Review of egusphere-2026-289 "Lake-Mountain synergy in convective clouds and precipitation over the Tibetan Plateau: a large eddy simulation study"

This manuscript presents an interesting large-eddy simulation study of controls on convection over an idealised high-altitude lake-mountain study area representative of the Tibetan Plateau. Sensitivity experiments varying the height of the mountain and removing the mountain or the lake are conducted to disentangle the different interacting processes which control where and how strong the convection is. The approach is a methodical one and there are some strong aspects to the analysis, but I do have some concerns about the model setup / results which need addressing, or at least justifying, in order for the reader to have confidence in the conclusions of the paper.

Reply: We sincerely appreciate the time and effort you have devoted to reviewing our manuscript and providing valuable comments and suggestions, which have helped us improve its quality and clarity. In response to your comments, we have revised the manuscript to provide a more detailed description of the model configuration and experimental design (lines 123-208 in the revised manuscript). We have also strengthened the physical interpretation of the results by further examining the mechanisms governing convective initiation, location, and intensity. In addition, we have expanded the discussion of the limitations associated with the idealized experimental setup and revised several statements in the Conclusions (lines 536-583 in the revised manuscript) to ensure that they are fully supported by the simulations.

All comments have been carefully considered, and our detailed point-by-point responses are provided below.

Major comments:

1) Initial / surface conditions: An initial temperature is given for the lake (284K) and a significantly warmer temperature for the land (300K). The initial near-surface air temperature (from the skew-T diagram in figure 3) is about 283K. This means there will initially be a significant positive surface heat flux from the land. How do the land (and lake) surface temperature and the surface fluxes evolve over time? Is there a diurnal cycle of radiation imposed? If so, what time of day / day of year/ latitude is the model initialised with? Or are the surface temperatures fixed? It is import to be clear about this as the surface fluxes are ultimately driving the convection. If a diurnal *cycle is not being imposed, how realistic is a 20 hour simulation with daytime surface conditions?*

Reply: In our simulations, the lake surface temperature is fixed at 284 K, whereas the land surface temperature is initialized at 300 K and subsequently evolves interactively through the surface energy exchange with the atmosphere. No radiation scheme *is activated, and thus* no explicit diurnal cycle of solar radiation is imposed. The 20 h integration is therefore not intended to represent a realistic diurnal evolution, but rather represents a controlled idealized experiment designed to capture the timescale required for lake-supplied moisture to be transported toward the mountain, interact with terrain-induced lifting, and contribute to the development of precipitation over and downstream of the mountain.

(1) The initial atmospheric thermodynamic profile is based on the 08:00 Beijing Time radiosonde observation at Naqu Station. However, the model initialization time is not intended to represent the actual surface conditions at 08:00. The radiosonde profile is adopted to represent the background thermodynamic structure of the free atmosphere over the TP, while the prescribed initial land-air temperature contrast is designed to mimic the intense surface heating that typically develops during the afternoon, thereby providing the thermal forcing for lake-mountain interactions. Specifically, the land surface is initialized at 300 K, substantially warmer than the near-surface air temperature of approximately 283 K, resulting in a positive sensible heat flux during the early stage of the simulation. After initialization, the land surface temperature is not prescribed but evolves interactively through the surface energy balance calculated by the model surface scheme. In contrast, the lake surface temperature is fixed at 284 K, based on the observed water temperature at a depth of 6 m in Nam Co reported by Wang et al. (2020). This temperature is close to the initial near-surface air temperature. Given the large heat capacity of the lake, assuming a relatively stable lake temperature over the timescale considered here provides a reasonable idealization. The relatively small lake-air temperature difference produces weaker sensible heating over the lake than over land, while evaporation provides a persistent source of atmospheric moisture. This configuration was selected because our primary objective is to directly investigate the afternoon interaction among lake-induced moisture transport, lake-breeze circulation, and terrain-induced lifting, rather than to reproduce the complete diurnal transition from morning to afternoon.

