

Reviewer comments in italics.

Author comments in upright text.

Review of “Stability constraining the radiation-driven convective anvil outflow”, by Zhequan Wang

This paper employs satellite-based observations to estimate local radiative and stability controls on the size and altitude of convective anvil clouds. Where the traditional anvil literature stemming from the FAT hypothesis is focused on the anvil dynamics imposed by radiation in distant clear-sky regions, this paper focuses on cloud radiative effects at the site of the anvil itself.

The paper identifies two local contributions to this anvil outflow: (1) A “radiative destabilization” outflow in which differential radiation between different vertical layers of a cloud causes horizontal divergence there, and (2) A “radiative heating” outflow in which net cooling against a background vertical gradient in static stability causes divergence. The manuscript then seeks to unify this work with previous “clear-sky convergence” (i.e., FAT-like) explanations for the location and horizontal extent of anvil clouds, identifying a critical “stability constrained radiatively-driven outflow layer” above the level of minimum static stability and below the level of the maximum gradient in static stability. Finally, it shows that the radiatively driven outflow is constrained to this stability-constrained layer even when the cloud top extends much higher, affirming the leading importance of these stability constraints rather than radiative constraints.

Some of the ideas presented here are novel to me: the analysis of radiatively-driven outflow at the site of an anvil cloud was especially interesting. The stability-constrained outflow layer is likely already known by intuition to other specialists in anvil clouds, but I think the author does a valuable service to identify and study this layer it. However, I do have substantial comments and corrections to suggest. For this reason, I recommend major revisions before publication.

Seth Seidel

Response: Many thanks for insights and efforts of Dr. Seth Sidel in reviewing this manuscript.

The author greatly appreciates your very valuable comments that help improve this manuscript.

I have carefully considered these comments and revised the manuscript accordingly.

Major Comments

1. At lines 213-220, the text explicitly states that radiation in clear skies “can constrain the anvil outflow through the mass conservation”. Then the text makes a point that Figures 2c and 2f show a balanced, radiatively-driven circulation between the cloudy sky and the clear sky. But this is impossible! The diabatic heating in cloudy skies is dominated by latent heating in the convective core. Much of the radiative cooling in clear skies is balancing that latent heating.

There is not a fully balanced diabatic circulation with radiation alone. Furthermore, the text suggests that this circulation is evidence that the clear-sky convergence must happen at the same level as the anvil divergence estimated in this study. Insofar as the anvil divergence is indeed at the same level as the clear-sky convergence, it is not because of a balanced circulation. The paper already hints at a better explanation: there is a special stability constraint that focuses both cloudy divergence and clear-sky convergence at that location. The fact that the radiative effects have opposite signs in the two regions is, from the perspective of this theory, a happy accident.

Response: Thanks for this insight! The latent heating dominates the diabatic heating and circulations in convective regions, particularly for heavy precipitating cores. Without the diabatic heating, radiation cannot explain this circulation. And the balance between the radiation-driven divergence and convergence is likely because the unified constraints of stability.

Accordingly, the analyses of this manuscript have been modified as follows:

- (1) At the beginning of analyses in Sect. 3.1, it has been clarified that diabatic heating is contributed by latent and radiative heating. It has been highlighted that only the radiation-driven divergence and convergence are focused on.
- (2) For the analyses in the revised manuscript, only anvil clouds that are organized within convective systems, nearly non-precipitating, and produced from the convective top are considered.
- (3) It is highlighted that the SCRAOL provides unified constraints in cloudy and clear skies, which are discussed separately in Sect. 3.2 and Sect. 3.3 in the revised manuscript. Compared with the previous version that primarily discusses the constraint in the cloudy sky, more discussions have been added for the stability constraints on the clear-sky convergence. Thus, the unified stability constraint makes the radiative-driven convergence and divergence at similar altitudes, which may enhance the circulation but is not the primary driver of the circulation.

