

Response to Reviewer #1:

Thank you for your professional review and instructive comments. We have revised the paper carefully based on your comments. These are described as follows (*italic text in blue color is from your review*).

This study evaluates the performance of a single-column model using three different convection schemes (ZM, TDK, UNICON) and their modified versions in representing the diurnal cycles of precipitation (DCP) at three typical locations in China. The manuscript is well motivated, as clarifying the roles of trigger and closure treatments can help deepen our understanding of the physical mechanisms underlying the DCP. However, the current version has several issues that prevent it from achieving its full potential, as detailed below. I therefore recommend a major revision.

Major comments

1. My primary concern relates to the experimental design: each sensitivity experiment changes two factors simultaneously. For example, the CAM_ZM sensitivity experiments include dCAPE + elevated convection, and the CAM_UNICON sensitivity experiment includes cold-pool feedback + elevated convection (Table 1). The authors attribute the improvement in CAM_ZM_SENS to the dCAPE trigger (lines 490–491) and that in CAM_UNICON_SENS to cold-pool-driven mesoscale flow, but this attribution is not supported by separate experiments. Similarly, the warm bias seen in CAM_UNICON_SENS cannot currently be ascribed unambiguously to the cold-pool feedback, to elevated convection, or to their combination. I suggest that the authors perform isolated experiments (e.g., dCAPE-only and elevated-convection-only tests). If additional runs are not feasible, the confounding issue must be explicitly discussed and the corresponding claims appropriately softened.

We thank the reviewer for this important comment. We acknowledge that each sensitivity experiment modifies two factors simultaneously, which complicates a clean attribution of the improvements. To address this, we have performed additional isolated experiments and carefully examined the individual contributions.

In the ZM and UNICON schemes, the implementation of elevated convection has no impact on afternoon convection, because the convective launching level remains near the surface even though the implementation of elevated convection allows parcels to be launched above the PBL. However, the Mb scaling in ZM that primarily applies to afternoon convection also affects nocturnal convection. Figure R1 compares the results for SCB among CTRL, the combined experiment, the trigger-only experiment, and the elevated-convection-only experiment. The trigger-only experiment behaves similarly to CTRL and does not improve nocturnal precipitation but weakens the precipitation (purple curve in Fig. R1a, b). In contrast, the elevated-convection-only experiment captures the nocturnal precipitation peak, but meanwhile overestimates daytime precipitation. Only the combined experiment, which includes the additional dCAPE trigger, reproduces both the nocturnal precipitation peak and the daytime precipitation minimum well. As shown by the Q_1 and Q_2 profiles in Figure R2a and the temperature and moisture biases in Figure R3, the dCAPE trigger helps mitigate the late-afternoon warm and dry biases in CTRL, with active nocturnal convection over SCB further reducing them.

For UNICON, we conducted a similar set of experiments but did not isolate the elevated-convection-only case, since elevated convection is intrinsically coupled to the cold-pool-driven mesoscale flow in this scheme. The cold-pool feedback alone substantially enhances convective precipitation relative to CTRL (Fig. R1d). However, total nocturnal precipitation is slightly reduced compared with CTRL (Fig. R1c). The improvement in nocturnal precipitation is achieved only when elevated convection is treated. The Q_1 and Q_2 profiles (Fig. R2b), as well the temperature and moisture biases in Fig. R4 all indicate that the warm bias seen in CAM_UNICON_SENS is due to the cold-pool feedback.

We have added a discussion of the confounding factors (L257-260; L320-336; L486-488; L491-493) and included the isolated experiments as supplementary figures (Figure S1). We thank the reviewer for prompting this clarification, which has substantially improved the robustness of our conclusions.

