## Reply to Referee Comment 1

This study uses large-eddy simulation to investigate the effects of aerosol and droplet microphysical processes on the life cycle and properties of radiation fog in the Po Valley, Italy. A suite of sensitivity experiments is conducted to systematically evaluate the impacts of aerosol physical and chemical properties, alongside droplet microphysics, on key fog characteristics—including fog formation and dissipation, atmospheric visibility, droplet number concentration, and droplet sedimentation velocity. Overall, the manuscript is well-structured, logically organized, and comprehensive in content, exhibiting good general quality. Nevertheless, the physical mechanisms underpinning several of the reported results lack sufficient in-depth discussion. I therefore recommend a Major revision, with a specific focus on strengthening the physical interpretation of the findings to further enhance the rigor and logical coherence of the manuscript.

Reply: We thank the reviewer for the constructive and valuable feedback. The manuscript has been revised in agreement with the comments. In particular, we have clarified the distinction between non-activated hydrated and activated particles, specified the criteria used to identify interruptions during fog periods, and strengthened the explanation of the physical mechanisms underlying the simulation results. Please find our replies and the corresponding modifications below.

1. Line 113, “Savre et al. 2014” should be “Savre et al., 2014”.

Reply: We have corrected *“Savre et al. 2014”* to *“Savre et al., 2014”* (~Line 129 in the revised manuscript).

Line 184, the unit of the “standard deviation to 1.9”

Reply: We have corrected this to *“the dimensionless geometric standard deviation to 1.9”* (~Line 201).

2. Section 2.1:

1) It is recommended that the authors add an introduction to the observational instruments. In particular, the measurements of aerosol size distribution, aerosol chemical composition, and fog droplet spectrum in the experiments should at least be described.

Reply: We have added a table entitled *“Overview of the main quantities and instruments from the FAIRARI campaign 2021/22”* in the updated Supplementary Information, which summarizes the meteorology, visibility, aerosol and droplet measurements in our project. This addition is included in Section 2.1 of the main manuscript as: *“The measured quantities and corresponding instruments used in this study are summarized in Sect. S1.”* (~Line 92).

2) The method for calculating hygroscopicity from aerosol chemical components should be stated (ZSR?).

Reply: Thank you for making us aware of this omission. We have now stated clearly that we use the ZSR method: *"The bulk median hygroscopicity parameter κ of the mixture, calculated using the Zdanovskii–Stokes–Robinson (ZSR) method (Stokes and Robinson, 1966), is approximately 0.45 during the fog events, indicating moderately hygroscopic aerosols."* (~Line 104).

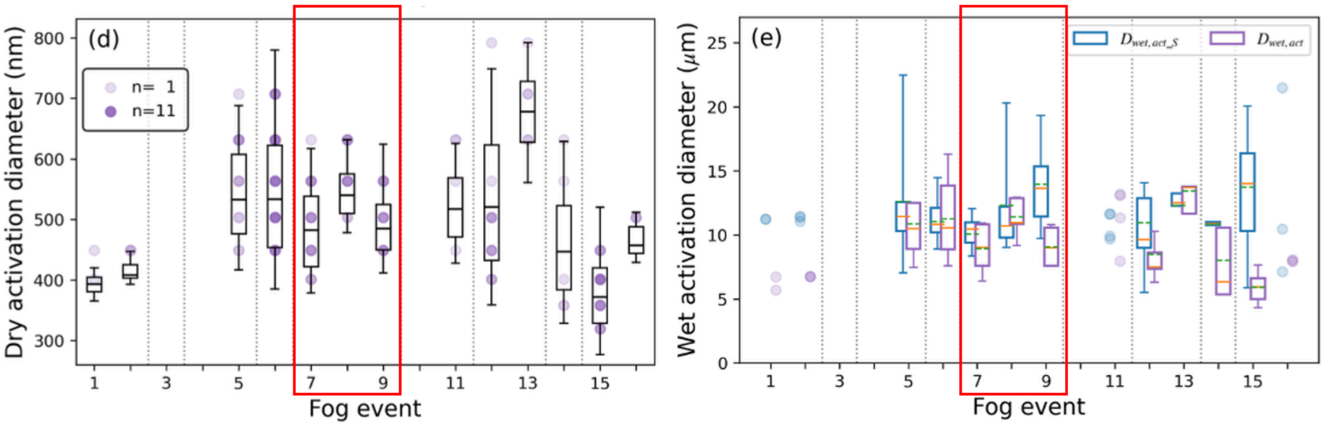
3) The term "hydrated particles" cannot intuitively represent unactivated haze particles, and it is recommended to use "unactivated particles" or "non-activated particles" directly.

Reply: In Section 2.1, we have clarified this as: "*The relatively high κ promoted hygroscopic aerosol growth and the formation of non-activated hydrated particles (hereafter referred to as hydrated particles)*" (~Line 106).