2. Lines 309-313, Figure 5b: It is not intuitively obvious to me that the anvil altitude should be lower because EAS is greater. I understand why high-altitude anvil clouds will be less extensive at high EAS (according to the stability iris argument), but the magnitude of the minimum in stability has no obvious bearing on the altitude of clouds. I suspect this reduction in measured “anvil” altitude may be due to the reduced anvil area: If the measurement captures some non-anvil clouds (such as congestus clouds) as well as true anvils (defined as deep convective outflows of ice clouds), then reducing the amount of anvil clouds will reduce the mean cloud

altitude by composition, even if the true anvils have not changed their own altitude.

Response: Thanks also for this insightful comment! Additional analyses have been supplemented in the main text of the revised manuscript.

(1) Anvil cloud identification has been modified. The anvil identification needs to satisfy three conditions: organized within MCSs, nearly-nonprecipitating (precipitation rate less than 1 mm/hour), and produced from the convective top rather than developing from the surface (cloud base higher than 5 km). As shown in Figure 1 below, this method well separates anvil clouds from deep convective clouds. In the previous version of the manuscript, the anvil and deep convective clouds have not been well separated.

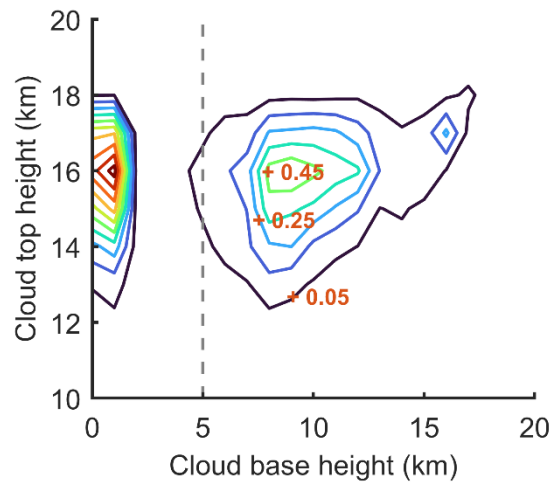


Figure 1. Joint PDF of GATM MCS cloud base and top heights of BT < 260 K and precipitation rate < 1 mm h⁻¹. The PDFs are normalized by dividing its maximum value. Contours of the joint PDF are shown from 0.05 to 0.95 at intervals of 0.10. The grey dashed line indicates the 5 km cloud base.

(2) EAS is no longer used for analyses in the revised manuscript. Instead, the magnitudes and altitudes of $(\frac{1}{S})_{max}$ and $(\frac{\partial}{\partial p}(\frac{1}{S}))_{max}$ are used, which can explain variations in SCRAOL and thereby constitute constraints on anvil clouds. This would provide the linkage between anvil clouds and stability more clearly. Thus, they are used to first explain the variations of the

radiation-driven divergence, and then to further explain the variations in the anvil properties (as identified above). In this way, in Figure 2 (shown below), consistent with the reviewer's comment, the changes of the anvil top are primarily associated with the variations in the altitude of $(\frac{1}{S})_{max}$ and $(\frac{\partial}{\partial p}(\frac{1}{S}))_{max}$. Their magnitudes are primarily associated with the strengths of the radiation-driven divergence. Additionally, the enhanced magnitudes of $(\frac{1}{S})_{max}$ and $(\frac{\partial}{\partial p}(\frac{1}{S}))_{max}$ may also contribute to thicker anvil clouds, by favoring both the peak of vertical outflow and the entire outflow layer.