(2) No radiation scheme is activated, and therefore no explicit diurnal cycle of solar radiation is imposed. This choice avoids introducing time-varying radiative forcing and cloud-radiation

feedbacks that could complicate the interpretation of the individual and combined effects of the lake and mountain in the sensitivity experiments. As described above, combining an observed morning sounding with an idealized afternoon-like surface thermal contrast allows us to separate the representative atmospheric background state from the imposed surface forcing. This controlled framework enables us to focus specifically on lake-mountain interactions rather than on the full diurnal evolution of radiative forcing. Similar prescribed or controlled surface thermal forcing has been widely adopted in idealized LES studies of sea- and lake-breeze circulations. For example, Antonelli and Rotunno (2007) imposed a constant surface heat flux over land and explicitly neglected its temporal variation; Crosman and Horel (2012) examined idealized lake- and sea-breeze sensitivity to prescribed land-surface sensible heat flux.

(3) The 20-h integration period should therefore not be interpreted as representing 20 h of realistic daytime heating. Instead, it was selected based on the characteristic timescales of moisture transport and convective development in the model. Sufficient integration time is required for air and moisture originating from the upstream lake region to be transported toward the mountain, interact with terrain-induced lifting, and subsequently contribute to precipitation formation over the mountain and in downstream regions. The first several hours primarily represent the adjustment of the model to the imposed initial thermal contrast. Therefore, our principal analysis focuses on 7-20 h, when the terrain-induced circulation has become well established and the more slowly developing lake-breeze circulation has had sufficient time to influence moisture transport and convective development.

We acknowledge that the absence of a radiative diurnal cycle limits the realism of the simulated timing and persistence of convection, and this limitation has been explicitly discussed in the revised manuscript. Nevertheless, the controlled forcing framework is designed to isolate the thermodynamic and dynamical mechanisms through which lake-induced moisture supply and terrain-induced lifting regulate convective initiation, organization, and precipitation development.

2) Lateral (y-direction) boundary: according to the text, these are period, in which case I would expect to see much more uniform results in the y direction since there is no variation in the topography or land surface in the y direction either. The results however (e.g. figure 5) show a very strong variation in the y direction with almost all the precipitation close to the centre line of the *domain. This seems physically implausible. Assuming the model description is correct this would imply that the limited domain width is having a significant and non-physical impact on the convection. Did you do any sensitivity tests on this? Given these issues with the boundary conditions and lack of uniformity in the y direction, it casts uncertainties on the rest of the results in the paper and so this issue needs to be resolved.*

Reply: The terrain and surface properties are homogeneous in the y-direction, and periodic lateral boundary conditions are applied. However, periodic boundary conditions only ensure continuity of the flow across the lateral boundaries and do not require the convective response to be spatially uniform. In three-dimensional simulations, small initial perturbations can grow under the imposed surface forcing and organize convection through nonlinear interactions among updrafts, cold pools, moisture transport, and cloud-precipitation feedbacks.

Previous studies (e.g., Fu et al., 2021) have shown that sufficiently wide periodic domains can capture the intrinsic three-dimensional organization of convection while reducing artificial constraints from lateral boundaries. To further evaluate the sensitivity to domain width, we performed an additional experiment with a doubled y-direction width (40 km) for the control case (Fig. R1).The comparison shows that the detailed precipitation distribution in the y-direction changes to some extent, but precipitation remains mainly concentrated near the central part of the domain, consistent with the precipitation pattern reported by Gowan et al. (2021). More importantly, the 40km experiment confirms that the primary physical mechanism remains robust: lake-induced moisture supply enhances convective development, while terrain-induced lifting determines the preferred location of precipitation initiation and enhancement.

