Response to Reviewer 1

The paper addresses how the cloud droplet concentration (N) affects the liquid water path (LWP) in terms of a factor m = d ln (LWP)/d ln (N). The paper contains some interesting ideas, and could be a nice addition to existing MLM studies. However, a central question is whether the MLM framework, and in particular the set up of the experiments in terms of construction of the buoyancy flux from the fluxes of energy and water (do they give a steady-state?), and the boundary conditions (constant surface fluxes?) justifies a generalization of the results. The results may depend strongly on the assumptions made in the model, like a zero entrainment response to simultaneous changes in N and the liquid water path (see the sentence "The conclusions drawn in this study are built upon the assumption that an increase in entrainment rate (w_e) due to an increase in N is exactly offset by a commensurate decrease in LWP, resulting in the same w_e irrespective of N".) I recommend to pay more attention to the validity of this assumption, in particular for some extreme values of N and LWP used in the study. Another concern is that the model framework builds on existing mixed layer model studies, but for the reader not familiar with this modeling technique it may be difficult to follow.

We thank the reviewer for the valuable comments that helped to clarify various aspects of our manuscript. Our answers are stated in green font, while excerpts from the (revised) manuscript are printed in blue font with italic characters. All references not explicitly stated in this letter can be obtained from the revised manuscript.

Main:

The conclusions of this study are "... built upon the assumption that an increase in entrainment rate (w_e) due to an increase in N is exactly offset by a commensurate decrease in L, resulting in the same we irrespective of N".

Please note that we will refer to this assumption as assumption (35) in the following.

The setup is somewhat unclear, and there may be inconsistencies, though I may be mistaken. For example, the surface fluxes are treated as constant, which seems to contradict the systematic changes in LWP. Since the boundary layer height is kept constant, the changes in LWP must stem from changes in humidity or temperature within the boundary layer, or both. I suspect that the thermodynamic profiles in the boundary layer are altered, as inferred from the dependence of the buoyancy jump (Delta b, line 183) on the settings. However, if the thermodynamic profiles in the boundary layer are modified, this would lead to changes in surface fluxes (see Bretherton and Wyant, 1997). Notably, most MLM studies cited in this paper (e.g., Wood 2007, Dal Gesso, Jones et al.) use surface boundary conditions dependent on wind speed and the difference between surface conditions and the air just above it.

First, we would like to clarify that did not use a <u>full</u> mixed layer model, which would be necessary to address some of the aforementioned effect. We only used the entrainment rate parameterization frequently used in mixed layer models, as stated in the abstract: *"In this study, we utilize a simple entrainment parameterization used in mixed-layer models to determine entrainment- mediated cloud-water adjustments in non-precipitating stratocumulus."* In 2.3, we further clarify that *"Using the mixed-layer model entrainment parameterization w_e outlined above, (35) is solved iteratively for δL using prescribed values of L, N , and δN , while keeping all* *other parameters constant."* We chose this this simplified framework to gain a better qualitative understanding of cloud-water adjustment, which in weakly and non-precipitating stratocumulus is believed to be mainly a result of changes in entrainment (e.g., Hoffmann et al. 2020).

To only rely on the entrainment parameterization, many parameters had to be assumed constant to provide a path forward to better understand the underlying physics. In particular, we do not aim for a quantitative understanding, for which more complex models, such as large-eddy simulations (LESs), are indeed necessary. In fact, the LESs of our companion paper (Chen et al. 2024) consider all the processes mentioned by the reviewer (a non-constant boundary layer depth, interactive surface fluxes), except shortwave radiation which we have studied in LESs in a previous study (Chen et al. 2024b).

