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 extents 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

Major Comments

1. When I first read the manuscript, I was confused regarding what region is being studied. To explain, let us consider a three-region model for understanding anvil clouds:

A) The convective core
B) The detrained anvil cloud
C) The cloud-free region

In the traditional FAT hypothesis (as well as PHAT and the stability iris), we wish to constrain outflows from the convective core (A). We do so using static stability as well as the radiative cooling in the clear-sky region (C) or the total radiative cooling of the anvil and clear-sky region (B+C). This is typically termed a "clear-sky convergence", largely because the original argument made by Hartmann and Larson (2002) is that the convective core detrainment is constrained by clear-sky radiative processes. For that argument, it was convenient to neglect the radiative effects of the anvil cloud (B). And both Zelinka and Hartmann (2010, the PHAT hypothesis) and Bony et al (2016, the stability iris) use global-average radiation including cloud radiative effects, treating B+C as the convergence region. In a similar vein, Kuang and Hartmann (2006) are quite explicit that the theory is supposed to predict convective detrainment, meaning that the divergence region is A alone, and the convergence region is B+C.

This manuscript does something different. According to my reading, the author wishes to group A+B as core+anvil region whose divergence is balanced by convergence in the cloud-free region C. Theoretically, this is valid. However, because this difference in framework is not made explicitly clear up front, it can lead to confusion. Indeed, I struggled with this for some time.

There are two options to remedy this:

1) Explicitly state that this is a different convergence region and different divergence region from much of the FAT literature. Mention that although these are the same equations as in FAT, you are defining the relevant regions differently. (This option is less work.)
2) Reformulate this work to be more in line with the FAT literature by looking at the anvil region independently.

As a reader, I think either option is acceptable. But I would encourage the author to consider option #2. The main reason is that diabatic heating in the convective core+anvil region is dominated by latent heating in the convective core. Therefore, these calculations which address radiative heating are in effect only a modest correction term. Imagine instead a chain of reasoning that looks like this:

> 1) Radiatively driven convergence in B+C constrain the detrainment from the convective core (A), consequently constraining the *source* of anvil clouds. (This is a FAT-like statement).
> 2) Within the anvil region (B alone), vertical gradients in radiative heating and static stability can cause divergence (anvil spreading) or convergence (anvil shrinking).

This is now a partial explanation for the lifecycle of anvil clouds after they are detrained from the core, complementing the FAT literature rather than constructing a parallel theory. I think that's potentially very interesting. (Please note that this framework does not completely avoid having to mention latent heating: evaporation of the anvil is a local source of diabatic cooling.)

As I mentioned above, it is up to you which option to choose. The existing framework is not wrong.

2. 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.

3. 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.

4. 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.)

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

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.

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?

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.

9) “But, notably, the impact of $R\frac{\partial}{\partial p}(\frac{1}{S})$ in Eq. (4) is missing in [the PHAT and stability iris] hyotheses.” - 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.

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.

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.

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

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.

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.

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