Review of "Stability constraining the radiation-driven convective anvil outflow"

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1 General Comments

In this manuscript, the author makes two novel points:

1. He shows how clear-sky constraints on anvil altitude can be reconciled with the radiatively driven outflow of the anvil clouds.
2. He shows that the profile of atmospheric stability constrains the anvil outflow to occur near the tropopause.

I think both points are of interest to the scientific community and that publication in ACP is justified.

The main shortcoming of the manuscript in its current form is, in my opinion, that those points are hard to extract. The readability is hampered by poor sentence structure, and the author presents a lot of evidence without directly using it to make a point. The manuscript could be much improved by restructuring it to make the key takeaways more visible (See Section 2.2).

2 Specific Comments

2.1 Language

Not being a native English speaker myself, I know that getting the wording of every sentence right can be hard. Nevertheless, I think the language of the manuscript needs to be improved. Some simple means to achieve this are to limit the length of sentences, use fewer abbreviations, and use AI tools to check and improve the wording.

2.2 Structure

One possible way of restructuring the paper could be the following:

Introduction: Point out more clearly that clear-sky constraints on anvil altitude are hard to reconcile with the radiatively driven outflow postulated by Lilly (1988).

Data and Methods: Separate data and methods sections more clearly (see Section 2.3).

Reconciling clear-sky constraints with the radiatively driven anvil outflow (new section): Use Figures 1 and 2 to show that Eq. 4 works for clear-sky areas and in non-precipitating anvils. Most striking to me is the resemblance of the divergence profiles (Figure 2 c and f). The author could put more emphasis on this and quantify how well convergence in clear-sky regions and divergence in cloudy regions compensate. To me, this is a really interesting finding that demands more visibility.

Stability constrains radiatively driven anvil outflow: I suggest explaining this constraint after showing the equivalence between the divergence/convergence in cloudy/clear-sky areas, since the stability structure of the atmosphere constrains both. Figure 4 could appear in this section, since the author uses it to argue that the stability profile constrains the outflow to happen at around 12 km, even though radiative heating/cooling can occur at any altitude where a cloud resides. I also suggest changing Figure 5 to prove the stability constraint in a statistical sense (see Section 2.7).

I want to emphasise that this structure is just a recommendation and I do not expect the author to follow it strictly. However, I think some restructuring is necessary to improve the clarity of the manuscript.

2.3 Data description

The description of the data and methods in section 2.1 is hard to parse. In its current form, the introduction of the satellite products that are used is mixed with the method for identifying MCS and anvil clouds and a more detailed description of the data. This should be disentangled. I suggest first introducing the satellite observations in detail and making clear which variables are used and how they are derived from the observations. This section can be followed by the method of identifying MCS and anvil clouds, and another section in which the author explains why he limits the study to the TWP and daytime-only observations.

In section 2.2 the author states that ERA5 is used to calculate potential temperature, but ERA5 is also used to calculate stability, right? If so, this should be stated, and the formula used to calculate stability should be referenced (I guess it will be one of the equations in L41). I suggest not including EAS as a as another measure of stability, but just using stability itself for clarity (see section 2.6).

2.4 Discuss Neglect of Latent Heating

Latent heating is neglected in Eq. 4, which needs to be discussed in more detail. Jeevanjee (2022) argue that latent heating can't be neglected, even in the clear-sky atmosphere. The author neglects it also in cloudy areas, which is a strong assumption. This simplification should be stated more clearly, and Figures 1 and 2 can be used to argue that it seems to be a valid assumption.

2.5 Quantify Compensation of Divergence and Convergence

I think the paper would be strengthened if the author could show to what degree the cancellation between divergence in cloudy regions and the convergence in clear-sky regions holds. To this end, I suggest integrating convergence/divergence over the respective clear-sky/cloudy region and comparing the numbers. Discrepancies should be discussed. Possible explanations for such discrepancies are the convective cores, which will have a strong divergence, the neglect of latent heating, or horizontal density gradients.

2.6 Only use Stability and not EAS

It is not clear to me why the author introduces EAS as another measure of stability, rather than using stability itself. I think it would improve the clarity of the paper if only stability were used, which is the key metric for the stability constraint introduced in this study.

2.7 Update Figure 5

If I understand the author correctly, the main point of Figure 5 is to show that anvil altitude varies with the stability profile of the atmosphere. I generally find it desirable to show how well the stability constraint on anvil altitude holds in a statistical sense. However, I think Figure 5 could be improved by not using EAS and Z_{EAS}, which are somewhat unconnected to the rest of the paper, but rather use $max(\frac{1}{S})$ and $max(\frac{\partial}{\partial p}\frac{1}{S})$ as the predictors of anvil cloud height, to be consistent with the rest of the paper. One possibility would be to define Z_{S} as the altitude between $max(\frac{1}{S})$ and $max(\frac{\partial}{\partial p}\frac{1}{S})$, i.e. the altitude at which we expect anvils to occur from the stability constraint. This could be used to plot a 2D histogram of Z_{S} and Z_{anvil} to see how well the actual anvil altitude is predicted from the stability constraint.

2.8 Include Nighttime Observations

The author limits the analysis to the daytime overpass of the A-train satellites. Including nighttime observations would significantly strengthen the manuscript. There is no obvious reason why the stability constraint should not hold at nighttime. Reproducing Figures 1, 2, and 4 for nighttime would be easiest, but they would need to be included in a supplement. An alternative could be to produce one additional figure to show the similarity (or differences) between day and night. Maybe convergence, radiative heating and $1/S$ during the day and the night.

3 Technical Comments

L8: This seems like an overstatement since geostationary satellite observations and the A-train satellites have been available for many years. What is new in this paper is that they are combined to investigate anvil formation. This should be specified.

L20: To be more precise, I suggest using "anvil cloud altitude" rather than "anvil-radiation feedback".

L41: Show only the equation for stability that you are using (pressure coordinates, I guess).

L49: This link was already established in earlier papers, which should be cited here. For example Bony et al. (2016); Jeevanjee (2022).

L62: This paragraph does not include any support for the claim that model parameterisations of anvil outflow lack observational evidence to be improved. While I believe this statement is true, some citation would be necessary to prove this. Otherwise, I suggest removing the statement, since it does not seem essential for the following arguments.

L119: Please include the citation for ERA5: Hersbach et al. (2020).

L120: Isn't the horizontal grid spacing of ERA5 0.25°?

L150-L152: This should be moved to the data and methods section, where the study region and time period are introduced.

Figure 3: I really like this figure! The convergence label on the blue arrow is somewhat confusing since it is placed between clear skies and cloudy skies. It should be all the way in the cloudy box to make sense.

L253: "Away from this layer, anvil production is suppressed by the environmental stability." This is a key sentence that is proven by Figure 4. This should be pointed out more clearly. I would put Figure 4 before Figure 3 and use Figure 4 to carefully explain this point and then pick it up again in some kind of summary based on the schematic (now Figure 3).

Figure 4: Is this only data for 13:30 local time? Please specify. Red and blue lines are not introduced in the caption, and the contours of stability are grey, not black.

L321: "for" identifying convection ..., not "of". Same for "of" detecting.

L324: It is a bit hard to believe that nobody has published anything substantial on the radiatively driven anvil outflow process between 1988 and 2025. I suggest checking for other references and citing them here and in the introduction.

References

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