4) Lines 105 to 108: The authors regard the first peak with diameters less than 10 µm in the droplet size distribution as unactivated particles, which I find hard to agree with. Based on Figures 1 and 3, I am more inclined to treat the peak of the first bin (possibly the 2–4 µm bin) as unactivated particles, given the magnitude of water vapor supersaturation estimated in previous studies on radiation fog (e.g., Shen et al., 2018,https://doi.org/10.1029/2018JD028315; Wang et al., 2021, https://doi.org/10.1007/s11430-020-9766-4; Mazoyer et al., 2019, https://doi.org/10.5194/acp-19-4323-2019). Alternatively, the authors are requested to provide a more robust justification for regarding particles with diameters less than 10 µm in the droplet size distribution as unactivated particles.

Reply: We calculated the number of activated droplets and corresponding critical activation size using two approaches (Please see also Neuberger A. et al., 2025, https://doi.org/10.5194/egusphere-2025-5419 for a detailed explanation). The first approach followed Hammer et al. (2014) (https://doi.org/10.5194/acp-14-10517-2014), where the ambient activation diameter, *D_wet,act_S*, was calculated using the ambient size distribution measured by the GFAS and taking its first local minimum from the right; Droplets larger than *D_wet,act_S* were counted into the activated droplet category *CDNC_act*. The second approach employed a closure analysis between dry particles measured by the DMPS and the droplet size distribution from the GFAS. Assuming that dry particles activate starting from the largest ones, we used the wet number concentration of activated particles, *N_GFAS(D_wet > D_wet,act_S) = CDNC_act*, and summed over the dry number size distribution until the diameter *D_dry,act* at which a closure was reached with the ambient number concentration: *N_DMPS(D_dry > D_dry,act)*, and the associated wet activation diameter *D_wet,act* was then calculated using κ-Köhler theory. The results are shown in the figure below (adapted from Figure 4 in Neuberger A. et al., 2025), with fog events 7–9 selected in our study.

Moreover, following the reviewer's suggestion, we also compared our Po Valley fog cases with the Paris radiation fog measurements reported by Mazoyer et al. (2019). For our Po Valley fog, the median critical supersaturation was 0.0129%, and the corresponding median dry and wet activation diameters were approximately 500 nm and 9.17 µm, respectively. In contrast, for Paris fog, the median critical supersaturation was 0.0430%, with median dry and wet activation diameters of 390 nm and 3.79 µm, respectively.



Based on the results above, we consider that adopting a wet activation diameter of approximately 10 µm is reasonable. Accordingly, we have extended the explanation in the manuscript (Section 2.1, ~Line 111) regarding the distinction between observed non-activated hydrated particles and activated droplets.

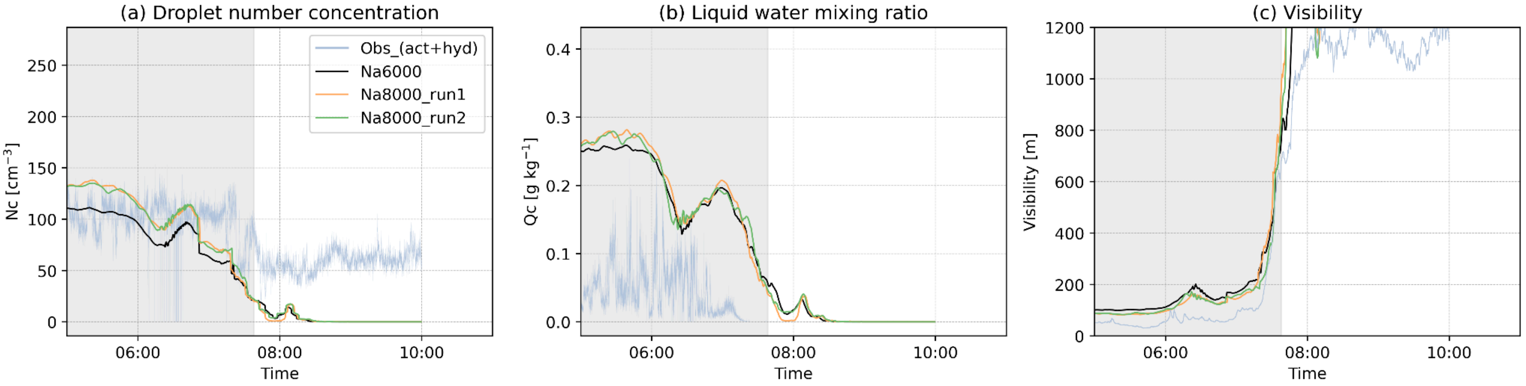
5) Lines 108–110: The authors state that there were two brief interruptions during the fog event, due to the rapid decrease in large droplets and LWC. However, the identification of the second interruption is confusing to me. It is not obvious from Figure 1a that it satisfies the author's criteria of "rapid decrease in large droplets and LWC". Could the authors please further clarify the criteria for identifying the second interruption, or provide a unified quantitative definition/indicator for "interruption"?