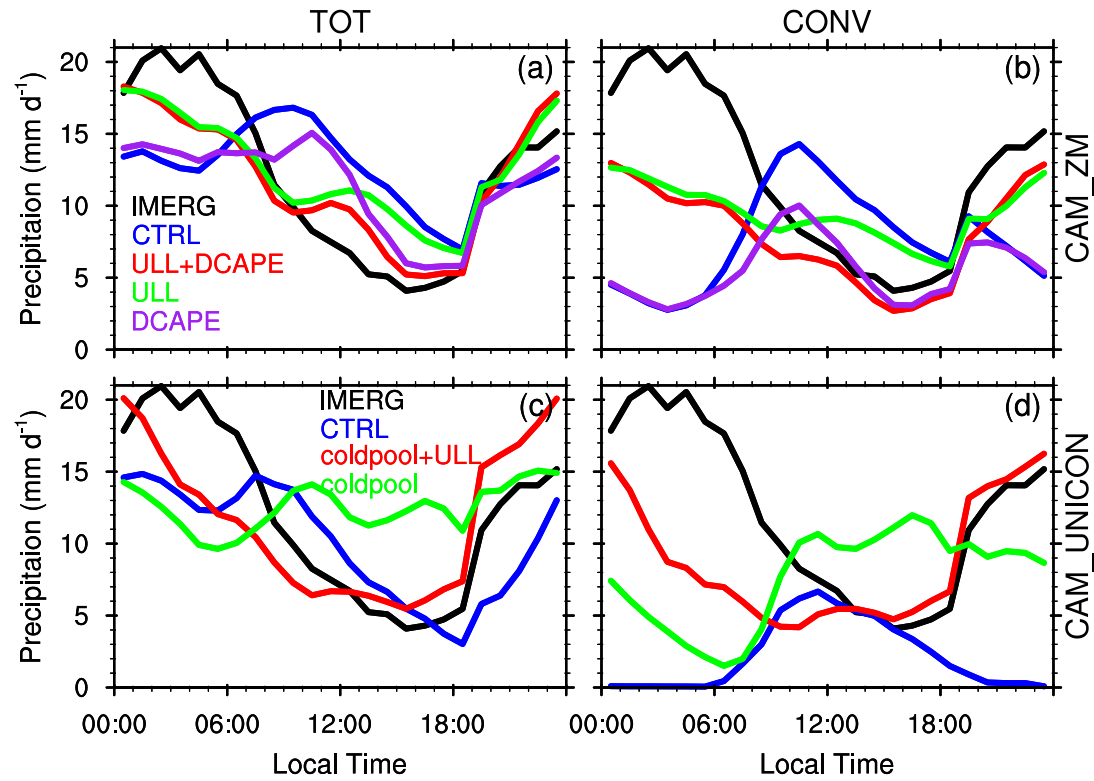


Fig R1. Diurnal cycles of total (a, c) and convective (b, d) precipitation over SCB for the control and sensitivity runs of each convection scheme. From top to bottom, the panels show the DCP simulated with CAM_ZM and CAM_UNICON, respectively. In CAM_ZM, the blue curve denotes the control experiment; the red curve denotes the sensitivity experiment with both the dCAPE trigger and elevated convection; the green curve denotes the experiment with elevated convection only; and the purple curve denotes the experiment with the dCAPE trigger only. In CAM_UNICON, the blue curve represents the control experiment; the red curve represents the sensitivity experiment with cold-pool feedback and elevated convection; and the green curve represents the experiment with cold-pool feedback only.