One clear result of our companion paper is that the entrainment rate increases after an initial perturbation in the cloud droplet concentration N, but approaches the unperturbed value after approximately 18 h. This is shown in Fig. 4a of our companion paper (Chen et al. 2024a), which depicts a time series of the entrainment rate for different perturbations in the cloud droplet concentration. Figure 2a of our companion paper (Chen et al. 2024a) indicates that this return of the entrainment rate to its unperturbed state is mainly due to a decrease in the liquid water path. Thus, we believe that the idealization that the entrainment rate of a perturbed case assumes its unperturbed value due to a decrease in liquid water is sufficient to understand this system better.

Overall, this idealization (35) can be considered a corollary of the feedback mechanism between entrainment rate and liquid water path originally suggested by Zhu et al. (2005) (see their Fig. 7), which has been also highlighted by Wood (2012) as a major mechanism responsible for the observed stability of stratocumulus (see their Fig. 26). Although there is certainly more nuance, e.g., in the variability of surface fluxes or the boundary layer depth in response to an aerosol perturbation, our simple model is sufficient to capture the main response of non-precipitating stratocumulus to perturbations in the aerosol concentration. Thus, idealizing the underlying system to focus on the main drivers of cloud-water-adjustments in weakly and non-precipitating stratocumulus can yield reasonable and – above all – new insights. We amended our last paragraph accordingly: *"All in all, this study showed that even comparably simple models, such as the one used here, can be applied to increase our fundamental understanding of aerosol-cloud interactions. In fact, the simplicity of the applied model allowed us to directly link cause and effect of cloud-water adjustments, which can be difficult in more complex models such as global circulation or even large-eddy simulation models due to confounding factors (Mülmenstädt et al., 2024). That being said, the nuance provided by these models should not be disregarded as they help to quantify the effects that have been neglected here (e.g., interactive surface fluxes, changes in boundary layer depth, or the diurnal cycle of solar radiation). Thus, we advocate that the assessment of aerosol-cloud interactions should balance the use of complex and simple approaches by substantiating quantitative understanding with qualitative insight."*

Moreover, we added the following statement to Section 2.3.2 on the model's base assumptions: *"Additionally, (35) does not consider adjustments in surface fluxes (Bretherton and Wyant, 1997), as well as the impact of solar radiation (Chen et al., 2024b), for which more complex models would be required. Nonetheless, the large-eddy simulations presented in our companion* *paper (Chen et al., 2024a) indicate that sufficient physics are captured in (35) to yield reasonable insights, as we will detail below."*

A particularly confusing sentence appears on line 232: "The decrease in Delta b is due to the stronger latent heat release at higher L, which decreases the temperature difference relative to the warmer free troposphere, as indicated by (11), enabling stronger entrainment." Here the factor Delta b (the vertical static energy jump across the inversion) depends on the inversion jumps of temperature and humidity, yet the sentence suggests that the boundary layer is warming (i.e., "decreasing the temperature difference", as stated). If this is the case, shouldn't the surface fluxes also change?

As stated above, we do not use a full mixed layer model, which would be necessary to address this effect. We only use an entrainment rate parameterization frequently used in mixed layer models, as stated in the abstract: *"In this study, we utilize a simple entrainment parameterization used in mixed-layer models to determine entrainment- mediated cloud-water adjustments in non-precipitating stratocumulus."* In 2.3, we further clarify that *"Using the mixed-layer model entrainment parameterization w_e outlined above, (35) is solved iteratively for δL using prescribed values of L, N, and δN, while keeping all other parameters constant."* As already outlined above, we believe that this simplified framework is necessary to gain a better qualitative understanding of cloud-water adjustment, while more nuanced answers would require more sophisticated approaches, e.g., a full mixed layer model or large-eddy simulations, creating a commensurately more difficult path forward to understand aerosol-cloud interactions.

In Section 2.3.2 it is unclear whether an equilibrium state is assumed? In any case the boundary layer depth h_t is assumed to be constant, and this leads to Eq 35, w_e(N,LWP)=w_e(N+dN,L+dL), with w_e being the entrainment velocity. The question arises whether Eq. 35 holds for large deviations dN and dL?

