

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

The revised manuscript is considerably improved following the comments of both reviewers. In particular, the authors have rerun the simulations to randomly seed the initial conditions with noise, largely avoiding the unphysical symmetry seen in the original simulations. I still have a couple of minor points for clarification.

1) Section 2.4.1. I don't think (as written) your decomposition of the flow into a base state and turbulent fluctuations makes sense. The base state must be averaged over some time or spatially to allow you to identify fluctuations about that base state, but you have written the base state as a function of t , x , y and z . How does this work? Is this a mistake, or is there some local scale spatial / temporal filtering going on, in which case you need to describe the filtering?

Reply: We are sorry for the misleading description. The fields used in our analysis are not obtained through a Reynolds decomposition without temporal and spatial filtering. We use the base-state and perturbation variables output by CM1. In CM1, the base-state zonal wind (u_0) and its perturbation (u_{pert}) are provided as separate output variables. Therefore, we use u_{pert} directly, rather than deriving a fluctuation from a temporally or spatially averaged wind field.

We have replaced “turbulent component” and “turbulent fluctuation” with “perturbation”, which are obtained from the CM1 output rather than derived from temporal or spatial averages.

2) Moist Froude number. I appreciate the clarification that in the moist Froude number U and N_m are calculated locally, but that H_0 is a constant. I am not sure that it makes sense to calculate a Froude number with a constant height, but spatially (horizontally and vertically) varying wind speed and stability. Really, this is just looking at the relative local importance of wind speed vs stability.

Reply: With H_0 fixed, the spatial variation of this quantity is controlled by u and N_m , with the local balance between cross-mountain flow strength and moist stability. We have therefore renamed it with the moist flow–stability index, (I_{mfs}).

I_{mfs} represents the local cross-mountain flow strength relative to the constraint of mixed-phase moist stability. Because H_0 is fixed, its spatial variability comes from changes in u and N_m . This local balance influences how readily air can be displaced vertically over the lake-mountain system. Higher

I_{mfs} values indicate weaker stability constraints and conditions more favorable for sustained ascent and vertical cloud development, whereas lower values indicate stronger resistance to vertical displacement. Therefore, I_{mfs} is used to describe spatial variations in the relative roles of cross-mountain flow and moist stability and their influence on vertical displacement and cloud development.

3) Fig 5, panel (b). The LM_3000 simulation still shows a very localised patch of rainfall over the summit of the ridge near the centre of the domain. Is this correct? It still seems a bit odd. Can you explain this?

Reply: We have examined the localized precipitation maximum in LM_3000 and confirmed its presence in the revised simulation. To determine whether this feature represents a persistent structure anchored near the domain center, we analyzed the temporal evolution of precipitation along the ridge crest (Fig. R1).

The analysis reveals that the precipitation maximum near $y = 10\text{--}12$ km is primarily associated with a relatively strong and long-lived convective event occurring between approximately 7 and 10 h. After this period, precipitation develops at various locations along the ridge, and the most intense cells undergo substantial shifts in the y -direction. Hence, the localized maximum in the time-mean precipitation field does not correspond to a stationary feature fixed at the domain center; rather, it largely reflects the contribution of a single stronger convective episode to the time average. The 3000 m ridge provides strong orographic lifting near the crest, while the imposed random perturbations enable three-dimensional convective development along the ridge. Consequently, the localized precipitation maximum in Fig. 5b arises from the combined effects of strong terrain-induced ascent and intermittent three-dimensional convective development.

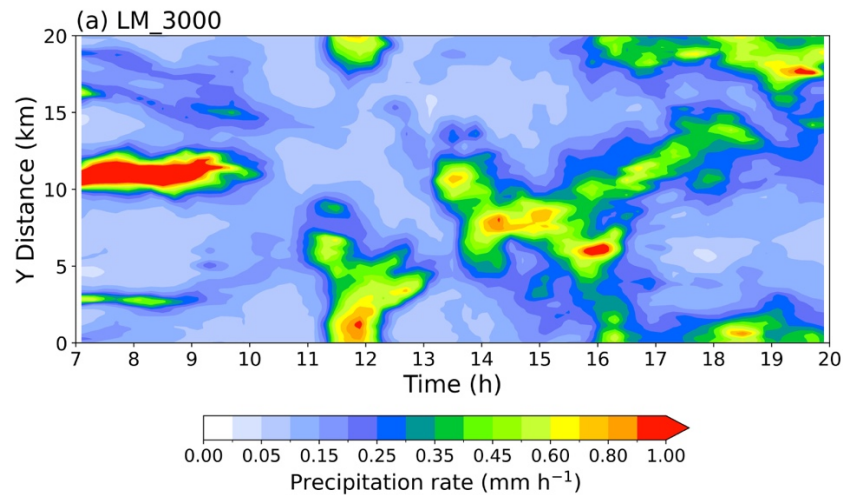


Figure R1. Temporal evolution of precipitation rate along the y -direction over the ridge-crest region (165–185 km) in the LM_3000 experiment. A strong precipitation area develops near $y = 10$ – 12 km during 7 and 10 h, whereas precipitation occurs at different locations along the ridge at later times. This early event contributes substantially to the localized maximum in the time-mean precipitation field.