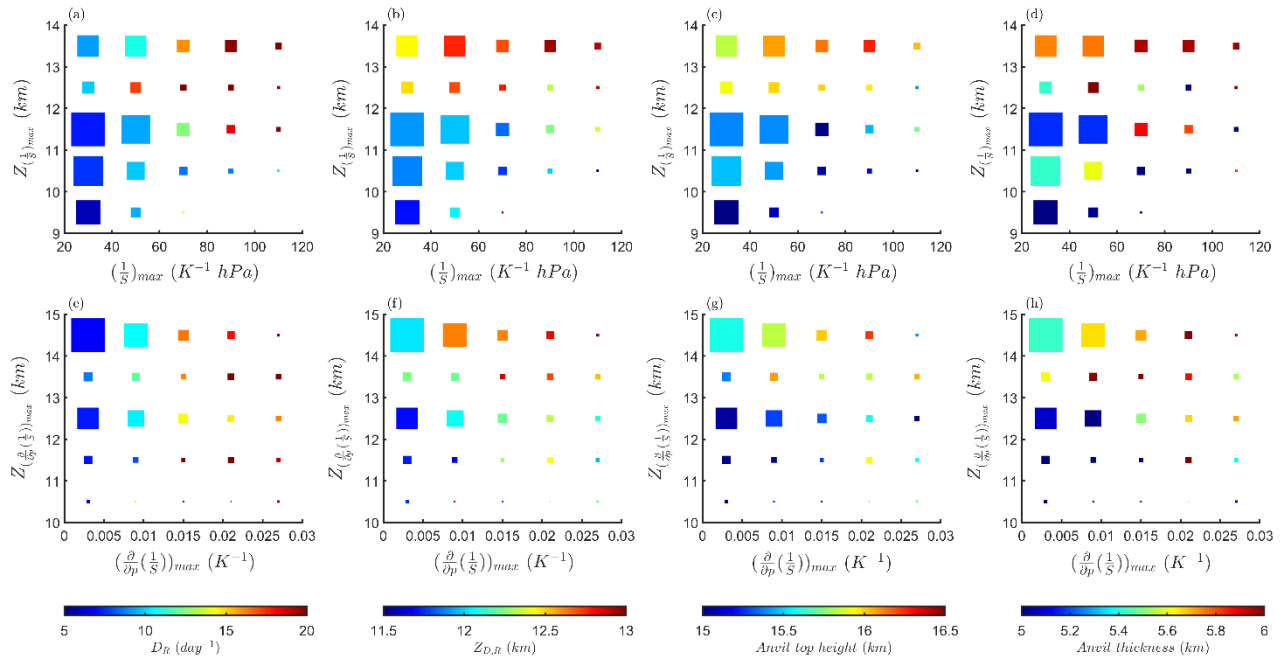


Figure 2. Associations of anvil clouds with the SCRAOL constraints. (a-d) Average D_R , $Z_{D,R}$, anvil top height and anvil thickness in each bin of magnitude and altitude of $(\frac{1}{S})_{max}$, respectively. The size of each square is proportional to the sample count. (e-h) Same as (a-d), but for $(\frac{\partial}{\partial p}(\frac{1}{S}))_{max}$.

3. The writing needs improvement, as it has many grammatical errors that impede interpretation. Given the volume of errors, I would suggest running this through grammar-checking software, though I don't know which would be available in the author's country. Perhaps even Microsoft Word would be helpful. In my own work, I have even used ChatGPT to identify grammar and style errors. (However, an LLM prompt must include explicit directions to only correct grammar and style errors while preserving the author's voice and intent.)

Response: Thanks. The grammar has been checked based on AI tools.

Minor comments:

- 1) Line 57: The manuscript defines FAT as the “Fixed Anvil-top Temperature”. However, FAT theory is not usually used to predict the top of the anvil. Rather, it is used to predict the altitude of the maximum of anvil cloud fraction or some other bulk measure of anvil cloud location.

Response: It has been corrected as: “fixed anvil temperature”.

- 2) Line 67: This is the first instance where the manuscript refers to differential radiative cooling such that the anvil top cools more as “destabilizing” the anvil, leading to outflow. I found this terminology confusing. My understanding of the theory being presented in this paper is that additional longwave cooling at the top of the anvil (relative to the bottom) compresses the anvil in the vertical, causing it to spread out in the horizontal. If the author wishes to refer to this as a destabilization, the manuscript needs more clearly explain what is the instability and how it generates an outflow.