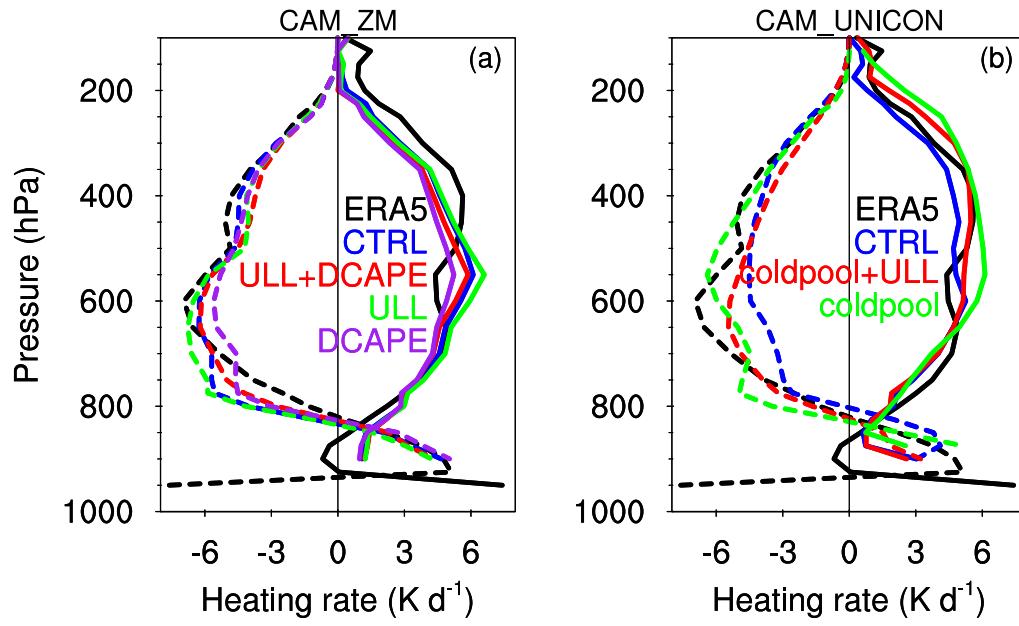


Fig R2. Simulated profiles of Q_1 (solid) and Q_2 (dashed) over SCB compared with ERA5 for CAM_ZM (a) and CAM_UNICON (b).

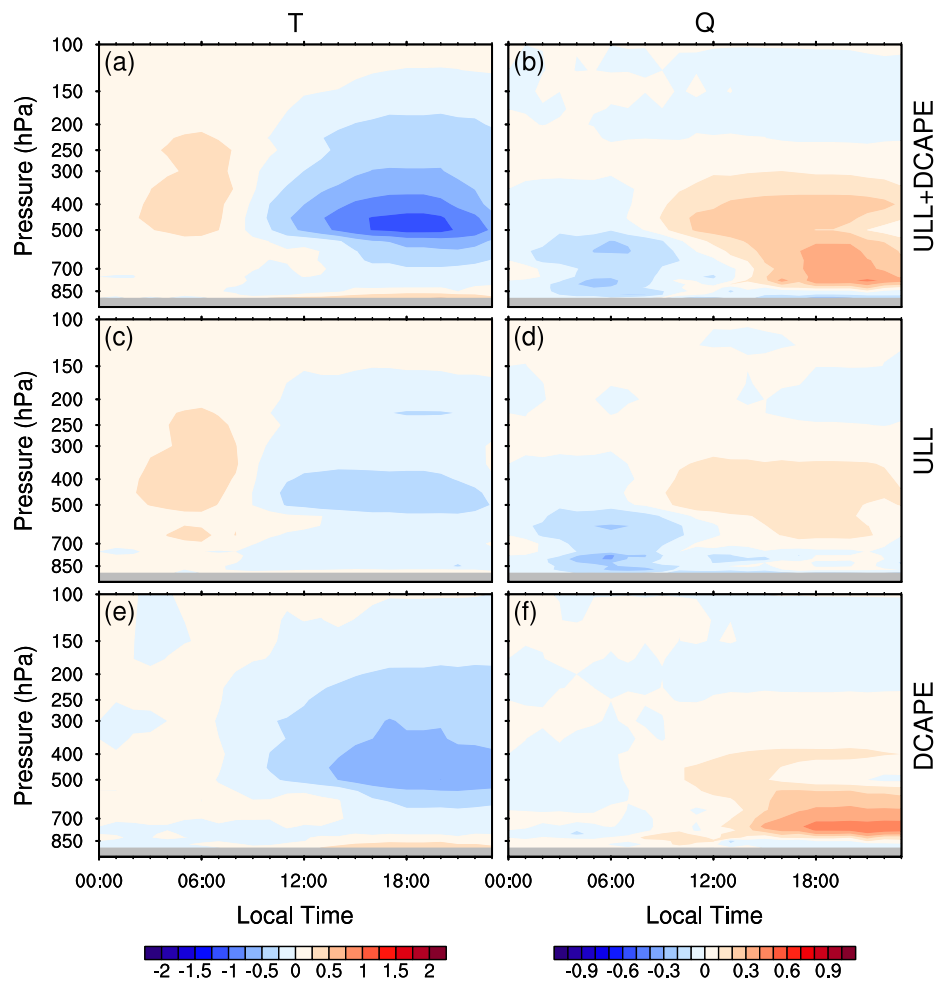


Fig R3. Temperature and moisture differences between each sensitivity experiment and the control run over SCB for CAM_ZM.