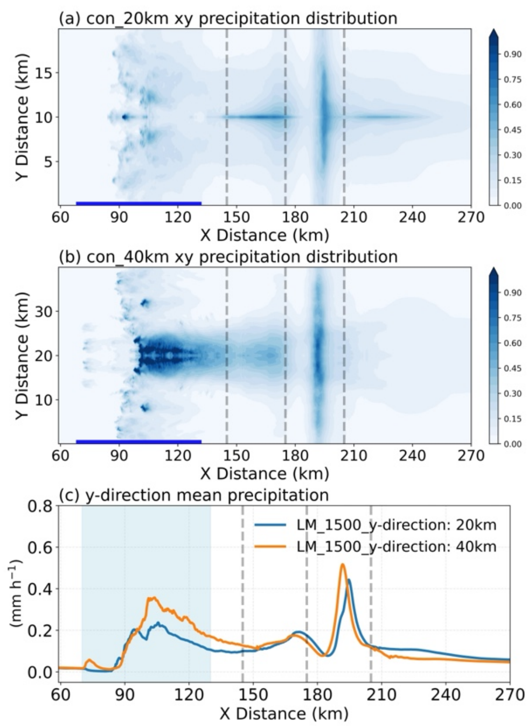

Figure R1. (a-b) Time-mean precipitation (mm h^{-1}) distributions in the x-y plane for y-domain widths of 20 km and 40 km in the y-direction, respectively. (c) Corresponding y-averaged precipitation profiles for the 20 km (blue) and 40 km (orange) domains. The light-blue shading indicates the lake region, and the gray dashed lines denote the locations of the windward slope, mountain crest, and leeward slope.

3) Related to 2). Many of the results are averaged over the central 15km of the domain. Given the boundary conditions are periodic in the y direction there is no reason not to average over the whole width of the domain. However, given the clear non-uniformity in the y direction noted in 2) the choice of what width to average over will affect the results. The choice of 15km seems arbitrary.

Reply: The 15 km central-band average was selected to capture the primary convective region, where the strongest thermodynamic and dynamical responses associated with lake-mountain interactions develop. Averaging over this region facilitates a clearer identification of the characteristic structures associated with these interactions, including perturbations in potential temperature, water vapor mixing ratio, and vertical motion.

To assess the sensitivity of the results to the averaging range, we repeated the relevant diagnostics using averages over the entire y-domain and compared them with the original central-band averages (Fig. R2). The comparison shows that the precipitation magnitude changes slightly because the full-domain average includes areas with weaker or more intermittent convective activity.

Nevertheless, the spatial distribution, timing, and major characteristics of the thermodynamic and dynamical responses remain consistent between the two averaging approaches. Therefore, the main conclusions regarding the roles of lake-induced moisture supply and terrain-induced lifting are robust and insensitive to the choice of averaging range.

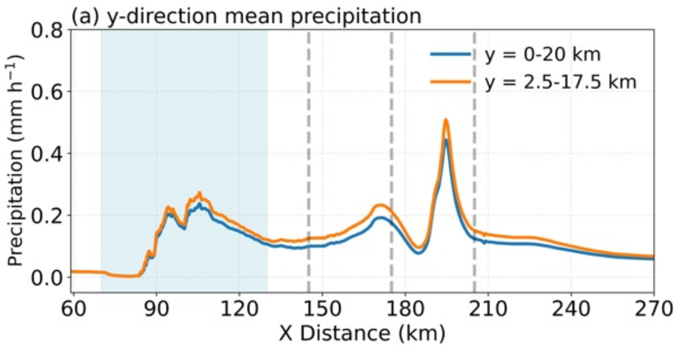

Figure R2. (a) The y-averaged precipitation profiles over the full domain (y = 0-20 km, blue curve) and the central region (y = 2.5-17.5 km, orange curve). The light-blue shading indicates the lake region, while the gray dashed lines mark the locations of the windward slope, mountain crest, and leeward slope.

4) Choice of averaging time. Many figures show results averaged over hours 7-20 of the simulation. While I appreciate the need to average LES results to produce the mean flow, this is quite a long period to average over. The flow is definitely not statistically stationary over this period (as can be seen from the time series, e.g. figure 4. Why this choice of averaging window? And what are the implications of this on the results?

Reply: The simulations are initialized from a cold start, and different components of the system develop on different timescales. Following initialization, the mountain-induced circulation responds relatively rapidly to the imposed mechanical and thermal forcing, whereas the lake-induced circulation and associated moisture transport require a longer adjustment period to develop. During the first several hours, the simulated evolution is therefore strongly influenced by initial adjustment processes and by the largely independent development of the lake- and terrain-induced responses. By approximately 7 h, lake-derived moisture has reached the mountain region, and the combined effects of lake moisture supply and terrain-induced lifting have become well established.