Reply: The interruptions were identified as periods that did not meet the fog criteria (Visibility ≤ 1 km and RH ≥ 90%, please see also in Neuberger A. et al., 2025, https://doi.org/10.1175/BAMS-D-23-0166.1). Moreover, a clear decrease in large droplets and LWC can be observed only in the first interruption. We have corrected the sentences to avoid confusion. (~Line 123)

3. Lines 317 to 319 & Line 343:

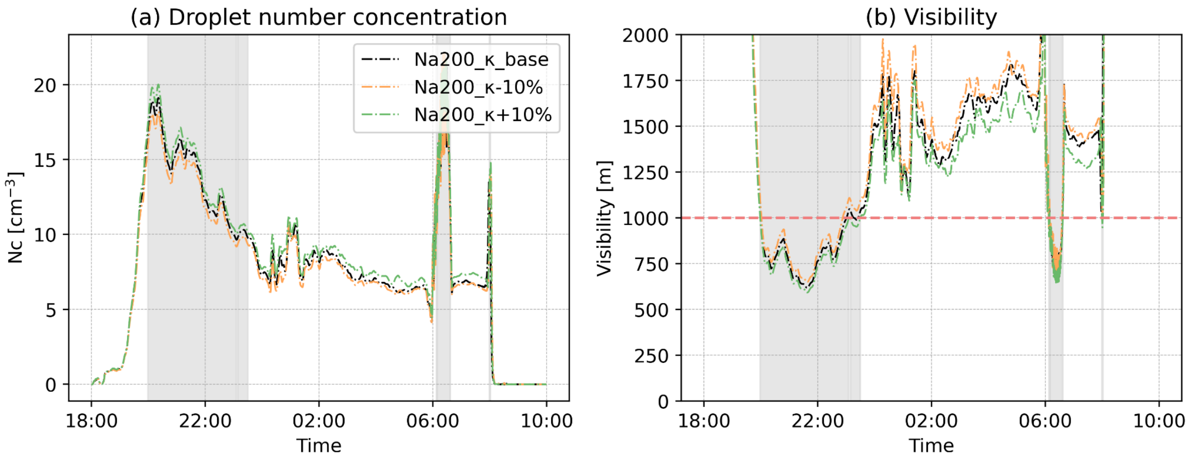
When Na = 8000 $cm^{-3}$, the simulated fog formation and dissipation times do not exhibit consistent characteristics. If the authors could further explore the possible reasons for this behavior, it would help make the analysis more rigorous.

Reply: For Na <14,000 $cm^{-3}$, the fog lifetime generally increases with Na, except for the case with Na = 8,000 $cm^{-3}$. Compared to the case with Na = 6,000 $cm^{-3}$ (dissipation time 07:45), the fog at Na = 8,000 $cm^{-3}$ dissipates slightly earlier, by about 7 minutes (~07:38), but this difference is not significant. The fog properties at Na8000 compared to Na6000 follow the general trend with increasing Na: the mean total cloud droplet number concentration Nc is 145 $cm^{-3}$, the mean liquid water content Qc is 0.24 g/kg, and the mean visibility is 110 m. In comparison, for Na6000 , the mean Nc is 122 $cm^{-3}$, Qc is 0.22 g/kg, and visibility is 129 m. The slightly earlier dissipation is mainly attributed to the faster decreasing rate in Nc and Qc during the dissipation phase, which accelerates the visibility increase above the 1 km threshold used to identify fog dissipation (see figure below). Moreover, we suggest that this behavior is also related to the random perturbations (in thermodynamical variables such as potential temperature), imposed in MIMICA during the simulation initialization. To test this, we ran a duplicate simulation for Na8000 (labelled as run2 in the figure below) using the same configuration and source code as in the original Na8000 simulation (run1). The results show that small random perturbations in the model can indeed influence the precise determination of fog lifetime: for example, the dissipation time is delayed by 3 minutes (07:41) in run 2 compared to run 1 (07:38), and the mean visibility increases by 1 m (to 111 m). We have added this explanation in the revised manuscript in Section 3.2 (~Line 347).



A similar issue exists for the exception case of *Na* = 200 cm$^{-3}$ mentioned in Line 343, and further explanation is also recommended.

Reply: When calculating the relative changes of Nc and Vis compared with the baseline κ simulations, we selected fog periods that meet the criterion Visibility ≤ 1.0 km and that are common in the two simulations. For the case of Na = 200 cm$^{-3}$, the visibility is generally very high, with only very short periods meeting the fog visibility criterion (shown as the gray shading in the figure below). Consequently, most data points with large relative changes of Nc and Vis are filtered out. This issue does not occur starting from the next Na level (Na = 600 cm$^{-3}$). Therefore, Na = 200 cm$^{-3}$ is an exceptional case that represents clean conditions with only short periods of fog, and where the "statistical responses" of activated Nc and corresponding Vis to κ perturbation during the periods of fog are weaker. We have added the explanation in the revised manuscript in Section 3.3 (~Line 379).



4. Lines 328 to 329:

It is recommended that the authors further discuss the possible physical mechanisms behind this result to strengthen the interpretation of the findings.

