Review of 'How meteorological conditions influence aerosol-cloud interactions under different pollution regimes'

This study uses WRF-Chem to simulate marine liquid clouds near the Eastern China Ocean during a selected winter period. The experiments are designed to examine aerosol effects on cloud and precipitation by comparing clean versus polluted scenarios. Results show that aerosols extend cloud lifetime in moist conditions but shorten it in dry conditions. In the polluted regime, continental aerosols lead to higher Nd and CLWP and smaller CER, while precipitation responses are mixed overall. Under the clean regime, aerosol activation is efficient and responses are clearer with supersaturation. In polluted conditions, clouds exhibit mixed responses tied to different cloud processes, reflecting regime-dependent ACI sensitivity.

While the overall narrative and logic read fine, I found multiple occasions where the interpretation and discussion are rather vague or misleading. As such, I have questions and comments on technical and interpretive details. The comments below include several general comments and more specific comments, laid out in the narrative sequence when reading the manuscript.

General Comments

In the Introduction, please situate this work within prior LES efforts (WRF-SBM, WRF-Chem, SAM, etc.) over the East China Ocean and synthesize what they concluded about ACI. Then clarify the remaining gap and why a chemistry-aware framework is needed here: specifically, what does WRF-Chem capture that a physics-only perturbation of aerosol number in WRF/WRF-SBM cannot? Because much of your analysis emphasizes dynamical/thermodynamic linkages, please consider discussing how chemical pathways modify those linkages and how your design advances beyond prior 'simple number-perturbation' studies.

Subsection 3.2, 'Experiment setup', should be moved into the WRF-Chem description or at least earlier in Section 3, preferably before Figure 2.

If I am understanding correctly, the middle columns in both Figs. 4 and 5 are from the Control experiment. If so, please clarify why the spatial distributions for Nd and CLWP in Fig. 5 differ noticeably from those in Fig. 4 (e.g., an empty area in Fig. 4 but not in Fig. 5, etc.), and why precipitation also exhibits subtle differences. Please ensure consistent sampling/calculation for region maps like these.

When describing figure results, please also list a plain-number expression alongside the scientific expression to help intuitive interpretation. For instance, at L402: ‘At sub-moderate supersaturation levels (…)’ include the normal expressed percentage values ($<10^{-0.9}\%$ → < ~0.12%).

Please carefully revisit whether the cited supporting figures actually show and support the associated statements. Some cases are included in detail comments below.

Specific Comments

L47. You may want to narrow the statement down to ‘satellite observation…’ given the reference cited.

L62. Please include a brief description of the following sessions of the manuscript.

L96. What is the uncertainties of this Nd retrieval compared with the in-situ observation.

L99. Please specify the (approximate) local hour of Terra overpasses over ECO.

L111. Please specify the matching logic (e.g., whether the WRF output at the nearest time step or an average over surrounding times was used). For a given WRF grid cell, approximately how many MODIS L2 records were averaged?

L126. How much uncertainty will these two different modeled Nd derivations introduce into the later ACI analysis? Can you show a comparison of calculated/simulated Nd vs. directly output Nd?

L141. Please specify the lat/lon range of the red box.

L152. Is there a specific reason for selecting 1300 m? Also, how does the model cloud-top height compare with the satellite CTH?

L152. Please correct me if I'm wrong, but by eye are LTS and RH exactly the same between the Control and Clean regimes, or are the differences too subtle to visualize?

L154. Do the variables shown in Fig. 2 represent domain means or cloud-sample means?

L155. This 'dominant-factor transition' statement needs physical-mechanism support. Otherwise, omit it and reframe as a description of wind direction (as in the next sentence), or simply state that the domain was impacted by cold-air advection from the northwest.

L162. …inhibiting updraft (Fig. S1i)

L163. I wouldn't say 'rapid' given the wind speed isn't significantly faster than in the LTS < 17 scenario.

L171. 'low-pollution' might be misleading, try 'moderate' or 'relatively low', given that 'clean' is used later.

L172. That is an ultraclean condition; I would like to see the corresponding Nd.

L173. Which case? Are you referring to the polluted or clean case as reflecting typical variations in the joint field over your study region? Please clarify. I recommend retuning this paragraph so the message is tied more tightly to this particular study if you want to keep these general statements.

