accuracy is lost…”: is this shown somewhere? Given my major comment, I would be interested to see the evidence behind this statement.

The phrasing has been modified in the revised version; but the proof of this statement is contained in the smallness of the residual term, upstream of the ridge top, in Figure 3C. **We have specified this in the revised version.**

12. L175 and L141: Why do you average 4000-2000 km upstream of the mountain here but over a different region (4000-2500 km upstream) on L141? Is there a justification for this inconsistency?

L175 contained a typo; **this has been corrected in the revised version**. Note that using 4000-2000 km or 4000-2500 km as a reference region does not make any difference to the analysis presented in the manuscript.

13. L183: I’m not so sure that a \theta_e change on the order of 0.1 K in can be called a “sharp” increase.

This adjective has been removed in the revised manuscript.

14. L206-207: Do all reanalyses used in this paper really use a “model prior to 1979”? I thought ERA5 used a modern numerical model. Or maybe I am not reading this sentence correctly.

Every reanalysis product uses a numerical model as its backbone; what we meant is that prior to 1979 and the advent of satellite observations, observational constraints were far fewer, and reanalysis data are closer to a model output than an observational product. We did not mean that the model used by ERA5 dates back to 1979, which is incorrect. **We have changed the sentence to clarify this point.**

15. L225: Measured how? Using gauges, as you claimed earlier? Surely the gauges don't align perfectly with the line shown in Fig. 4B. What data set are you using, and how are you analyzing these data? In this section you use a variety of data sets, and the reader needs some help to keep track as you weave through different analyses.

At the beginning of section 4, we explain that the sensitivity of P to U is estimated using a gauge-based precipitation dataset. Details are given in appendix B: we use the APHRODITE dataset, a gridded product of rainfall data derived from a dense network of rain gauges. **In the revised version, we have added to the main text the specification that the product we use is ‘gridded’ (first paragraph of section 4). We have also reminded the reader, in the second paragraph of section 4, that we are using gridded gauge-based data.** The details of the products used are kept in Appendix B to keep the main text concise. The line shown in Fig. 4B (6B in the revised manuscript) outlines a closed region and divides the grid points that it averages over from the ones that are excluded.

16. L229-230: OK, but what about P'? Are you saying here that P' should be a function of P0? That's not what was assumed in the linear model.

The analysis of the sensitivity of P' is treated in the next paragraph. In the linear model and simulations, U is changed while keeping everything else fixed. In the real world, interannual changes in U may be correlated with changes in other variables, including the background stability, moisture content, or P_0. We are showing in Figure S2 that in Malaysia and the Philippines, increased U is correlated with a decrease in the background specific humidity throughout the troposphere, and are speculating that this may drive a decrease in P_0 (although we cannot show this using the APHRODITE dataset, which does not have data over oceans). The goal of the following paragraph is to estimate the sensitivity of P' (allowing us to test the hypothesis of a decrease in P_0 with increased U in Malaysia and the Philippines) using two other precipitation datasets. **We have modified the text to make a clearer transition between the paragraph analyzing the sensitivity of P and the next one, treating the sensitivity of P': we have stressed that we hypothesize a reduction in P_0 associated with increased U in Malaysia in the Philippines, and that we turn to other precipitation datasets to estimate the sensitivity of P', which allows to test this hypothesis.**

17. L259-260: "assuming a 3.5 K warming": measured where? At the surface???

In the SSP5-8.5 scenario, the multi-model mean surface warming in the tropics between 1980-2000 and 2080-2100 is about 3.5K. **We have changed 'warming' into 'surface warming' to clarify this point.**

18. L263-269: Related to this text, another midlatitude study is Kirshbaum and Smith (QJ, 2008), who found < CC scaling w.r.t. temperature changes in simulations designed to test this exact question. Their result and physical explanation is very similar to the one posed by O'Gorman and Schneider (2009) for the climate at large, but with a slightly different mathematical formulation.
We have added a citation of this study, and have cited two other studies tackling the sensitivity of midlatitude orographic precipitation to warming that we deemed equally relevant.

19. L288-291: Another limitation that should be acknowledged is the fact that orographic convection is poorly resolved at Δ= 3 km (Kirshbaum, JAS 2020). The model effective resolution is no better than 20 km, and orographic cells typically have horizontal scales of 1-10 km. Also, for larger temperature deviations one must also consider cloud-microphysical changes (e.g., less ice-phase microphysics) that could offset the projected enhancements in regions experiencing significant warming.

We note two things about the simulations of Kirshbaum (2020). First, their domain size is 30 times smaller than ours in the x direction, 3 times smaller in the y direction, and their mountain width is also 3 times smaller. Moreover, their MECH simulations have no surface fluxes upstream of the mountain. A second set of simulations performed with upstream surface fluxes (thus closer to our setup), termed MECH-FLX, show that the orographic rainfall profile is adequately captured with Δ= 2 km (their simulations with Δ>= 5 km use a convective parameterization, and are thus not comparable to the simulations presented here). We also note that Zhang and Smith (2018) found that resolutions of 2 km vs 6 km made little difference in simulating orographic convection over the Western Ghats.

A study comparing a large number of convection-permitting models (Wing and Singh, 2024, cited in the manuscript) has shown that resolution is not a particularly important factor dictating variations in the behavior of convection between models. Many other factors contribute to differing behavior, including turbulence and microphysical parameterizations. **Thus, we have opted against citing horizontal resolution as a particular limiting factor of our model; there are many others, and these have been collectively acknowledged in our first sentence.** About the reviewer's second point, we note that the simulations presented here do not feature any change in surface temperature.