4) Line 469-470. “precipitation modify the local thermodynamic structure and weaken the warm potential temperature anomaly” -> “precipitation modifies the local thermodynamic structure and weakens the warm potential temperature anomaly”

Reply: We have corrected the grammatical error as suggested.

5) Line 255. “in Supplement” -> “in the Supplement”

Reply: Done.

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

I thank the authors for their responses to my comments. The manuscript is significantly improved. Specific comments are listed below:

2. Hydrometeor mass growth and loss (Figure 9)

Thank you to the authors for examining the calculation of Q_{ten} . The revised equation description and the author’s explanation make sense. In the new description from L. 225 to 233, though, there appears to be a mistake in the ordering of the equations that disrupts the flow of the text. The text reads “The total hydrometeor mixing ratio is given by:”, and then two equations are given. Eq. 5 appears out of place here. The authors should revise this text to correctly place Eq. 5 and the sentence describing it on L. 230.

Thank you to the authors for including Fig. R3. To my eye, the lines in Fig. 9a,b were very symmetrical, but Fig. R3 proves that this is not the case.

Reply: We agree that Eq. (5) was inadvertently placed after Eq. (4), disrupting the logical flow of the description. We have revised this section accordingly: Eq. (4) is followed by the definitions of the individual hydrometeor components, while Eq. (5) is presented separately to define the tendency of the total hydrometeor mixing ratio. This has been corrected in the revised manuscript. (Lines 226-231)

6. Supplemental Material

Thank you to the authors for addressing my comments on the supplemental material. In their response, the authors state “We have added this interpretation to the manuscript” when referring to the NLM simulation. I believe that is the line of text at L. 320-321 that reads “In the absence of strong... difficult to initiate (Fig. S3)”. I wonder if NLM should be explicitly mentioned here, so that the reader understands what Fig. S3 refers to and it makes it clearer that they have an NLM simulation. This would also warrant adding NLM back to Table 1. Ultimately, it is the authors’ choice about how to treat NLM in the manuscript, but I think the results are interesting as a comparison to the lake and/or mountain simulations.

Reply: We appreciate this suggestion and agree that the NLM experiment provides a useful reference for evaluating the effects of the lake and mountain forcing. However, because NLM is used only as a supplementary reference case and is not included in the main comparative analyses, we prefer to retain it

in the Supplementary Material rather than add it back to Table 1. To make its role clearer, we have revised the corresponding text in the main manuscript to explicitly identify the NLM experiment when referring to Fig. S3. (Lines 325-328)

8. Grammar

The grammar in this manuscript is greatly improved, but there are still several grammatical errors that the authors should address to improve readability. Here are a few examples:

L. 423-424 - “... proving the near-surface moistening promote convection initiation.” This should say “proving that near-surface moistening promotes convection initiation”.

L. 93-95 – “... regional moisture budget (Wang et al., 2019, 2020a), providing the basic conditions for the cloud-precipitation process.” This should say “regional moisture budget (Wang et al., 2019, 2020a) and providing the basic conditions for the cloud-precipitation process.”

Reply: We have corrected these errors.

L. 389 - : “Across the configurations with a lake configurations” – this appears to be a typo. I’m not entirely sure what the author’s are trying to say.

Reply: Reply: We meant that earlier hydrometeor formation over the lake is a common feature of all experiments that include a lake. We have revised the sentence accordingly.

Use of the term “plains”

Perhaps this is something that I did not notice in my first read of the manuscript, but the authors frequently refer to “the plains” as a region that they compare to, without ever introducing what this means. I assume that this is referring to the Great Plains region of the United States, but maybe I’m mistaken. The authors should say what they are referring to when they mention “the plains” upon first use in the manuscript. Also, if they are referring to the Great Plains, is that the best comparison? There is no lake effect precipitation in the Great Plains. Perhaps the authors are instead thinking of the Great Lakes region, which they refer to in L. 83.

Reply: In this study, “the plains” was not intended to denote the Great Plains or the Great Lakes region of the United States specifically. Rather, it was employed generically to refer to low-elevation regions in contrast to the high-elevation Tibetan Plateau. Consequently, we have amended the manuscript,

replacing “the plains” with “low-elevation regions”.