While this has not been explicitly studied, we are also concerned that large deviation in N and L should not be assessed: *"Note that (35) describes a condition that is assumed to be valid in addition to other changes affecting L and N. Since (35) is only valid after sufficient time has elapsed (18h) (Chen et al., 2024a), stratocumulus that exhibit faster changes in L and N should not be assessed using (35). This might be the case for stratocumulus that are far from their steady state L (Hoffmann et al., 200 2020; Glassmeier et al., 2021; Hoffmann et al., 2024b)."*

It is not explicitly stated whether the key assumption of a zero entrainment response to changes in N to minimum and maximum values applied in the study have been tested with the LES, for example for some extreme values of N, say 10 and 1000 cm-3, and for L = 10 or 1000 g/m2? Perhaps the reader could be directed to relevant sections in the accompanying paper.

The assumption (35) has only been applied to liquid water paths between 10 and 100 g m^{-2} and droplet concentrations between 100 cm^{-3} and 1000 cm^{-3} *"Figure 3 shows $m \equiv d \ln (L)/d \ln (N)$ as a function of N for two different L values: L = 100 g m^{-2} (continuous lines) is representative for most stratocumulus (e.g., Wood, 2012). L=10 g m^{-2} (dashed lines) is representative for optically thin stratocumulus, which reflect markedly less solar radiation, but cover substantial regions of the globe (e.g., Leahy et al., 2012). Moreover, the emission of longwave radiation for L = 10 g*

m^{-2} is not saturated, enabling the potential for different cloud water adjustments than at higher L. In the following, the case analyzed above (the default) is indicated by black lines, while altered setups are indicated by colored lines. For clarity, we will focus our discussion on how m changes between N = 100 and 1000 cm^{-3}, typical values that circumscribe weakly and non-precipitating stratocumulus" While these values slightly exceed the range of values tested in our companion paper (Chen et al. 2024a), in which liquid water paths in the range of 40 to 100 g m^{-2} and droplet concentrations between 100 and 400 cm^{-3} are analyzed (see their Fig. 1), the assumption (35) is probably valid for the largest part of the phase space shown in Fig. 3.

The assumption of constant entrainment is special, as other perturbations, such as changes in surface temperature or free tropospheric conditions, would likely lead to a nonlinear entrainment response. For instance, De Roode et al. (2014) examined the entrainment response to changes in large-scale conditions and found that the entrainment response significantly altered the feedback strength.

While it is interesting to look at other perturbations, those are not in the scope of this study. This study aims to develop an understanding of the processes determining the slope of cloud-water adjustments in weakly and non-precipitating stratocumulus, i.e., feedbacks caused by perturbations in the cloud droplet concentration, which are believed to be primarily caused by a response in the entrainment rate (e.g., Hoffmann et al. 2020). Please note that we investigate how the free-tropospheric moisture effects cloud-water adjustments in Section 3.3.

Additionally, numerous studies using LES models show that changing cloud droplet concentration has a strong impact on entrainment and I am not sure whether those results are in line with the assumption of w_e(N,LWP)=w_e(N+dN,L+dL). The implications of the latter condition warrant a more critical discussion.

We agree that an aerosol perturbation results in an initial increase in the entrainment rate, as stated in our manuscript: *"The large-eddy simulations in our companion paper (Chen et al., 2024a) show that a positive perturbation of N , $\delta N > 0$, results in an increase in w_e in response to an aerosol perturbation (see their Fig. 4a). After sufficient time (18 h), this increase in w_e is diminished, resulting in negligible differences in w_e among the perturbed and unperturbed simulations."* Moreover, we state that *"This study was built upon the assumption that an increase in entrainment rate w_e due to an increase in N is exactly offset by a commensurate decrease in L, resulting in the same w_e irrespective of N. This idea is based on large-eddy simulations presented in our companion paper (Chen et al., 2024a) (see their Fig.4a), and can be considered a corollary of the w_e-L feedback mechanism suggested by Zhu et al. (2005) (see their Fig. 7)."* This mechanism has been highlighted by Wood (2012) as being responsible for the observed stability of stratocumulus (see their Fig. 26). Thus, our assumption (35) is not only in line with the large-eddy simulations presented in our companion paper, but also based on our general understanding of stratocumulus-topped boundary layers.