Reply: We have expanded the interpretation of the physical mechanisms behind the nonlinear response of fog lifetime to Na. Specifically, under highly polluted conditions (Na > 12000cm$^{-3}$), aerosol activation transitions from an aerosol-limited to a supersaturation-limited regime. Ambient supersaturation declines and stabilizes, which inhibits efficient activation of additional aerosols, leading to minimal changes in droplet number. Meanwhile, with limited condensable water vapor, the mean droplet size varies little with Na, and correspondingly, the sedimentation rate changes only little. Given that the effects on both condensational growth and sedimentation are small, the response of visibility and fog lifetime to increased Na becomes small. We have added the explanation in the revised manuscript in Section 3.2 (~Line 355).

5. Figure 9 & Figure 13:

Two sets of curves are shown in panel (a), but the legend does not currently distinguish between the fog top and fog base. It is recommended that the authors clearly indicate this information in the figure caption or legend to avoid ambiguity.

Reply: Different line styles have been adopted to distinguish the fog top and fog base in Figure 9 and Figure 13, and the figure captions have been updated accordingly.

6. Lines 359 to 362:

The sentence attributes the asymmetry to changes in the hygroscopic growth factor associated with variations in $\kappa$. However, the explanation mainly describes why increasing $\kappa$ leads to lower visibility (and vice versa when $\kappa$ decreases), without clearly addressing how this mechanism results in the asymmetric behavior highlighted in the text.

The authors should clarify how this mechanism specifically leads to the asymmetric response and provide a more detailed explanation to support the statement more effectively.

Reply: We have provided a more detailed quantitative description of this mechanism in the revised manuscript in Section 3.3 (~Line 398): *"This asymmetry arises not only from the nonlinear dependence of aerosol hygroscopic growth and activation on $\kappa$ perturbations but also from the characteristics of the aerosol size distribution. Taking Na = 12,000 cm$^{-3}$ as an example, a 10% increase in $\kappa$ relative to the baseline reduces the critical activation diameter, resulting in an approximate 4.0% average increase in activated droplets. At the same time, the hygroscopic growth factor increases by 4.9%, shifting the wet aerosol spectrum toward larger sizes. Because particles in the accumulation mode are more highly concentrated at smaller sizes, as seen from the lognormal distribution, a large number of fine particles originally just below the 2 μm threshold can exceed it under enhanced hygroscopic growth and are counted into the total Nc. This results in a substantial increase in total Nc (+27.1%) and, consequently, a sharp decrease in visibility (−14.6%), given the strong negative* *correlation between visibility and Nc. In contrast, a 10% decrease in κ leads to an average reduction in the activated droplets of about 5.3% and lowers the hygroscopic growth factor by 5.1%. Given the rapid decline of aerosol number concentration with increasing particle size in the accumulation mode, the wet size distribution resulting from hygroscopic growth scaled by the bulk growth factor retains the same decreasing trend. As the spectrum shifts toward smaller wet sizes with a lower growth factor, only a few particles above the 2 μm threshold fall below it and are excluded from the total Nc. As a result, the reduction in total Nc is relatively limited (−7.8%), and so is the corresponding increase in visibility (+10.6%).”*

7. Lines 52 to 65:

Regarding the shape of the cloud/fog droplet size distribution, the following studies could serve as valuable references:

Wang et al. (2023, https://doi.org/10.1029/2022JD037514) and Zhang et al. (2025, https://doi.org/10.1029/2024GL111643; 2026, https://doi.org/10.1029/2025MS005410) developed a novel parameterization for cloud/fog droplet size spectra, and further investigated its impacts on fog microphysical processes, sedimentation characteristics and optical properties via WRF-LES simulations.

Reply: We thank the reviewer for pointing out these studies to us. We have now cited the study of Wang et al. (2023, https://doi.org/10.1029/2022JD037514) in the introduction section (~Line 64).

## Reply to Referee Comment 2

The paper investigates the importance of representing aerosol growth, activation and interaction with clouds for radiation fog. To do so, a set of Large Eddy Simulations (LES) are performed, with varying conditions, for a case study of a real fog event from the FAIRARI campaign, on February 18-19, 2022 in the Po Valley (Italy). Although studies of the impact of aerosols on radiation fog are not new, this one stands out because detailed observations of aerosols and droplets were available to constrain the model initialization and for its evaluation, and because it focuses on the importance of accounting for non-activated aerosols. The experiment design and results are clearly presented, and the paper is easy to read and follow. However, I have one major and some minor concerns that need to be addressed before publication.

Reply: We thank the reviewer for the constructive and valuable feedback. The text and figures have been revised accordingly. In particular, we have clarified the distinction between non-activated hydrated particles and activated particles in both observations and simulations. We have also performed additional simulations to address the potential confusion regarding the supersaturation treatment and provided further clarification on the choice of domain size and the warm advection nudging. Please find our replies and the corresponding modifications below.

### Major comment

My most important concern is about the distinction between the non-activated aerosols and the cloud droplets. To me, this is the most interesting part of the paper, so I would like some more precision/discussion on the method and its quality and uncertainty.