Accordingly, the 7-20 h period was selected to characterize the mean response associated with the coupled effects of lake moisture supply and terrain-induced lifting on cloud development, hydrometeor evolution, and precipitation distribution. Although convection continues to evolve during this period, applying the same averaging window to all experiments provides a consistent basis for comparing how modifications to the lake and terrain affect convective development. The resulting fields should therefore be interpreted as time-averaged responses during the established lake-mountain interaction stage, rather than as representations of a statistically stationary state.

5) Averaging CAPE / CIN over pre / during and post convection. CAPE and CIN are primarily used to diagnose conditions conducive to convection and so I am not sure it makes sense to average these values over the whole 7-20 hours of the simulation which includes the convection (and post convection). To understand where and when convection occurs you need to study CAPE and CIN values before the convection initiates.

Reply: The purpose of the averaging used in this study is not to diagnose only the convection triggering conditions, but rather to characterize the overall thermodynamic evolution during the mature lake-mountain convection process. In our simulations, convection is initiated and maintained through different mechanisms during the integration period. In particular, mountain-induced lifting can trigger convection at an earlier stage, whereas lake breeze circulation requires additional time to develop under sustained surface heating and contributes to later convection enhancement. Therefore, the 7-20 h period was selected to include the evolution of both mountain-driven and lake-driven convective processes.

To clarify this point, we have revised the manuscript to state that the time averaged CAPE and CIN values are used to represent the thermodynamic environment during the active convection period, rather than solely the conditions before convection initiation.

6) Why the choice of a sheared background wind profile? This does give low wind speeds at low levels, but becomes much stronger higher up. The results of the OL experiment suggest that this background wind dominates over any thermally induces lake-land breezes, while the OM experiment suggests that wave trapping/ wave breaking is a key features of this setup which is again controlled by the background wind field alongside the stratification. In particular, a shear wind profile with wind speed increasing with height will promote wave trapping since the Scorer parameter decreases with height. This has a significant impact on the results over the mountain. Maybe this is realistic for the TP, but you should be clear about the rationale for the choice and the implications of that *choice.*

Reply: The prescribed background wind profile increases linearly with height, with relatively weak low-level winds and stronger winds aloft. This profile was designed to represent the typical vertical structure of the environmental flow over the TP rather than to reproduce a specific observed event.

The weak low-level winds are important because they allow thermally driven lake-land circulations to develop and enable lake evaporation to modify the near-surface thermodynamic environment. Meanwhile, the stronger winds aloft facilitate the downstream transport of lake-supplied moisture toward the mountain region, where terrain-induced lifting can further promote cloud development and precipitation formation. This configuration is broadly consistent with previous idealized studies of lake-mountain convection over the TP, which have emphasized the importance of interactions among the background flow, lake-breeze circulation, and topography in regulating moisture transport and convective initiation (Gerken et al., 2014).

We acknowledge that a vertically sheared wind profile can also influence mountain-wave propagation, wave trapping, and wave breaking through changes in the Scorer parameter. In our experiments, these dynamical effects are therefore not excluded but represent part of the mountain response under a realistic TP background environment. However, the objective of this study is not to isolate mountain-wave dynamics alone, but to quantify how lake-induced moisture supply interacts with terrain lifting under weak synoptic forcing. Since the same environmental wind profile is applied in all sensitivity experiments, the comparison primarily reflects the impacts of lake presence and terrain on convective development.

7) Section 2.4.1. I'm not sure this section is correct. If perturbations are from a horizontal average, then what do you do over the mountain where the horizontal intersects the terrain? Looking at later figures of e.g. θ', they can't be perturbations about a horizontal average as at a given level the values are all positive or negative (and by definition the horizontal average of a perturbation has to be zero). Is it perhaps perturbations from the background (upstream) profile or the initial profile? Please check and correct.

