Response to Reviewer 2

This study heuristically devised a simple model describing the behavior of liquid water path (L) as a function of cloud droplet number concentration (N) with its parameters adjusted to previously performed LES simulations and perturbed to explore the system sensitivity. The simple model is also used to analyze how the system behaves depending on external forcing of L and N. As the main framework of this study, these analyses are performed in terms of the logarithmic sensitivity parameter of L with respect to N where the precipitation and thermodynamics controls are identified and linked with each other. This is a very interesting study that provides a useful process-level insight into the cloud water adjustment to aerosol perturbations. I only have some relatively minor comments (listed below) that mainly require further clarifications of some model setup. I would recommend the manuscript be published after these comments are appropriately addressed.

We thank the reviewer for the support of our study, and the comments that helped to remedy unclear parts of the manuscript.

Specific points:

Line 27, Line 166: What is "inverted v"? I cannot find the description of "v".

The 'inverted v' refers to the shape of L caused by increase for lower N, followed by a decrease for higher N. However, we have removed any references to 'inverted v' in the revised manuscript.

Line 166: Likewise above, what is "regular v"?

With 'regular v', we are refereeing to the case in which L increases for lower N, followed by a stronger increase for higher N. However, we have removed the sentence mentioning 'regular v' in the revised manuscript.

Line 65: Insert "Based on (2)" prior to "The thermodynamic carrying capacity is...".

Following the reviewer's suggestion, we changed the sentence to: "The thermodynamic carrying capacity is derived from (2), and expressed as […]"

Line 90: "The source S_N has been neglected for simplicity": Does this mean that N is monotonically decreasing with time during the time integration according to (6)? If so, N should not reach the steady state. Please explain what happens with temporal evolution of N in this computational setup.

Yes, N is monotonically decreasing. We amended the revised manuscript as follows to address the reviewer's comment: "The individual simulations (gray lines in Fig. 1a) show substantial motion in the L direction, while motion in the N direction is only relevant at low N < 100 cm^{-3}

due to precipitation scavenging and at high $N > 1000\ cm^{-3}$ due to Brownian coagulation. Although $S_N = 0$ and hence $dN/dt < 0$ everywhere in the phase space, a stable population of simulations persists between these limits for at least the 7 days of simulated time considered (brown dots). [Baker and Charlson (1990) showed how the consideration of a $S_N > 0$ could offset the losses in N, causing N_∞ steady states.] In the L direction, these simulations approach a steady state L_∞ […].”

Line 106: “Without N dynamics”: Does this mean that only (3) is used without (6)? Please clarify.

Exactly. We have clarified the revised manuscript: “Solving only (3), i.e., without the N dynamics considered by (6), the steady state L_∞ exhibits very similar features to the previously discussed solution (Fig. 1b).”

Line 177: Would it be possible to write down the equation describing how the perturbation timescale (t_{prt}) comes into the perturbation added ($\Delta \ln(N)_{prt}$). It is unclear (at least for me) how the prescribed perturbation timescale is used in calculation of the perturbation.

This was indeed unclear and has been revised as: “In this study, perturbations are modeled as a Bernoulli process, and are applied with the probability $\Delta t/\tau_{prt}$ evaluated for every timestep of the model. Here, τ_{prt} is the perturbation timescale, which is varied from 20 min to 2 weeks.”