In the model (lines ~135), it is clearly stated that particles with a wet diameter above 2 microns are classified as fog droplets, with a criterion on the activation size to differentiate between non-activated and activated aerosol.

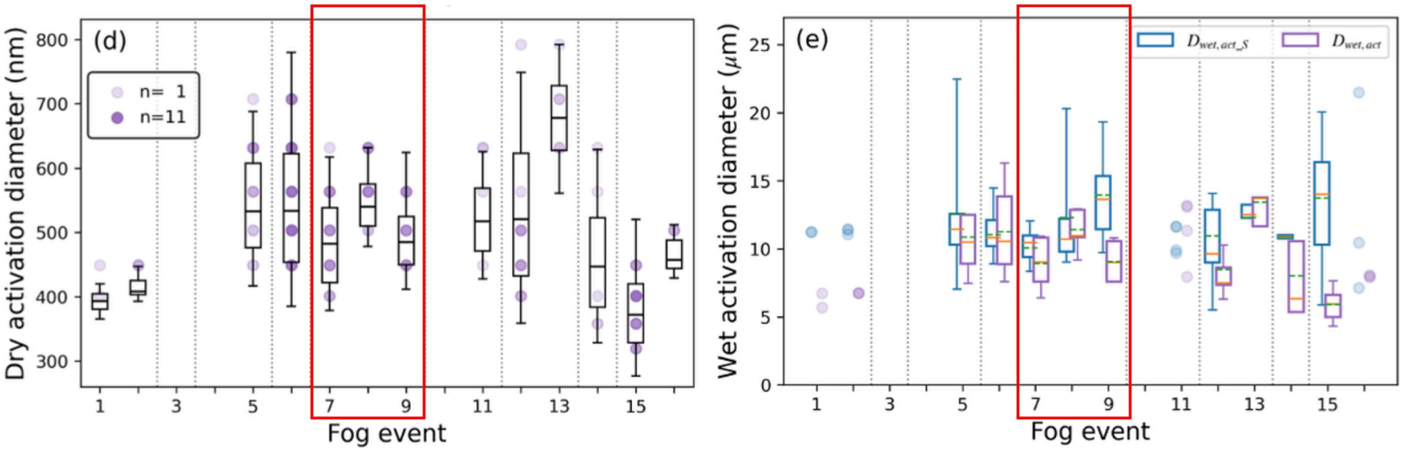
In the observations (lines ~107), all particles below 10µm are counted as non-activated aerosols.

It seems to me that the two approaches are very different. If I made no mistake with the Köhler theory, a 10µm activation diameter corresponds to rather large dry aerosols, over 500nm, that are almost absent from the dry aerosol population (fig. 1b)? Could you elaborate maybe on the validity of this assumption, and the uncertainty it introduces when comparing observations and simulations in the next sections?

Reply: In the model, wet particles with a diameter between 2 µm and the activation size were classified as *non-activated* hydrated aerosols, while wet particles exceeding the prognostic activation diameter were considered activated droplets.

In the observations, wet particles between 2 µm (typical lower measurement limit of fog monitors) and 10 µm (mean activation size in selected fog events) were classified as *non-activated* hydrated particles, and those larger than 10 µm were considered activated droplets. We have provided additional clarification in the manuscript to avoid confusion.

Moreover, the observed activation size of 10 µm was determined based on two approaches (Neuberger A. et al., 2025, https://doi.org/10.5194/egusphere-2025-5419). Please see the results adapted from Figure 4 in Neuberger A. et al. (2025) below. The first, following Hammer et al. (2014) (https://doi.org/10.5194/acp-14-10517-2014), identified the ambient activation diameter (*D_wet,act_S* in the figure) as the first local minimum from the right side of the droplet size distribution; particles larger than *D_wet,act_S* were classified as activated droplets. The second approach used a closure analysis, comparing the dry aerosol spectrum with the droplet spectrum. Assuming that activation starts from the largest particles, the dry distribution was integrated until it matched the observed number of activated droplets, resulting in the dry critical activation size, based on which the wet activation size (*D_wet,act*) was calculated using κ-Köhler theory. For fog events 7–9, the mean wet activation diameter was 9.7 µm (We took ~10 µm).



Does that change the possible interpretation of fig 3a, where the bimodal PSD is linked to the presence of non-activated aerosols? Could such bimodal PSDs also result from collision-coalescence starting from a population of small droplets, even in schemes that do not account for hydrated non-activated aerosols?

Reply: Some highly hygroscopic aerosols might grow via collision-coalescence before reaching the critical supersaturation, leading to large particles that would be counted as "activated". However, effective cloud droplet collision-coalescence generally requires diameters larger than ~20 µm (e.g., Sharon & Levy, 2026, https://doi.org/10.1016/j.ijmultiphaseflow.2026.105613), whereas the resolved hydrated particles in our study (from κ-Köhler theory) are much smaller (<10 µm; see Figure 6 in the manuscript). As a result, the collision-coalescence efficiency is generally very low, and the contribution of such processes to the second PSD peak is likely small (as also discussed in our sensitivity experiments in Section 3.4 and Section S5).