Reply: The original description in Sect. 2.4.1 was incorrect and could lead to the interpretation that the perturbations were defined relative to a horizontal mean. No horizontal averaging is used in the decomposition. Instead, each variable is decomposed point by point into a four-dimensional base component and a perturbation component, both of which retain their dependence on time, height, and the x- and y-directions.

$$\phi(t,z,x,y) = \phi_{base}(t,z,x,y) + \phi_{turb}(t,z,x,y)$$

Consequently, the perturbation field is not required to have a zero horizontal mean, and no special treatment is needed at levels that intersect the terrain. The averaging in the periodic y-direction is applied only after the decomposition when producing the two-dimensional cross-sections shown in the later figures. We have revised Sect. 2.4.1 and Eq. (2) and removed the misleading references to horizontal and phase averaging.

8) Explanation of cold air pooling, starting at line 475. Blocking can trap (potentially) cold air at low levels, but it cannot generate cooling. This must be through some diabatic process. It is not clear what the surface heat fluxes are (see comment 1 above), but it seems unlike that that is the cause of the cooling since it is not seen in the OL simulations. More likely is that the cooling is a result of evaporation / sublimation – possibly of precipitation since there is much less cooling in the OL simulation which contains less precipitations. Some further analysis is needed to properly determine the cause of this cooling.

Reply: The terrain blocking itself cannot generate cooling; instead, it primarily influences the transport and accumulation of near-surface cold air. Therefore, we have revised our description to distinguish the source and maintenance mechanisms of the cold pool. Additional analysis of hydrometeor and precipitation distributions suggests that the cold pool is mainly generated by precipitation-related diabatic cooling.

In the lake-mountain experiments, enhanced moisture transported from the lake increases cloud and hydrometeor development over the mountain region. As shown in Fig. 7a-c, the LM experiments, particularly LM_3000, exhibit substantially enhanced hydrometeor concentrations over and upstream of the mountain compared with the OL experiment (Fig. 7d). Correspondingly, stronger precipitation occurs over the mountain region in LM_3000, whereas precipitation is much weaker in OL (Fig. 6a-e). These results indicate that the enhanced lake moisture supply and terrain lifting promote precipitation formation, providing favorable conditions for evaporative cooling and ice-phase sublimation/melting cooling.

The resulting diabatic cooling produces negative near-surface temperature perturbations and contributes to the formation of the cold pool, as shown by the low-level cooling region in Fig. 10b. However, the terrain does not act as the cooling source. Instead, the mountain-induced blocking reduces the horizontal ventilation of the cooled air, allowing the cold air to accumulate and persist near the windward slope. This interpretation is also supported by the weaker cold pool in the OL simulation, where precipitation and hydrometeor development over the mountain are substantially reduced (Fig. 6d and Fig. 7d). Thus, our results suggest that precipitation-related diabatic cooling primarily generates the cold pool, whereas terrain blocking favors its accumulation and persistence.

9) You talk about how the moistening affect Nm, but not how the changes in potential temperature affect the stability. Maybe this is less important since the changes (probably) happen after the convection and precipitation has occurred, but I can't be sure from the plots in the manuscript.

Reply: We agree that the original discussion placed too much emphasis on the moisture contribution to N_m and did not sufficiently explain the role of thermal stratification. In addition to the moisture contribution, N_m is determined by both the thermal stratification term and the vertical distribution of water substances. Specifically, the vertical gradient of virtual potential temperature ($\frac{dT}{dz}$) controls the static stability of the atmosphere. Enhanced surface heating increases low-level potential temperature and can reduce the near-surface vertical θ gradient, thereby weakening static stability during the convective development stage. Conversely, stronger stratification increases N_m and suppresses vertical motion.

$$N_m = \sqrt{\frac{g}{T}\left(\frac{dT}{dz} + \Gamma_m\right)\left(1 + \frac{Lq_s}{RT}\right) - \frac{g}{1 + q_w}\frac{dq_w}{dz}}$$

In our simulations, all experiments use the same initial atmospheric thermodynamic profile and background conditions. Therefore, the differences in potential temperature among experiments mainly arise from the different surface forcings associated with lake presence and terrain configuration. These thermodynamic differences are relatively limited compared with the changes in moisture distribution caused by evaporation from the lake and precipitation. The warmer land surface produces positive sensible heat fluxes and contributes to low-level destabilization during the convective development stage. Meanwhile, the evaporation of precipitation and mixed-phase processes can further modify the thermal structure after convection initiation. Therefore, the simulated N_m variations reflect the combined effects of thermal stratification, moisture availability, and the vertical distribution of total water, with the moisture-related contribution playing an important role in distinguishing the different lake-mountain configurations.