It is difficult to read for non-experts. It could help to start with stating upfront that you will apply a summation of the individual fluxes. An explanation of the MLM, its setup and some of its results in a figure would be helpful, for example like Fig. 11 from the MLM paper by Nicholls (1984) or Figure 4 by Bretherton and Wyant (1997). As a reason, I am not able to derive whether the total fluxes of the conserved variables are linear in height? Actually, one would expect them to be constant with height, as this implies a steady state.

We do not use a mixed layer model. We only used the entrainment rate parameterization frequently used in mixed layer models, as stated in the abstract: "In this study, we utilize a simple entrainment parameterization used in mixed-layer models to determine entrainment-mediated cloud-water adjustments in non-precipitating stratocumulus." In 2.3, we further clarify that "Using the mixed-layer model entrainment parameterization w_e outlined above, (35) is solved iteratively for δL using prescribed values of L, N , and δN , while keeping all other parameters constant (e.g., surface fluxes, boundary layer depth)." To use the entrainment rate parameterization, we have to determine the integrated buoyancy flux, as outline in Section 2.2.2, with some ideas related to mixed layer modeling. These calculations are based on existing literature, extensively cited in Section 2.2.2.

"Under well-mixed conditions, contributions to the buoyancy flux that originate from the surface or the top of the boundary layer can be assumed to increase or decrease linearly within the boundary layer". Note that linear flux profiles are only valid for moist conserved thermodynamic variables. This is now stated only implicitly. If the fluxes of conserved variables are linear with height this means that the shape of the vertical profile of the mean state is constant in time. Well-mixedness is not a necessary constraint here: d/dz d/dt X_mean = d/dt d/dz X_mean = - d/dz d/dz w'X' = 0 for a linear flux profiles (the term in the middle indicates the shape of the vertical profile is constant in time).

Our assumption (35) does not require the mixed layer to be in a steady state. (Although we assume the boundary layer depth and other parameters to be constant to simplify our calculations.)

The discussion on optically thin clouds is interesting. I would like to mention the works of Stephens (1978) or Slingo et al. (1982) who showed the relation between the LWP and the downwards emissivity being discussed in the study. It would also be nice to mention that this regime is commonly present, which would strengthen the discussion, for example Leahy et al. (On the nature and extent of optically thin marine low clouds, JGR, 2012).

We have amended the following statement: *"Figure 3 shows $m \equiv d \ln (L)/d \ln (N)$ as a function of N for two different L values: $L = 100$ g m^{-2} (continuous lines) is representative for most stratocumulus (e.g., Wood, 2012). $L=10$ g m^{-2} (dashed lines) is representative for optically thin stratocumulus, which reflect markedly less solar radiation, but cover substantial regions of the globe (e.g., Leahy et al., 2012). Moreover, the emission of longwave radiation for $L = 10$ g m^{-2} is not saturated, enabling the potential for different cloud water adjustments than at higher L."* We thank the reviewer for reminding us about Stephens (1978), which has been added the revised manuscript: *"[…] where the first term on the right-hand side describes the radiative cooling across the boundary layer, which is scaled by $[1 - \exp(-\kappa_r L)]$ to consider the saturation of longwave radiative cooling toward larger L (e.g., Stephens, 1978)."*

One additional issue to consider is that thin clouds may appear broken, in which case the assumptions of the MLM may break down. This possibility should be discussed further.