From another perspective, for Event 7 and Event 8 (please see figure above adapted from Neuberger A. et al., 2025), the statistical difference between the activation size computed from κ-Köhler theory (*D_wet,act*) and the empirically estimated activation size (*D_wet,act_S*) is small. This indicates that the majority of the larger droplets contributing to the second PSD peak come from purely thermodynamic activation.

**Minor comments**

- Introduction: In the first scientific question, meteorological conditions are not really the topic here?

Reply: Indeed, changing meteorological conditions were not the primary focus of this study and we have modified the first research question to better reflect this (~Line 81). We did perform sensitivity experiments to examine the impacts of changing surface forcing and warm air advection. These results are presented in the Supplementary Information. In the main manuscript, we suggest that the skin surface temperature (SST) and moisture (SSM) play a critical role in the fog lifecycle, while warm air advection is key in determining fog layer height and near-surface fog microphysical properties.

- The paper of Schwenkel and Maronga (2019) is very interesting and complementary to the work presented here. It is only briefly cited here, but probably could be discussed in more depth. First, the choice of the supersaturation treatment in the model may have an impact on the results. The Morrison and Grabowski approach is chosen here (line 123), do you think that the results would be different with another method? The comment about mixing ratio overestimation could also be expanded, as Schwenkel and Maronga show that very different results are obtained in their sensitivity tests, so is that really a common plague of bulk schemes?

Reply: Indeed, as noted by Schwenkel and Maronga (2019), the saturation adjustment scheme tends to overestimate the LWC (when an interactive aerosol scheme is enabled), while diagnostic and predicted supersaturation schemes produce similar aerosol activation. However, their aerosol input was representative of a typical continental aerosol background (based on Cohard et al., 1998) and a comparison between the modeled fog microphysics and an observed fog case could not be made.

In our study, we employed a prognostic supersaturation scheme, under which we still observed an overestimation of near-surface droplet number and LWC. Furthermore, in the current version of MIMICA, the saturation adjustment scheme can only be applied under prescribed CCN conditions. As a result, we are unable to perform a comparison of different saturation treatments within an interactive aerosol scheme as done by Schwenkel and Maronga (2019). Nevertheless, based on their conclusions, it is reasonable to infer that adopting a saturation adjustment scheme in our case would likely further overestimate the droplet number concentration and LWC.

- Case description lines 96-97: are 13.3-792 nm and 2-60 μm the observation limits for aerosols and droplets? And is that why a 2μm threshold is chosen for fog droplets in the model?

Reply: Yes, 13.3-792 nm corresponds to the observational range for aerosols measured by the DMPS, while 2-60 µm corresponds to the size range used for droplet measurements. Although GFAS can capture droplets below 2 µm, a 2 µm lower threshold is commonly applied for fog monitors, and we adopt the same range consistent with that in our observation paper (Neuberger A. et al., 2025, https://doi.org/10.5194/egusphere-2025-5419). We have added this clarification in the case description (~Line 100, Line 119) and in the supplementary table on measurements (in Section S1).

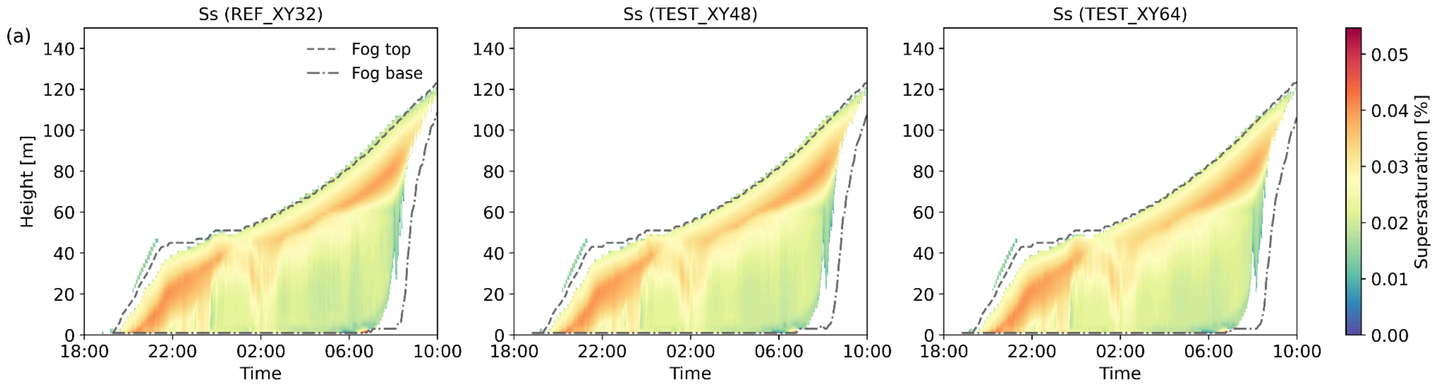
- Visibility calculation: why use the parametrization by Gultepe, instead of computing explicitly the visibility from the Koschmieder formula? Do you think this can be an explanation for the difference between the observed visibility and the visibility computed from the observed act acd+hyd particles in fig. 5d? Or is there an important effect on visibility of hydrated aerosols that are too small to be observed?