10) Figure 11 shows a vertical cross section of moist Froude number. The Froude number is useful defined as a bulk measure across some layer (i.e. over the height of the mountain), as in equation 8. How do you define a Froude number at a particular height? And why is that useful? If you want a local measure of the profile stability then something like a Richardson number would be more usual.

Reply: (1) The Fig. 11 is calculated locally as:

$$\mathrm{Fr_{m,loc}}(z, x) = \frac{u(x, z)}{N_m(x, z)H_0}$$

where u(x, z) and N_m(x, z) retain their spatial and vertical variations, while H_0 is the prescribed mountain height. Because H_0 is constant, the vertical and horizontal variations of $\mathrm{Fr_{m,loc}}$ mainly reflect changes in flow speed and moist stability. This local diagnostic allows us to identify regions where the background flow and moist stability are favorable or unfavorable for moisture transport and convective development, and to compare these features with the thermodynamic and hydrometeor structures.

Previous studies have demonstrated that the interaction between moist stability and terrain-induced flow plays an important role in controlling orographic convection and precipitation (Durran and Klemp, 1982; Jiang, 2003; Kirshbaum and Durran, 2004). In these studies, spatial variations of environmental flow and stability are commonly examined to interpret the dynamical regime over complex terrain. Following this concept, we calculate a local moist Froude number in the x-z band, where the wind speed and moist Brunt-Väisälä frequency retain their spatial variations.

(2) We agree that the Richardson number is widely used as a local measure of the balance between stratification and vertical wind shear. However, diagnosing shear instability is not the primary objective of the present analysis. Instead, our focus is on how moist atmospheric stability modulates terrain-induced flow and the associated development of clouds and precipitation within the lake-mountain system. The moist Brunt-Väisälä frequency is particularly relevant for this purpose because it directly characterizes the buoyancy response of saturated air. Moreover, in our

formulation, it accounts for the effects of mixed-phase processes, which are particularly important in the cold, high-altitude environment of the TP. Therefore, the moist Froude number provides a more physically relevant diagnostic for the specific dynamical processes examined in this study than a Richardson-number-based measure of shear instability.

Minor comments

1) *Line 62. "can profound influences" -> "can have profound influences"*

Reply: Thank you for pointing this out. We have corrected "can profound influences" to "can have profound influences" in the revised manuscript.

2) *Fig 1. The blue line floating about the ground to mark the lake is a bit confusing. I would suggest positioning this at the surface. Just ensure you make it thick enough that it stands out from the grey of the land surface.*

Reply: In the revised Fig. 1, the blue line indicating the lake has been repositioned at the surface and made thicker to improve its visibility. (Line 149)

3) *Figure 2 caption. "The different sizes of the lake are marked." This seems a mistake. As far as I can see only one size of lake is used.*

Reply: The statement "The different sizes of the lake are marked" was inaccurate, as only one lake size is used in the present experiments. We have corrected the caption of Fig. 2 to avoid this confusion. (Line 155)

4) *Figure 2. Why is the red dashed box bigger than the domain? Results are presumably only reported within the domain. Also mark on the distance upstream of the lake where the box starts (assuming that not all the domain is used).*

Reply: The red dashed box in the original Fig. 2 was intended only to schematically indicate the analysis region. However, its extent was not drawn accurately, which could have given the misleading impression that the analysis region extended beyond the model domain. In the revised Fig. 2, we have redrawn the box so that it lies entirely within the model domain. We have also indicated the upstream distance from the lake to the starting location of the analysis region to clarify the portion of the domain used for the reported diagnostics.

5) Line 172. "Where" -> "where" (this is not the start of a sentence)

***Reply:** Corrected as suggested.*

6) Line 201. Is this horizontal average in the x as well as the y direction? If so, how do you deal with terrain?