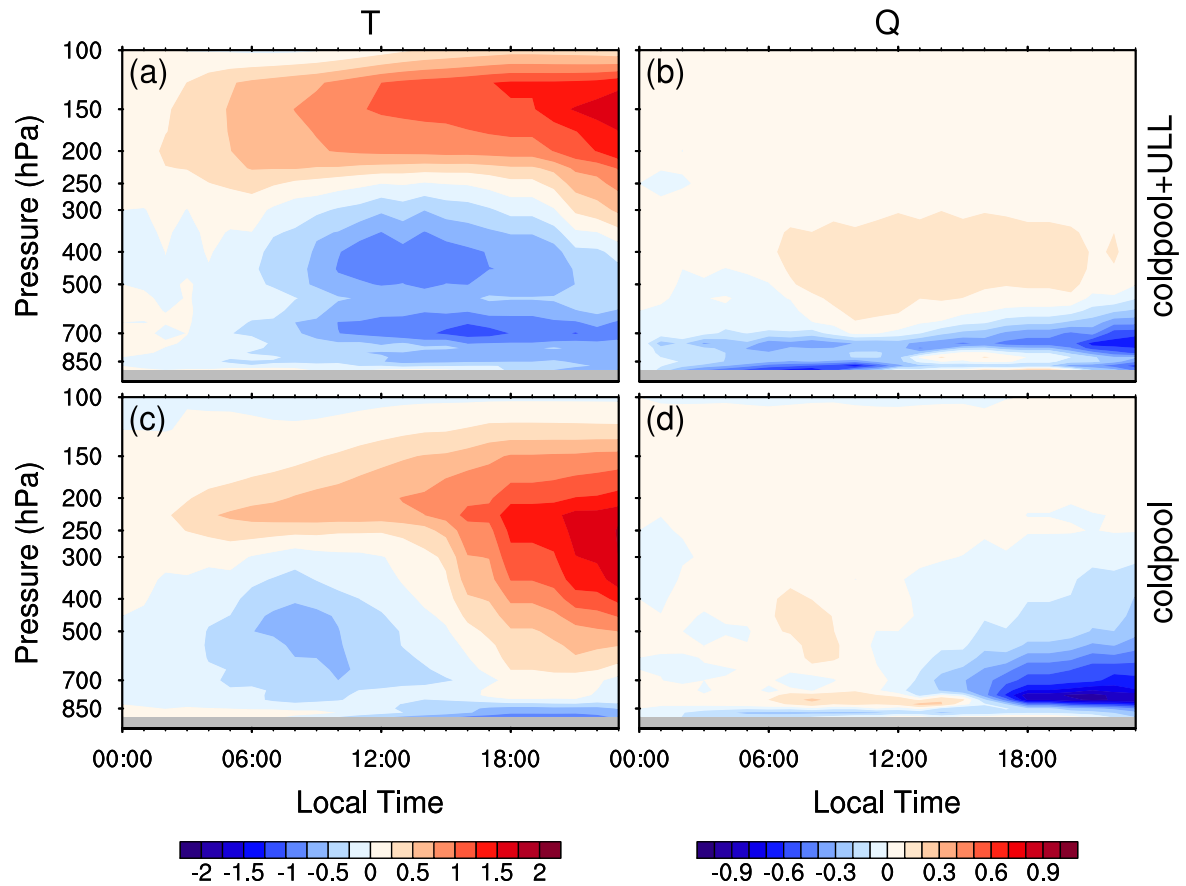


Fig R4. Same as Figure R3 but for CAM_UNICON.

2. In Section 3.2, the modification to CAM_ZM is described as follows (lines 239–240):
“Xie and Zhang (2000) addressed this by reducing Mb when dCAPE is below a threshold value of $100 \text{ J kg}^{-1} \text{ hr}^{-1}$, with the reduction factor linearly related to dCAPE.”
 However, this appears inconsistent with the statement in Section 2.2, which says that the CAM_ZM sensitivity experiment “adopts the dCAPE trigger and permits elevated convection, following Xie et al. (2019).” These are not the same implementation. The authors must clearly state which version was actually used in CAM_ZM_SENS and, preferably, provide the governing equations.