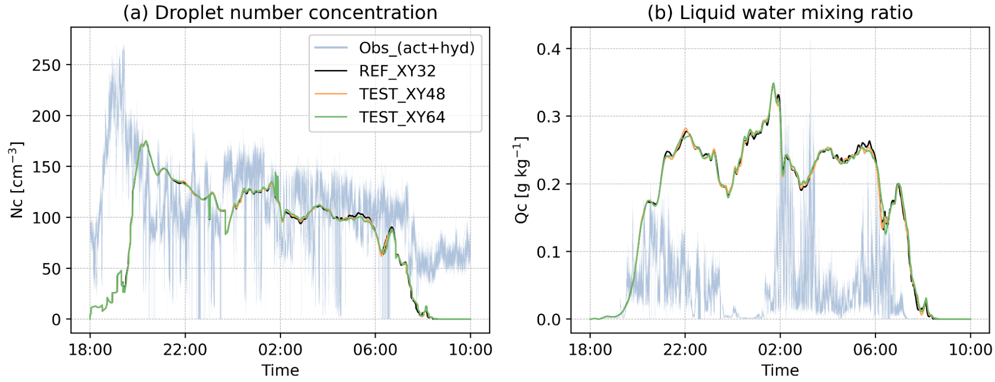
Reply: In our model MIMICA, the extinction coefficient β_ext is not explicitly calculated as a function of wavelength and droplet size distribution as in the Koschmieder formula Vis(λ) = constant/β_ext(λ), which would still require empirical relations based on diagnostic LWC and R_eff. Moreover, hydrated aerosols contribute significantly to extinction but only slightly to LWC, so we adopted the Gultepe06 visibility parameterization (Gultepe, I. et al., 2006, https://doi.org/10.1175/JAM2423), which depends on droplet number concentration (where both non-activated hydrated and activated particles were included in our study) and LWC. In contrast, methods like the Kunkel84 scheme (Kunkel, B. A. 1984, https://doi.org/10.1175/1520-0450(1984)023<0034:PODTVA>2.0.CO;2), which rely only on LWC, would introduce larger biases as the hydrated particles are not explicitly considered with this scheme.

Regarding the differences between the observed visibility (from the visibility sensor) and the parameterized visibility computed from observed Nc and LWC (from GFAS), indeed, this discrepancy can be explained by the fact that the Koschmieder formula was not explicitly applied, so the extinction was not fully resolved and accurately represented. However, it may also be due to the observations. The visibility sensor accounts also for extinction from very fine particles, whereas the calculation with the Gultepe06 scheme, only droplets larger than 2 µm are included. As a result, the parameterized visibility tends to be higher than the observed values.

- Simulation set-up: it is very nice that you checked the impact of the time step on supersaturation. But I am also concerned about the domain size, is it large enough to resolve small-scale circulations that happen inside the fog layer, even in radiative cases? Have you checked if the reference simulation on a larger domain behaves similarly? The fog evolution on fig. 4 shows nothing wrong, so this might not be an issue.

Reply: Additional simulations with a domain of X,Y = 64 and X,Y = 48 grid cells, compared to the reference X,Y = 32 have been conducted to evaluate the sensitivity to the domain size. The results (as shown in the figures below) indicate that increasing the horizontal domain by factors of 1.5 and 2 has a negligible effect on the vertical evolution of fog and the near-surface microphysical properties.





- Is cloud droplet deposition at the ground parametrized? As for example in:

Katata G. Fogwater deposition modeling for terrestrial ecosystems: A review of developments and measurements. Journal of Geophysical Research: Atmospheres 2014;119(13):8137–8159.

Could that explain overestimation of liquid water content and droplet number concentration at the ground (lines 277-278)?