Response: Thanks. It has been further clarified as: “Specifically, LW radiation warms cloud bases and cools cloud tops. This effect tends to destabilize the anvil and compress it vertically to promote horizontal outflow.”.

- 3) Line 80: Can LW and SW heating profiles really be measured? I would think they can only be estimated.

Response: It has been corrected.

- 4) Line 84: Would it be helpful to define RAOL when the Lilly 1988 paper is first introduced? That would better connect the jargon to the ideas being introduced.

Response: It has been modified.

- 5) Lines 127-128: “Nevertheless, a strong negative correlation between daily high-cloud area coverage and the ERA5-based EAS exceeds -0.6 over the Tropical Western Pacific (TWP).” - The importance of this should be emphasized in plain language: A greater minimum in static stability is associated with reduced anvil cloud coverage. It may also be worthwhile to explain a little more what’s going on: The static stability is inversely related to cloudy mass flux (or vertical velocity), which must generally go to zero (or near zero) via detrainment at the top of the troposphere. Greater static stability causes greater mass flux, which causes greater detrainment (or divergence) in the anvil layer.

Response: Thanks! The importance of stability has been emphasized in Sect. 4.1 in the revised manuscript: “Stability is inversely related to cloudy mass flux (or vertical velocity), which must generally go to zero (or near zero) via detrainment at the top of the troposphere. Greater $(\frac{1}{S})_{max}$ causes greater mass flux, corresponding to greater divergence in the anvil

layer, and vice versa. Thereby, the vertical minimum stability (leading to $(\frac{1}{S})_{max}$) also defines the estimated anvil-outflow stability (EAS) for radiative processes (Wang and Yuan, 2025). A strong negative correlation (exceeding -0.6) between daily high-cloud area coverage and EAS is found over the TWP.”.

6) Line 139: Calling these terms “radiative destabilization” and “heating” is not intuitive to me? What is the instability? Both terms have radiation and both terms have stability. The difference is that the first term is due to a vertical gradient in radiation and the second term is due to a vertical gradient in static stability. When reading the paper, I thought of the first term as radiatively driven vertical compression and the second term as a static stability-driven vertical compression.

Response: Thanks! A new section (Sect. 3.1 in the revised manuscript) has been added to explain these terms and their physical meanings at the beginning of analyses.

Given $\omega = -\frac{Q}{S}$, R serves as the driver of ω and D_R (the divergence contributed by radiation), since D_R does not exist if $R = 0$. S serves as the thermodynamic constraint on the dynamic effects of R . D_R can be further expressed as:

$$D_R = \frac{1}{S} \frac{\partial R}{\partial p} + R \frac{\partial}{\partial p} \left(\frac{1}{S} \right).$$

The term $\frac{1}{S} \frac{\partial R}{\partial p}$ represents the D_R forced by the vertical gradient of R , and this dynamic effect is constrained by $\frac{1}{S}$. The upper-tropospheric thermal stratification is normally stable ($S > 0$).

When $\frac{\partial R}{\partial p} > 0$, lower levels are heated more than higher levels, corresponding to radiative destabilization. Accordingly, this yields stronger ω at lower levels than at upper levels, compressing the layer and enhancing the vertical gradient of mass flux; this enhanced gradient must be balanced by stronger D_R , and vice versa. The less stable the stratification is, the more efficient D_R driven by $\frac{\partial R}{\partial p}$ becomes. Another term $R \frac{\partial}{\partial p} \left(\frac{1}{S} \right)$ represents the dynamic effect forced by R and constrained by $\frac{\partial}{\partial p} \left(\frac{1}{S} \right)$. For anvil outflow near the tropopause transition layer between the convection-dominated troposphere to the radiation-controlled stratosphere (Folkens, 2002; Gettelman and Forster, 2002), S increases with height, corresponding to $\frac{\partial}{\partial p} \left(\frac{1}{S} \right) > 0$. If this layer, whose base is less stable than upper levels, is heated ($R > 0$), ω at the base would be stronger than that at upper levels, compressing the layer and promoting D_R , and vice versa. The stronger the vertical gradient of stratification is, the more efficient D_R driven by R becomes. Overall, SCRAOL encompasses two dynamic effects associated with

radiative destabilization and heating, both of which are constrained by atmospheric thermal stratification.