We thank the reviewer for pointing out this inconsistency. The dCAPE trigger follows Xie and Zhang (2000), while the implementation of elevated convection follows Xie et

al. (2019). These are two complementary components of the CAM_ZM sensitivity experiment. Specifically, the scaling of the cloud-base mass flux M_b is explicitly stated as follows, which is fully consistent with the description in Section 3.2. We have clarified this point and included the governing equation in the revised manuscript (L142; L244; Table 1).

$$M_b = \begin{cases} \frac{CAPE}{\tau \cdot F} & \text{if } dCAPE > 100 \\ \frac{CAPE}{\tau \cdot F} \cdot \frac{dCAPE}{100} & \text{if } dCAPE \leq 100 \end{cases}$$

3. Section 5 and Fig. 9 compare simulated Q1/Q2 against “observations derived from ERA5 reanalysis as residuals ... following Yanai et al. (1973),” and the figure caption labels these as “observations.” However, ERA5 is a model product, and its Q1/Q2 inherently carry uncertainties associated with the IFS convection scheme. The authors themselves note that ERA5 exhibits an early phase bias in precipitation over the TP and SECN (line 185). I suggest relabeling these as “ERA5-derived Q1/Q2” and discussing how ERA5’s convection may contaminate the comparison. Alternatively, the authors could consider an independent constraint, such as TRMM/GPM latent-heating products.

We thank the reviewer for this important suggestion. We fully agree that referring to ERA5-derived Q_1 and Q_2 as "observations" is misleading, as ERA5 is a reanalysis product with inherent uncertainties associated with its convection scheme. We have therefore relabeled all relevant figures and text as "ERA5-derived Q_1/Q_2 " throughout the manuscript.

Regarding the suggestion to use TRMM/GPM latent heating products as an independent constraint, we have carefully considered this option. However, we note that the GPM Spectral Latent Heating (SLH) product provides only the diabatic heating rate ($Q_1 - Q_r$), rather than the full apparent heat source Q_1 , making a direct comparison with our Q_1 calculations technically challenging. Moreover, we note that the GPM Spectral Latent Heating (SLH) orbit product results in very limited temporal sampling over our study

regions (only 1–2 overpasses per day), which is insufficient for resolving the diurnal cycle of Q_1/Q_2 —the central focus of our analysis.

We also fully agree with the reviewer that ERA5’s convection scheme can affect the comparison. From their diagnostic definitions, Q_1 and Q_2 are obtained as residuals of the thermodynamic and moisture continuity equations, and both involve vertical advection terms. Vertical advection depends on the vertical velocity, which is itself strongly influenced by the model’s physical parameterizations—especially convection. For example, if convection is under-represented in ERA5, the associated vertical velocity would be weaker, which would in turn bias Q_1/Q_2 low. We have added a discussion in Section 5 (Lines 440–447) noting that ERA5-derived Q_1/Q_2 carry uncertainties associated with the IFS convection scheme.

4. The manuscript tests only three subregions of China, without covering eastern or northern China. I therefore suggest moderating the title to reflect a more limited regional scope.

We agree that the original title may imply broader spatial coverage than our study actually provides. Our analysis focuses on three representative regions—the Tibetan Plateau (TP), southeastern China (SECN), and the Sichuan Basin (SCB)—which capture the key afternoon and nocturnal precipitation regimes, respectively. These regions are not intended to represent all of China, and we have therefore revised the title to more accurately reflect this regional scope. The revised title is: “Sensitivity of the Summertime Diurnal Precipitation Cycle over Three Representative Regions of China to Convective Trigger and Closure Treatments in a Single-Column Model”.
Thank you.

Minor comments

- 1. Line 132: “planet boundary layer” should be “planetary boundary layer”.*

Adopted and done.

2. *Line 147: Please justify the use of a 3-hour momentum relaxation toward ERA5.*

We thank the reviewer for this comment. The 3-hour momentum relaxation affects only the wind field and has no direct impact on the thermodynamic or diabatic heating processes that are central to this study. We have therefore removed this sentence from the revised manuscript to avoid confusion.