Broken stratocumulus are typically associated with decoupling, which is discussed in the manuscript: *"The stippling marks potentially decoupled boundary layers, where the buoyancy flux is too weak to ensure a well-mixed boundary layer. These regions have been determined using the approach by Turton and Nicholls (1987). Reasons for the decoupling will be discussed more deeply when addressing* $\langle B \rangle$*. As decoupled boundary layers violate many assumptions reasonable for well-mixed boundary layers, this part of the phase space should not be assessed."*

Deardorff's entrainment relation (7) parameterization. Note that Van Zanten et al. (1999) write about Deardorff's (1976) constant: "The value of Add1 is not constant (we found an order of magnitude variation) with respect to all types of convective boundary layers, so the closure is rejected.". Perhaps the equation that introduced the factor k* can be omitted as it is not applied directly in the study, but instead the other entrainment efficiency factor A, which is not constant (Eq. 25).

We have adapted the text as follows: *"Typically,* w_*^3 *is related to the vertically integrated buoyancy flux* $h_t\langle B \rangle$ *[…] such that* $w_*^3 = A\, h_t \langle B \rangle$*, with an efficiency factor A (Deardorff, 1976; vanZanten et al., 1999)."* Followed by minor adjustments around (25).

A weak point of the study is that solar radiation is not taken into account. Solar radiation constitutes a key radiative forcing. The absorption of solar radiation strongly impacts cloud dynamics, as it reduces the effect of longwave radiative cooling. I can imagine that it may have an impact on the results.

We agree that solar radiation has an important impact on the development of stratocumulus, as we have studied previously (Chen et al., 2024b). However, considering solar radiation and its diurnal cycle will cause a time dependency in (35), which for now would complicate our main intent to understand cloud water adjustments. We now state explicitly that *"Similar to our companion paper (Chen et al., 2024a), we neglect interactions with solar radiation for simplicity." and "Additionally, (35) does not consider adjustments in surface fluxes (Bretherton and Wyant, 1997), as well as the impact of shortwave radiation (Chen et al., 2024b), for which more complex models would be required."*

Fig. 3c analyzes the influence of free tropospheric humidity on the feedback factor m by halving or doubling the value of the humidity jump across the inversion. Line 375 summarizes the findings: "Further, we showed that increasing free-tropospheric humidity strengthens negative cloud-water adjustments, in contrast to modeling by Glassmeier et al. (2021), but in agreement with Chun et al. (2023) and our companion paper (Chen et al., 2024a)." Dussen et al. (2015) also found that the inversion humidity jump strongly affects the (equilibrium) LWP and inversion height. With respect to the latter I have difficulties to understand how the experiment in the study was set-up. Is it just a matter of changing the humidity jump Dqt in the entrainment parameterization while keeping the rest the same?

Exactly. As we are only working with assumption (35) and hence only the entrainment parameterization, the humidity jump and the boundary layer depth are changed in the entrainment parameterization while keeping all other parameters the same. We now state: *"To address this, we vary* h_t *(red and green lines) and* Δq_t *(blue and orange lines) by halving or doubling their default values in (35), while keeping all other parameters the same."* The reference to Dussen et al. (2015) is very interesting. However, that study assumes a constant droplet concentration and therefore does not enable any insights on the cloud water adjustments studied here.

De Roode, S. R., Siebesma, A. P., Dal Gesso, S., Jonker, H. J., Schalkwijk, J., & Sival, J. (2014). A mixed-layer model study of the stratocumulus response to changes in large-scale conditions. Journal of Advances in Modeling Earth Systems, 6(4), 1256-1270.

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Leahy, L. V., R. Wood, R. J. Charlson, C. A. Hostetler, R. R. Rogers, M. A. Vaughan, and D. M. Winker. "On the nature and extent of optically thin marine low clouds." Journal of Geophysical Research: Atmospheres 117, no. D22 (2012).

Vanzanten, M. C., Duynkerke, P. G., & Cuijpers, J. W. (1999). Entrainment parameterization in convective boundary layers. Journal of the atmospheric sciences, 56(6), 813-828.