- 7) Figure 1: I am assuming that these are composites of MCSes or some other deep convective element. Can this be clarified in the figure caption? Additionally, can the text of the manuscript clarify how the composites were weighted? Based on area coverage or based on count?

Response: For the analyses in the revised manuscript, the composited profile corresponds to only anvil clouds that are organized within convective systems, nearly non-precipitating, and produced from the convective top (see Response to Major Comment 2). These composites are weighted by count. These has been clarified in the caption and main text.

- 8) Lines 176-179: The text claims that below the minimum in S , the temperature profile is controlled by convection and above the minimum in S , the temperature profile is controlled by radiation. I think I understand what the manuscript is trying to communicate - we are moving from a layer (below) where the temperature profile can be understood in terms of convection alone to a layer (above) where the temperature profile can be understood in terms of radiation alone. There are two issues with this statement: (1) It is simply stated without any argument or source. (2) The second claim - that the anvil layer's temperature profile is controlled primarily by radiation - is probably not true. There, convection and radiation may be in close competition, but you don't get to a truly radiatively dominated regime until you're in the tropopause layer, where radiative heating prevents non-overshooting convection in equilibrium.

Response: Yes, I agree. It is a tropopause transition layer between the convection-dominated troposphere to the radiation-controlled stratosphere (Folkins, 2002; Gettelman and Forster, 2002). And within this transition layer, radiation and convection are in close competition, and radiation gradually becomes more important with height to increase stability, but does not control this layer completely.

This statement has been modified as: "As suggested by Gettelman and Forster (2002), below the level of $(\frac{1}{S})_{max}$, variations in S are dominated by the moist adiabat of condensational heating. According to the Clausius-Clapeyron equation, the saturation vapor pressure decreases as temperature falls with height, as does condensational heating. In this case, S first decreases with height, corresponding to $\frac{\partial}{\partial p}(\frac{1}{S}) < 0$. Above the level of $(\frac{1}{S})_{max}$, S becomes gradually more controlled by radiation to increase with height and thus $\frac{\partial}{\partial p}(\frac{1}{S}) > 0$."

- 9) But, notably, the impact of $R \frac{\partial}{\partial p} \left(\frac{1}{S} \right)$ in Eq. (4) is missing in [the PHAT and stability iris] hypotheses.” - Is this true? This term can be derived directly from Harmann and Larson (2002), though that paper emphasized the gradient in clear-sky radiative cooling. Furthermore, I understand the gradient in static stability to be the main theoretical focus of the PHAT paper (Hartmann and Zelinka 2010). Even in the case of the stability iris (Bony 2016), this term is present in their equations, and in fact we should expect a baseline increase in static stability to reduce the magnitude of both the stability-gradient and cooling-gradient terms.

Response: Yes, I agree. This statement has been deleted.

- 10) Line 250: I believe the term “overshoots” is misused here. Overshooting typically refers to convection which has risen above its level of neutral buoyancy, retaining positive vertical velocity despite negative buoyancy. (The same mistake appears later in the manuscript as well.) Instead, I believe the author means that the convection “reaches” that level or perhaps that it detrains there.

Response: Yes, it has been corrected as “reaches”.

- 11) I believe it would be helpful to have a plain-language explanation of why the layer between $\max(1/S)$ and $\max(dS/dp)$ is so important. Tie these terms back to the theory. This is one of the central points of the paper, so it should be very what the author wants the reader to think. In particular, I don’t understand why $\max(dS/dp)$ must be the top of the outflow layer.

Response: Thanks. The SCRAOL constrained between the levels of $(\frac{1}{S})_{max}$ and $(\frac{\partial}{\partial p} (\frac{1}{S}))_{max}$ are further explained as follows, particularly for why the level of $(\frac{\partial}{\partial p} (\frac{1}{S}))_{max}$ is the SCRAOL top.