3. *The investigation uses only one summer (2021), which was characterized by notable circulation anomalies, and composites are based only on precipitation days exceeding 5 mm d⁻¹ over the TP and 10 mm d⁻¹ over the SECN and SCB. The rationale for and limitations of these choices should be discussed.*

We acknowledge that our analysis is based on a single summer (2021), which may have been influenced by notable circulation anomalies. However, this season still contains a sufficient number of precipitation events for robust composite analysis: 49 days over TP, 45 over SECN, and 34 over SCB. These sample sizes are comparable to those used in the observational composites of Liu et al. (2026). Moreover, the precipitation thresholds (5 mm d⁻¹ for TP; 10 mm d⁻¹ for SECN and SCB) are identical to those adopted by Liu et al. (2026), ensuring consistency and facilitating direct comparison with their results. Since all SCM experiments are driven by the same large-scale forcing, the single-summer setup does not affect our main conclusions regarding the sensitivity of the DCP to convective trigger and closure treatments. We have added this clarification in the revised manuscript (L165-166; 170-173).

Response to Reviewer #2:

Thank you for your professional review and instructive comments on our manuscript. We have revised the paper carefully based on your comments. These are described as follows (*italic text in blue color is from your review*).

This study presents a systematic evaluation of three convective parameterization schemes (ZM, TDK, and UNICON) using a single-column model, focusing on how different trigger and closure treatments affect the simulation of the summertime diurnal cycle of precipitation over China. The experimental design is well conceived, and the analysis spans from precipitation and cloud-base mass flux to diabatic heating and thermodynamic biases, forming a coherent physical chain. The findings offer valuable insights into the role of closure assumptions in shaping DCP and the applicability of various improvement strategies. The manuscript is suitable for publication in ACP pending minor revisions. Below are my specific comments and suggestions.

1. Although ERA5 can serve as a reference of observation, Liu et al. (2026) has pointed out the deficiencies of ERA5 in representing the diurnal cycle of precipitation and diabatic heating. The authors should add statement to warn the potential limits when using ERA5 as forcing and comparison.

We fully appreciate the reviewer's concern. ERA5 likely carries uncertainties in its large-scale forcing, as divergence and vertical velocity are not directly assimilated and thus remain subject to parameterization-induced errors. Nevertheless, ERA5 remains the most reliable product currently available for driving SCM simulations focused on precipitation over China. More importantly, our use of ERA5 forcing consistently reveals the sensitivity of the DCP to various aspects of convection schemes, which is the central focus of this study. Following the reviewer's suggestion, we have added a statement in Section 2.1 (Line 120-123).

2. In Figure 4, the launching-level frequency for CAM_TDK shows a distinct peak around 600 hPa, substantially higher than for ZM and UNICON. Could this be related to the TDK scheme's treatment of mid-level convection?

We thank the reviewer for this insightful observation. Yes, the distinct launching-level peak near 600 hPa in CAM_TDK is directly related to the TDK scheme's treatment of mid-level convection. Unlike ZM and UNICON, which primarily launch parcels from the boundary layer or surface, the design of middle-type convection in TDK allows it to tap into mid-level instability, which is particularly relevant for nocturnal elevated convection over SCB. We have clarified this point in the revised manuscript (L286-288).

3. Line 100: "aiming to elucidating" → "aiming to elucidate".

Adopted and done.

4. Line 127: "shallow convection are treated with" → "shallow convection is treated with".

Adopted and done.

5. Lines 285–287: The attribution of stronger nocturnal convection in the CAM_TDK sensitivity run to daytime convective suppression could be further supported by referencing the reduction in PBL-contributed PCAPE. I suggest revising the sentence to: "...likely because the exclusion of PCAPE contributed by PBL during the daytime (Figure 3f) suppresses convection, thereby favoring subsequent nocturnal development".

We thank the reviewer for this constructive suggestion. The sentence has been revised as recommended to emphasize the role of PBL-contributed PCAPE reduction in

suppressing daytime convection and favoring subsequent nocturnal development. The change has been made in the revised manuscript (Line 294-295).

6. Heading of Section 5.1: The use of "Observation" for ERA5-derived Q_1 and Q_2 may be misleading, as ERA5 is a reanalysis product rather than direct observations. I recommend rephrasing the heading more accurately.

The heading of Section 5.1 has been rephrased as “Diurnally Averaged Q_1 and Q_2 : Simulation vs. ERA5” to more accurately reflect the data source. Thank you.

7. Please check and ensure that the headings follow a consistent capitalization style. E.g. Line 228, 318, 385, 422.

Thank you for pointing this out, we have unified the capitalization style of all section headings throughout the manuscript.

8. Line 508: “deteriorates the thermodynamic fields” → “deteriorate”

Adopted and done.