Thank you to both reviewers for their insightful comments. We have made major changes to the manuscript to address the reviewers' comments. Most significantly, we have:

- analyzed additional experiments with Nd = 50/cc and Nd = 100/cc to identify that our results do show signs of the inverted-V LWP response,
- performed an additional sensitivity experiment in which evaporation of precipitation is disabled to evaluate the sensitivity of the subcloud moistening/cooling on the cloud evolution,
- strengthened the referencing in the introduction.

Our point-by-point responses to all of the referee's comments are below. The referee comments are in black, and our responses are in blue text.

Response to Reviewer #2

MAJOR COMMENTS:

The discussion of the physical processes influencing the results is somewhat underdeveloped. For instance, in Figure 4, entrainment, coupling, and radiative heating are shown in different units, which makes direct comparisons between them challenging. While these processes can be qualitatively associated with the evolution of LWP, the relative importance of each remains unclear, weakening the overall argument. The role of precipitation is neglected. In particular, a relevant mechanism to consider is the evaporation of drizzle below the cloud base, which dampens buoyancy flux, weakens turbulence, and reduces entrainment. See the Introduction section in Uchida et al. (2010, doi:10.5194/acp-10-4097-2010) for a summary of this mechanism. How does this process influence the results presented in this study?

Than you for this comment. While we find it to be a very important point, it is not so obvious how to assess the impact of different processes in a credible way using the same units, because they can modify the system in a different way, on different time scales, and those non-linear interactions can accumulate over long times differently. Also, the way we present our results is one of possible approaches, used in many previous studies (e.g., Stevens et al. 2005, Sandu and Stevens 2011, van der Dussen et al. 2013, Bretherton and Blossey 2014, Chun et al. 2023, Chun et al. 2025).

However, to make the comparison more quantitative in terms of state variables, we have chosen an alternative approach, in which we run a set of sensitivity experiments accounting for all non-linear interactions, while adjusting a single process controlling the development of the system (see Tab 2 in the revised paper). Noteworthy, our set of sensitivity experiments includes the runs with precipitation strongly suppressed, as well as with the evaporation of precipitation disabled (the latter in the supplemental material). These tests help to better quantify the very impact of precipitation on the transition.

One major change, made with the potential reader in mind, is that we decided to switch from dlnF/dlnN to dF/dlnN. This change is reflected in the equations and figures, and the susceptibility is now expressed in units of W/m^2. We believe that presenting the results in terms of W/m^2 will make the results more approachable to the average reader.

As the authors noted, there have been many papers on the ACI in marine shallow clouds. It would greatly benefit the readers and strengthen and current paper if the authors can more clearly connect their new findings to the existing body of work. This is not about advertising any previous studiy, but about highlighting the novel aspects of the current paper through thoughtful comparison. For example, the diurnal LWP pattern, higher at night and lower during the day, is reminiscent of the results in Sandu et al. (2008), who proposed an explanation for this behavior. While a comprehensive literature review or exhaustive testing of all previous hypotheses is not necessary, a deeper and more explicit discussion of how this study builds upon or diverges from past work would be highly valuable.

In the introduction, we cite several previous studies that we are aware of that specifically address the diurnal cycle of the sensitivity of shallow clouds to aerosol perturbations. For example, the Sandu et al. (2008) study referenced by the reviewer was already cited on (now) line 75. We also added more citations to the text.

MINOR COMMENTS:

I found it concerning that some well-established ideas are cited using only recent publications, rather than the original sources. For example, citing Wall et al. for ERFaci and Hoffmann et al. (2023) for Eq. 6. Even a quick check of the cited papers would tell references to the original sources, which the authors should cite directly to properly acknowledge the historical development of these concepts.

Thanks. We have added a couple of the foundational references where appropriate. On line 20, we add reference to the AR5 IPCC report (Boucher et al., 2013) where the concept of ERF as opposed to RF was first socialized on a large scale across multiple forcings including for ACI and CO2.

Reference: Boucher, O. et al., 2013: Clouds and Aerosols. In: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex, and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 571–657, doi:10.1017/cbo9781107415324.016.

We add a classic reference (Brenguier et al., 2000) for the adiabatic cloud optical depth calculation but keep the Hoffman et al reference since there is a parameter choice which is needed and we use the specific parameter given by Hoffman. The text is changed as follows:

'*The cloud optical depth is calculated at each time step from the domain mean time-dependent modeled LWPc and Nc assuming an adiabatic cloud vertical structure (Brenguier et al., 2000) following the specific implementation of Hoffmann et al. (2023).*

Reference: Brenguier, J., H. Pawlowska, L. Schüller, R. Preusker, J. Fischer, and Y. Fouquart, 2000: Radiative Properties of Boundary Layer Clouds: Droplet Effective Radius versus Number Concentration. *J. Atmos. Sci.*, **57**, 803–821, https://doi.org/10.1175/1520-0469(2000)057<0803:RPOBLC>2.0.CO;2.

Please clarify the connection between Eqs. 7-9 and Eq. 1. Are these equations intended as proxies for specific terms in Eq. 1? Please state clearly.

Here we refer the reviewer back to Equation 1 where the three terms (S_N), S_{LWP}, and S_f are denoted underneath the curly brackets. We added a clarification at line 164 that we are referring to the terms in equation 1:

'The offline radiation calculations are used to decompose the ERFACI into the three indirect sensitivity terms defined in Eq. 1.'

We also corrected two problems with eqs. 7-9. First, we now write them in the same form as equation 1 and second, we also add an approximately equal sign to account for the fact that we are estimating the derivatives.

Line 149: Why does the current work focus only on two aerosol scenarios? More specifically, did the authors observe any indication of the previously reported "inverted-V" shape in the LWP-N relationship? If not, it is worth noting. This comment is not meant to cast doubt on the results, but rather to encourage a more complete discussion of their implications.

Thanks for this comment. We do see some evidence for the inverted-V phenomenon. We now comment on the "inverted-V" shape in the text (lines 48-50, 228-232-250, 308-310). We also included more results based on the N50 and N100 results as well. They are summarized in Tab. 1, where the susceptibility of different terms is calculated for the more polluted (N200-N100) and more pristine (N50-N50) sides.

Line 180: The term "similar" seems too vague here. Please be more specific about the particular features the authors intended to highlight.

Good point. This was vague. We have rewritten the sentence as follows (lines 214-216):

The other two cloud adjustment terms (S_{LWP} , S_f) have similar diurnal patterns, each having positive values in the morning and negative values in the afternoon that over the course of the diurnal cycle partially cancel the Twomey effect.

TECHNICAL ISSUES:

Line 129: It seems that LWP refers to the sum of LWPc (cloud LWP) and RWP. If this is the case, please state it explicitly.

That interpretation is not correct. The LWP is defined on line 27 as the grid mean cloud liquid water path: LWP = $f_c LWP_c$. The old Line 129 (now 163) and the following equations explicitly reference the LWP_c. No changes are made to the manuscript as these definitions are already very clear.

Line 169: Should this refer to panel e instead of d? Please double-check.

Thanks for catching this typo We have changed d to e.

Since Section 2 is titled "Methodology," Subsection 2.4 should be moved to a new section for clarity and consistency in the manuscript structure.

Thanks for catching this. 2.4 and its subsections 2.4.1 – 2.4.3 have now been changed to section 3 with subsections 3.1. – 3.3. The conclusions is now section 4.