L211. It looks like the Control has larger error bars (and slightly more outliers) than Control_NoDA, especially for the wind components. I recognize the better correlation in Fig. S2; however, can you quantify mean bias or RMSE between these two and observations to support your statement?

L240. Please also note in the caption that the lower subpanels are for the red-box domain.

L297. If you only present counts in Fig. 6m-o, I would not call it 'occurrence frequency', which usually meant for depict fraction or percentage. Suggest just use 'counts' or 'samples'. Or maybe you can get the actual fractional frequency of occurrence of clouds.

L301. Do you have a physical explanation for the increased cloud lifetime at the high-LTS tail (>24 K), which also corresponds to the lowest RH? I wonder if entrainment mixing is inhibited, given that Nd (CER) is comparatively lower (larger) at that tail.

L335. It will not be intuitive to say 'more large droplets,' since the 75th percentile of CER under Control is still largely smaller than under Clean. Consider: 'collision–coalescence at lower levels can produce large droplets.'

L336. Have you checked cloud fraction and sample size vertically? In the Increase_ scenario, the ~3 km samples appear fewer (narrower spread), so precipitation enhancement may stem from different cloud layers (and not only from 'vertical development of the cloud layer'). If so, please make this clear.

It would help to examine vertical cross-sections of clouds (Nd, CER, Nr, etc.) for both scenarios to confirm. Since Fig. 4 suggests regional clustering for Increase_/Decrease_ samples, a SW–NE slice could work, please consider this.

Figure 8. The legends (e.g., Increase/Decrease_CER) are misleading. Consider renaming to clearer identifiers, e.g., CER (increased-precip samples).

L364. The correlation is significant but moderate; what message is this intended to convey? Because you did not show the relative vertical position of 950 hPa cold-air advection versus the cloud layers, the physical interplay is not determinate. I guess you can remove this...

L365. Can you remind me where did you show the Na relationship to LTS? Are you referring to Fig. 9a? And please add the support figure indentifer to this two statements.

Figure 10. Please clarify the units in panel (a). The in-cloud Na magnitudes in both experiments seem to far exceed the number concentrations shown previously (e.g., Figs. 2, 5). If you are showing an aerosol size distribution, the y-axis should be dN/dlogDp (cm^{-3}).

Also, what is the relationship between activated aerosol Naa and cloud-droplet Nd in the model outputs? Please elaborate.

L369. Fig. 10b provides limited support for this statement (no CLWP/RLWP shown). Please elaborate and/or provide a clear reference.

L377. At least CER and RWP do not increase steadily with RH. Please provide statistical support for 'stronger sensitivity,' e.g., correlations or regression slopes of cloud properties vs. LTS/RH.

L379. Please elaborate. If activation ratio, CLWP, and RWP largely increase with Na across RH ranges, how is 'greater sensitivity' exhibited?

L380. Regarding the 'manner' of cloud vs. Na being similar to high LTS (I presume Fig. 9b, 9f, etc.): would that mean cloud properties are largely impacted by aerosol loading rather than environmental parameters? If so, what 'dominated role' is cold-air advection playing? You have not established a strong physical relationship between cold advection and cloud (a correlation between column-mean Na and 950 hPa temperature is not enough). If you want to discuss the physical role of cold advection, at least show the vertical dependence of cloud properties on it. Please elaborate and provide figure support.

L392. The RWP sharply decrease with Na (> $10^{-0.4}$) under low supersaturation, which reflects the precip suppression effect of aerosol, please spell it out.

L406. ~0.16% supersaturation is not necessarily 'high' condition.

L407. ~0.01% supersaturation is fairly low, again, describing the supersature with a normal expression of number will give the reader more sense.

L410. As above, please further explain the physical basis for cold-advection dominance. This statement is somewhat misleading: updraft strength has limited direct relationship to aerosol concentrations. Please specify which cloud process is dominated by updraft under low-aerosol conditions and how that dominance is dampened under high-aerosol conditions. In which experiment would cold-advection effects be more effective?

L411. Provide statistical evidence or figure support for “At moderate Na, both updrafts and cold advection are weak…”

L449. You may wish to point out that the extensions of cloud lifetime (actually cloud frequency in this study; Fig. 6) at low and high LTS arise from different physical reasons: low LTS/vigorous updrafts invigorate frequent cloud hydrometeors, whereas high LTS/stronger subsidence maintains the cloud layer.

L482. I would like to see a revised Conclusions section after you address the main-text comments.