Overall, the stratification features, $\frac{1}{S}$ and $\frac{\partial}{\partial p} \left(\frac{1}{S} \right)$, limit the dynamic effects of radiative destabilization and heating, respectively. Near the tropopause, the transition layer between the convectively dominated troposphere and the radiatively controlled stratosphere leads to vertical maxima of $\frac{1}{S}$ and $\frac{\partial}{\partial p} \left(\frac{1}{S} \right)$ at ~12 and 14 km (Gettelman and Forster, 2002; Folkins, 2002). It requires deep convection to reach the level of $(\frac{1}{S})_{max}$, and radiative destabilization can then efficiently promote anvil production. Stability is inversely related to cloudy mass flux (or vertical velocity), which must generally go to zero (or near zero) via detrainment at the top of the troposphere. Greater $(\frac{1}{S})_{max}$ causes greater mass flux, corresponding to greater divergence in the anvil layer, and vice versa. Thereby, the vertical minimum stability (leading to $(\frac{1}{S})_{max}$) also defines the estimated anvil-outflow stability (EAS) for

radiative processes. Wang and Yuan (2025) found a strong negative correlation (exceeding -0.6) between daily high-cloud area coverage and EAS over the TWP .

When deep convection further develops to reach the level of $(\frac{\partial}{\partial p}(\frac{1}{s}))_{max}$, strong stability limits the dynamic effects of radiative destabilization, but radiative heating can then efficiently promote anvil production. Greater $(\frac{\partial}{\partial p}(\frac{1}{s}))_{max}$ corresponds to stronger vertical gradient of environmental stratification. Heating clouds in the less stable environment at lower levels can yield stronger buoyancy than that in the more stable environment at higher levels. The greater the vertical gradient of stratification is, stronger the dynamic effects of heating clouds are. Outside the SCRAOL, the radiative-dynamic effects are limited by strong stability and weak stability gradients. The magnitudes and altitudes of $(\frac{1}{s})_{max}$ and $(\frac{\partial}{\partial p}(\frac{1}{s}))_{max}$ contribute to the potential thermodynamic constraints on anvil clouds.

12) Line 282 and Figure 4 caption: I believe the text should say “gray contours”, not “black contours”.

Response: It has been corrected.

13) Figure 4h: Why is the radiative destabilization not better confined to the stability defined RAOL? On the right-hand side of the figure, I see considerable green contour above the blue line, which denotes the top of the outflow layer.

Response: The radiative destabilization in panel (h) shown below refers to $\frac{\partial R}{\partial p}$, while the its contributed outflow is $\frac{1}{s} \frac{\partial R}{\partial p}$. Thus, if $\frac{\partial R}{\partial p}$ exceeds the SCRAOL (i.e., the considerable green contour above the blue line; panel (h)), its dynamic effects are still largely constrained within SCRAOL (shown in panel (i)).

To eliminate this ambiguity between radiative destabilization and its contributed outflow, $\frac{\partial R}{\partial p}$ is used for labels in figures.