Reply: The wording "horizontal average" in the original manuscript was misleading. The decomposition used here is not an average over both the x and y directions. Instead, each variable is decomposed into a base component and a turbulent component, both of which retain the full four-dimensional model structure variable (time, level, x, y). Therefore, no horizontal averaging across terrain is involved in the definition of the decomposition, and no special treatment of terrain-following or below-ground grid points is required for an x-y averaged field. We have revised the text to clarify this distinction. (Line 210-216)

7) Line 201. I think you have θ' and θ the wrong way round here (<θ> is the average of θ and θ' is the perturbation about this average).

Reply: The original wording incorrectly described the relationship between the mean and perturbation quantities. In the revised manuscript, we revised the explanation of Eq. (2) by defining the decomposition as $\phi(t,z,x,y) = \phi_{base}(t,z,x,y) + \phi_{turb}(t,z,x,y)$, *where* $\phi_{base}(t,z,x,y)$ is the base component and $\phi_{turb}(t,z,x,y)$ is the turbulent fluctuation relative to this base component.

8) Line 210. "Where" -> "where" (this is not the start of a sentence)

Reply: Corrected as suggested.

9) Line 225. "Where" -> "where" (this is not the start of a sentence)

Reply: Corrected as suggested.

10) Line 225. Here I assume u is averaged over the layer 0-H0 (and the same for N)?

Reply: *u* and N_m are not vertically averaged over the H_0 layer. Both variables retain their vertical and x-direction dependence, with dimensions of *(z, x)*. The moist Froude number is therefore calculated at each vertical level and x-location using the corresponding values of $u(z, x)$ and $N_m(z, x)$, together with the prescribed mountain height H_0.

11) Line 230. "Where" -> "where" (this is not the start of a sentence)

Reply: Corrected as suggested.

12) Line 320. "precipitation appears as a narrow in the leeward, terrain-locked band with limited downstream growth". This phrase is quite hard to understand. How about "precipitation appears as a narrow band locked to the terrain over the lee slope with limited downstream growth"

Reply: We agree that the original phrase was unclear, and we have modified the sentence in the revised manuscript. (Line 298-302)

13) Figure 5. Missing label on y-axis. I assume this is distance in the y direction?

Reply: We have added the missing y-axis label in the revised Fig. 5. The y-axis now indicates the distance in the y-direction. (Line 310)

14) Figure 5 caption. "Time mean precipitation rate of 13h". What does this mean? Do you mean the time mean precipitation rate between 7 and 20 hours?

Reply: The phrase "time-mean precipitation rate of 13-h" refers to the precipitation rate averaged over the 13-h period from 7 to 20 h. We have revised the caption to make this time interval explicit.

15) Figure 5 caption. The caption talks about difference plots (c), (e), (g) and (i) but these are not shown in the figure. Please update the caption to correctly describe the plots.

Reply: We have revised the caption of Fig. 5 so that it correctly describes only the panels shown in the figure. (Line 311-313)

16) Line 335. "To diagnose the convective supply, instability" -> "To diagnose the convective supply and instability"

Reply: Corrected as suggested.

17) Line 357. "as mountain height increases, CAPE over the lake region increases". I think this should be "CAPE over the lake decreases" from looking at figure 6? I don't think the trends in CAPE and CIN are really clear. It depends a bit where over the lake you are looking. You might want to explain this logic more thoroughly.

Reply: After reexamining Figs. 6b, 6d, and 6f, we agree that the original statement that "CAPE over the lake region increases with increasing mountain height" was incorrect. This was a misinterpretation of the CAPE distribution, and we have revised the corresponding description in the manuscript accordingly. (Line 337-339)

18) Line 358. "exhibits the opposite tendency" is ambiguous as you are talking about two different tendencies in the previous line. I would be explicit and say "whereas precipitation over the lake increases."

Reply: We agree that the phrase "exhibits the opposite tendency" could be unclear. We now state explicitly that, as mountain height increases, precipitation becomes more concentrated over the lake and windward slope, while its downstream extension is reduced.

19) Line 395 "in the absence of lake" -> "in the absence of the lake"

Reply: Corrected as suggested.