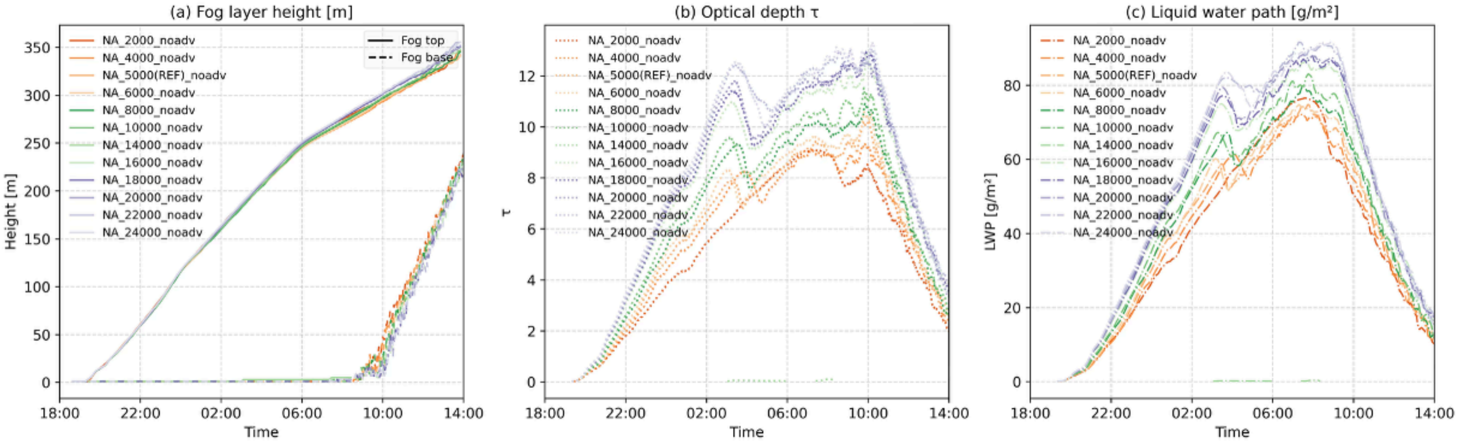
Reply: Droplet deposition at the ground is not yet represented in our model. And yes, this could also contribute to the overestimation of liquid water content and droplet number, which we have now clarified in the revised manuscript in Section 3.1 (~Line 296): *”Another potential reason for the overestimate by MIMICA is that the model does not include a detailed parametrization of atmosphere–surface interactions. For example, processes such as droplet deposition at the surface and interception by vegetation are not represented (Katata, 2014).”* We are developing an interactive surface scheme in MIMICA and will present its effects in our future work.

- Fog top height (and vertical structure): At lines 330+, the very similar fog top height in all simulations is discussed. But can there be an impact of the temperature nudging that represents the warm advection? This nudging also should be more precisely presented in sect.

2.3 (only adding one sentence, mostly about the altitudes where nudging is applied, and keep the figure in the supplemental material).

Reply: We conducted the AERO_NA experiments without warm advection (Please see figure below), which shows that warm advection is not responsible for the similar fog top across the Na experiments. We are implementing the full aerosol-radiation interaction (ARI) in MIMICA, and our preliminary results show that ARI also plays a significant role in the fog top lifting and shaping the vertical structure of the fog layer.

We have added a brief description of the nudging in Section 2.3 (~Line 181) as: *"To reproduce this feature, we applied nudging based on the ERA5 profiles between 21:30 and 22:30 LST, primarily for warm advection above 120 m (Fig. S1).".*



- Fig 7: clarify in the caption that Nc, Qc and Dc are for act+hyd particles?

Reply: Yes, Nc, Qc and Dc in Figure 7 are for act+hyd particles. We have clarified in the figure caption: *"where both activated and hydrated particles are counted in the fog droplet category and the visibility parameterization".*

- Fig. 13: have a colour scheme that depends on narrow / wide spectrum following the classification in fig. 11? A2N1 could be an orange shade, A1N8 a green shade?

Reply: Figure 13 has been revised to follow the order of increased droplet spectral width rather than the α value, consistent with Figure 11. (Figure 2 has also been updated to match this order). The color scheme in Figure 13 has been updated accordingly: A2N1 is shown in orange and A1N8 in green.

## Additional Modifications

1. The section "Code and data availability" has been updated, the LES model output is now available at https://doi.org/10.17043/fairari-2021-2022-les2-1. (~ Line 493)

2. We have examined Figures 2 and 3 using the Coblis Color Blindness Simulator, as suggested by the editor. We confirm that the figures are color-blind friendly, with all color categories and line types distinguishable across different types of color vision deficiency.

3. We have added a further explanation in Section 3.1 (~Line 319) regarding what the simulated visibility is higher than the observations: *"The parameterized visibility from the simulation is generally higher than the observed one (Fig. 5d). This discrepancy can be partly attributed to the fact that extinction, as a function of wavelength and droplet size distribution (Koschmieder 1924), is not explicitly resolved in MIMICA. Another reason is that in the observations the visibility sensor accounts also for extinction from very fine particles, whereas in the parameterization from Gultepe et al. (2006), only droplets larger than 2 µm are included. Additionally, neglecting the hydrated particles would result in a further overprediction of visibility (17.80%-79.03%, mean 39.64%) and biases in fog period estimation (Fig. 5d)"*

4. We have rephrased the description in Section 3.2 regarding the weak sensitivity of fog vertical structure to changes in aerosol number concentration: *"This suggests that the geometric thickness of the fog layer exhibits a relatively weak sensitivity to variations in Na, when only longwave emission by droplets and subsequent buoyancy-driven turbulent mixing are considered (and no other aerosol-driven radiative effects).*

5. Minor corrections:

- In Section 2.1, *"with a mean visibility ranging between 66 and 151 m."* has been corrected to *"with a mean visibility Vis ranging between 66 and 151 m"* (~Line 96).

- In Section 3.2, *"Considering the underestimate of the hygroscopic growth factor noted in section 3.1"* has been corrected to *"Considering the underestimate of the hygroscopic growth factor noted in Sect. 3.1"* (~ Line 338).

- The *"sea surface temperature"* in the caption of Figure S4 has been corrected to *"surface skin temperature".*