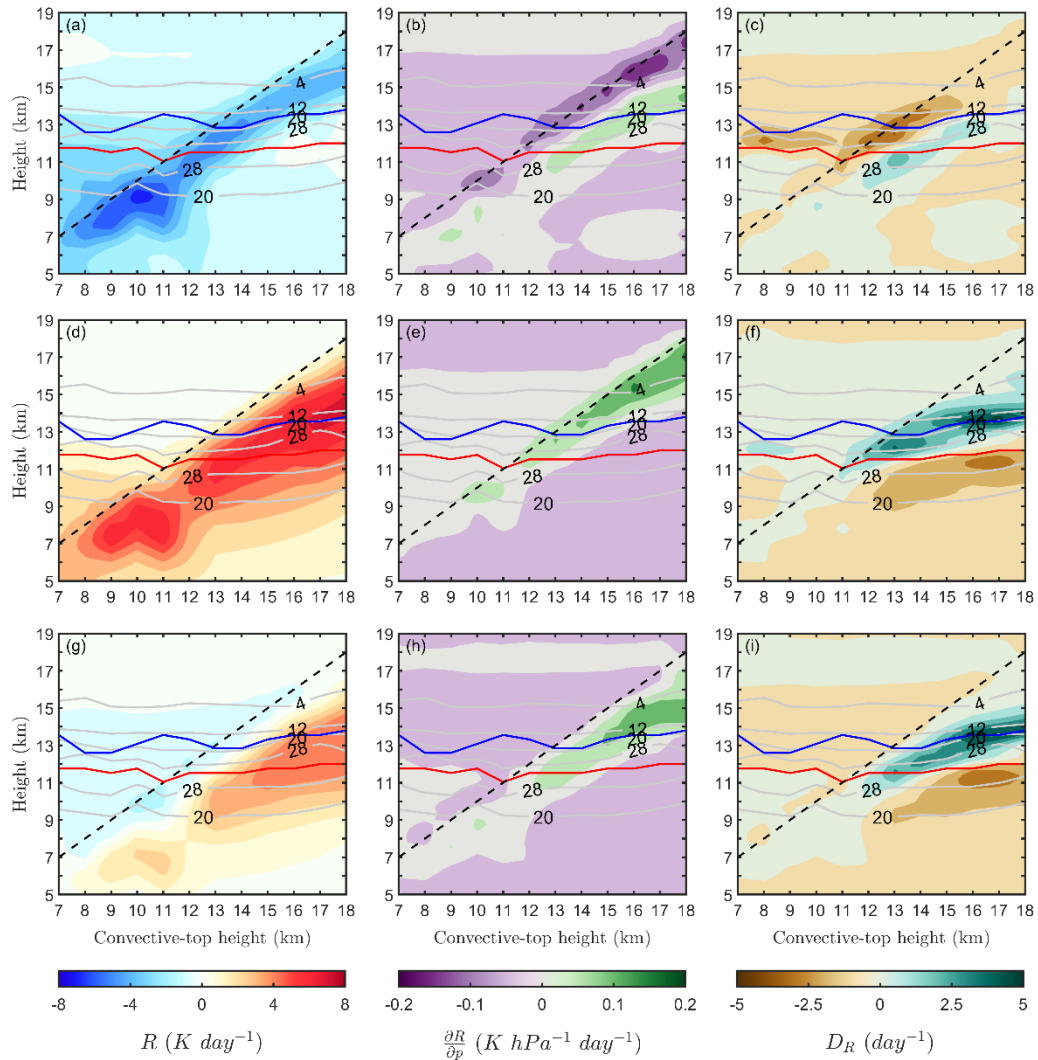


Figure 3. Thermodynamic constraints of SCRAOL under the convective regime based on deep convective and anvil clouds identified in the GATM dataset at 13:30 local time. R , $\frac{\partial R}{\partial p}$ and D_R associated with LW (a-c), SW (d-f) and net (g-i) radiation, respectively. The solid gray contours denote $\frac{1}{S}$, with the units of $K^{-1} hPa$. The red and blue lines mark the levels of $(\frac{1}{S})_{max}$ and $(\frac{\partial}{\partial p}(\frac{1}{S}))_{max}$. The dashed black lines indicate convective-top height.

- 14) Line 284: “But higher-topped clouds seem to correspond to a greater peak value of $1/S$.”
 - This statement should be better tied to the plot: The red curve increases in altitude as you move to the right.

Response: This sentence has been deleted from here. More analyses in a statistical sense have been added for the associations between anvil outflow and stability, as shown below.