20) Line 396 "deep convection occurs when moisture supports with suitable orographic lifting" does not make sense. Do you mean something like "deep convection occurs when moisture coincides with suitable orographic lifting"

Reply: We have revised the sentence to clarify that deep convection occurs when sufficient moisture coincides with suitable orographic lifting.

21) Line 408. "Across the involving lake configurations" -> "Across the configurations with a lake present"

Reply: Corrected as suggested.

22) Figure 8. I assume the y axis here is height above ground level? Why not plot over the terrain as you do in other figures? At least you need to be explicit what you are plotting.

Reply: We have revised the figure caption and axis label to explicitly state that the vertical coordinate in Fig. 8 is height above ground level. Figure 8 is different from the other *x-z* cross-section figures (rather than the cross section along *x*-direction) because it shows time-height profiles of hydrometeor mixing ratios averaged over three subregions: the lake, mountain, and downstream areas.

23) Line 444. "results in the precipitation formation is inhabited" -> "results in the inhibition of precipitation formation" or "results in the suppression of precipitation formation"?

Reply: Corrected as suggested.

24) Line 450 "using an open water configuration comparable to OM". I think you mean OL not OM? There is no lake in OM.

Reply: Corrected as suggested.

25) Line 479. "hydrometeor of condensate growth and transport" does not make sense. Maybe just "hydrometeor growth and transport"?

Reply: Corrected as suggested. We have revised the sentence for clarity.

26) Line 481. "enhanced hydrometeor" -> "enhanced hydrometeor mixing ratio"

Reply: Corrected as suggested.

27) Figure 10. Include a reference wind arrow on the plot to show wind speed?

Reply: We have added a reference wind arrow to Fig. 10 and clarified this information in the revised caption.

28) Line 502. "Nm. and" -> "Nm, and"

Reply: Corrected as suggested.

29) *Figure 11 caption. "most Froude number" -> "moist Froude number"*

Reply: Corrected as suggested.

30) *Figure 11 caption. "As in (a)" -> "as in (a)" - it is not a new sentence.*

Reply: Corrected as suggested.

31) *Line 536. "most trajectories" -> "Most trajectories" - start of a new sentence.*

Reply: Corrected as suggested.

32) *Line 538. "lower than in the involving lake experiments" -> "lower than in the experiments involving the lake"*

Reply: Corrected as suggested.

33) *Figure 12. Is the colour here water vapor mixing ratio? If so, why is it not conserved along a trajectory? Is this due to condensation and cloud formation?*

Reply: The color in Fig. 12 represents the local water vapor mixing ratio interpolated from the Eulerian model output along the calculated trajectories. The trajectory calculation only provides the pathways of air motion based on the model wind field and does not track the evolution of water vapor contained within an individual air parcel. Therefore, the water vapor mixing ratio shown along the trajectories is not expected to be conserved. Its variation reflects the spatial and temporal variability of the Eulerian water vapor field encountered by the moving trajectories, which can be influenced by moisture transport, surface evaporation, condensation, and mixing processes.

The purpose of Fig. 12 is to diagnose the transport pathways and vertical lifting of near-surface moist air from the lake region toward the mountain and downstream areas, rather than to construct a closed moisture budget. To avoid confusion, we have revised the caption and related text to clarify that the trajectories are colored by local water vapor mixing ratio rather than representing water vapor tracer particles.

34) *Figure 12 caption. "in experiments with lake presence" is not true – you also show results for*

OM with no lake. Just remove this phrase?

Reply: Removed as suggested.

35) *Line 559-561. This sentence is repeated. Just delete the second occurrence.*

Reply: Deleted as suggested.

Reference:

Antonelli, M. and Rotunno, R.: Large-eddy simulation of the onset of the sea breeze, J. Atmos. Sci., 64, 4445–4457, https://doi.org/10.1175/2007JAS2261.1, 2007.

Crosman, E. T. and Horel, J. D.: Idealized large-eddy simulations of sea and lake breezes: Sensitivity to lake diameter, heat flux and stability, Bound.-Layer Meteorol., 144, 309–328, https://doi.org/10.1007/s10546-012-9721-x, 2012.

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