Review of " The Impact of Aerosol on Cloud Water: A Heuristic Perspective" by Hoffmann et al. [Research Article, egusphere-2024-1725]

This paper employed a heuristic model, which was derived from two prognostic equations (liquid water path *L* and cloud droplet number concentration *N*), to understand cloud water adjustments in stratocumulus to aerosols. The primary model parameters were chosen by matching the ensemble LES modeling of Glassmeier et al. (2021). This heuristic model successfully reproduced the inverted “v” shape relationship for *L*-*N*, which was found in LES simulations and satellite retrievals: *L* increases with aerosols at low *N* via suppressing precipitation, while *L* decreases with aerosols at high *N* through thermodynamic effects such as entrainment drying. Intriguingly, the authors found a tight relationship between adjustments at low and high *N*, demonstrating that entrainment effects that predominate at high *N* influence adjustments in precipitating clouds at low *N*. They also examined the sensitivity of cloud water adjustments in precipitating and non-precipitating clouds to the heuristic model’s parameters, along with external *L* or *N* perturbations.

I enjoyed reading this paper. It was very well organized and easy to follow. This study showcased a useful and effective way of applying a simple heuristic model to decipher the intricate cloud-aerosol interactions (ACI). The tight relationship between adjustments at high and low *N* found within this study is also beneficial in identifying potential aerosol-meteorology co-variability in the ACI study. I believe this paper will be suitable for publication in ACP if some issues outlined below are addressed.

Major comments:

1. Lines 69-70 (or L69-70): The authors claimed that $L_{\infty,h}$ is applied to all *N* with *the same* $m_{\infty,h}$, because they assumed that the sensitivity of temporal change in L due to thermodynamics to *N* *seems* not dependent on the presence of precipitation. However, if we take a closer look at Figure S1 (or see figure below), the sensitivity of L tendency due to entrainment (see the slope of the green line) to N is found to be notably different between the precipitation period and the non-precipitation period.

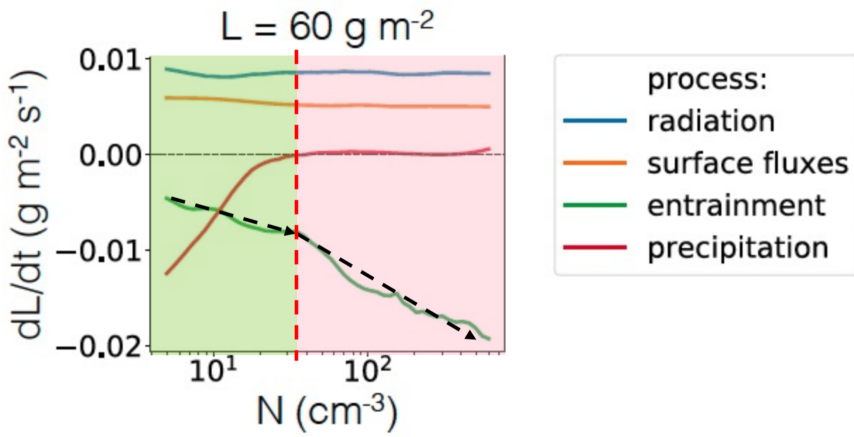

Figure R1. Same as Figure S1, but adding black dashed lines to help discern the difference in green line slopes between the presence (marked by green areas) and non-presence (marked by pink areas) of precipitation.

Specifically, when precipitation occurs (or at low *N*), the boundary layer becomes more stable, thereby leading to a relatively small sensitivity of *L* tendency due to entrainment to *N*. Conversely, the sensitivity should be relatively large when precipitation is absent (or at high *N*), as demonstrated by Figure R1. Given these facts, the authors need to clarify the rationality of assuming $m_{\infty,h}$ is independent of *N* and discuss the impact of this hypothesis on the main conclusion.

The above sensitivity contrast also indicates weaker thermodynamic adjustments (mainly entrainment) at low *N* that transition into stronger thermodynamic adjustments at high *N*, which is, however, opposite to the authors' discussion in L230-234. Are there any reasons for the inconsistency?

2. In Section 5, the authors examined the susceptibility of cloud water adjustments to external perturbations in *N* and *L*. They modeled these perturbations as Bernoulli processes. The perturbation of τ, σ, and m is chosen to represent the general sensitivity of the system rather than matching a realistic case. I am curious if it is possible to perturb these parameters or impose *N* (*L*) perturbations per the influence of large-scale meteorological factors (MFs) like the moisture contrast between 1000 hPa and 700 hPa, which can alter the efficiency of entrainment drying and thus influence cloud water adjustments, especially at high *N*. Such a perturbation due to MFs would be more realistic and physically reasonable.

3. The paper is well-structured and concise, but in certain places, it is overly brief, particularly when introducing concepts without sufficient explanation. This brevity may stem from the text limitations imposed by the previous submission to GRL. Given that ACP does not have such restrictions, I recommend that the authors expand on and clarify specific concepts or physical mechanisms in more detail. Below, I provide some examples for the authors' consideration.

(a) L45: The authors claimed that thermodynamic effects on L include the influence of entrainment, radiative cooling, and surface fluxes. It would be helpful if the authors could elaborate on how these three terms affect cloud water at a process level.

(b) Suggest briefly explaining the concepts of "entrainment warming and drying", "Brownian coagulation", "Bernoulli process", etc., and adding citations as well.

4. L31: The authors mentioned the limitations of LESs in understanding cloud water adjustments due to limited spatial domains and specific initial and boundary conditions. However, the authors tuned their heuristic model parameters to match the ensemble LES modeling of Glassmeier et al. (2021). In that regard, I'd assume the heuristic model derived here is subject to LESs' limitations. Generally, this study would be more insightful if the authors could use one paragraph or section to discuss their model's limitations and possible improvements (e.g., including a prognostic equation for cloud fraction), helping refine its applicability in future research.

Minor comments:

L16: "droplet concentration" to "cloud droplet number concentration"

L33: Did you mean co-variability of aerosols and meteorology? Please be specific.

L35: "letter" to "paper"

L39: "letter" to "paper"

L64: Does this source refer to cooling-induced water vapor condensation or enhanced PBL turbulent moistening?

L68: Add references for "many studies"

L72: Remove a duplicate "the"

L75: It would be helpful if the authors could provide more technical details on tuning the parameters to align with ensemble LES modeling. Additionally, including a validation figure of L evolution predicted by the heuristic model relative to LES modeling would enhance the clarity and robustness of the study.

L104: Please clarify how the cloud top effective droplet radius was derived and plotted in Figure 1.

L124: Is the threshold of 100 for N consistent with findings from LES modeling or satellite observations?

L163-171: The authors highlighted some interesting values when perturbing $m_{\infty,h}$. However, I am not sure if $m_{\infty,h} = 2.0$ is physically meaningful as entrainment is supposed to dominate at high N (Figure S1), and the sinking term (entrainment) for L outweighs the source terms (longwave radiative cooling and surface fluxes), yielding $m_{\infty,h} < 0$. It might be better to take into account some physical constraints when perturbing the parameters.