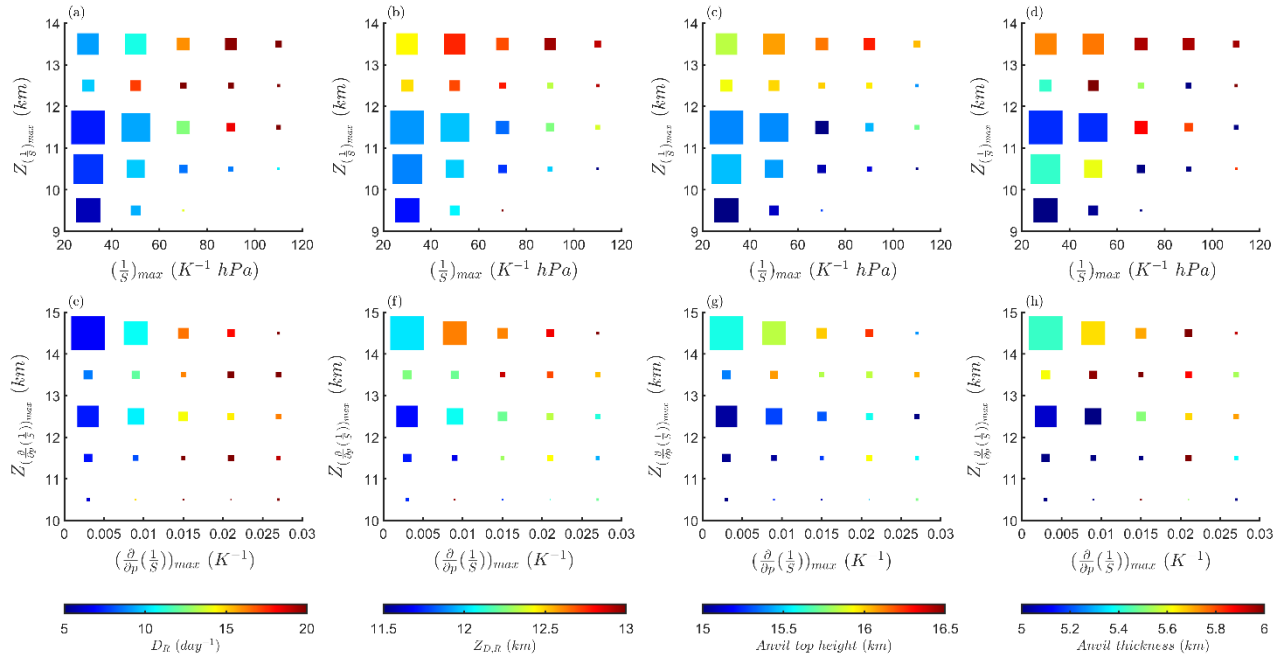


Figure 4. Associations of anvil clouds with the SCRAOL constraints. (a-d) Average $D_{R,1}$, $Z_{D,R,1}$, anvil top height and anvil thickness in each bin of magnitude and altitude of $(\frac{1}{s})_{max}$, respectively. The size of each square is proportional to the sample count. (e-h) Same as (a-d) but for $(\frac{\partial}{\partial p}(\frac{1}{s}))_{max}$.

15) Figure 5b: As a technical note, I do not know what is the contour interval for the color contours. Consequently, I have trouble comparing those contour steps to the distances on the vertical axis (Z_{EAS}). Also, there may be a unit error on the horizontal axis, as EAS should have units of K/km. I also think

Response: It has been corrected.

Reference

Folkens, I.: Origin of Lapse Rate Changes in the Upper Tropical Troposphere, Journal of the Atmospheric Sciences, 59, 992-1005, 10.1175/1520-0469(2002)059<0992:Oolrci>2.0.Co;2, 2002.

Gettelman, A. and Forster, P. M. d. F.: A Climatology of the Tropical Tropopause Layer, Journal of the Meteorological Society of Japan. Ser. II, 80, 911-924, 10.2151/jmsj.80.911, 2002.

Wang, Z. and Yuan, J.: A Novel Thermodynamical Predictor of Tropical High-Cloud Area Coverage: Estimated Anvil-Outflow Stability, Geophysical Research Letters, 52, e2025GL115026, <https://doi.org/10.1029/2025GL115026>, 2025.