Replies to the Editor are in blue. Editors' comments are in black. Modifications in the main text appear in red.

Dear Editor,

The authors would like to thank the reviewers for their helpful and interesting comments on the manuscript. Thanks to the reviewers, we do think this new version of the manuscript has been greatly improved. Further discussions on sedimentation process, thermodynamic effects, turbulence, and the bottom-driven boundary layer have been added. Figures 11 and 12 were revised to enhance clarity in budget readings, a dedicated subsection was added to provide deeper insights into the model and observation comparisons, and a bullet-point list was included at the end of the discussion to better highlight the key takeaways. Accordingly, e add in the acknowledgments section : 'We would like to thank the Editor and two anonymous reviewers for their thoughtful comments and efforts towards improving our manuscript.'

Please find below the point by point responses to the reviewers' comments.

REVIEWER 1:

Overview

This paper uses observations of microphysics in fog from the SOFOG3D campaign to evaluate the performance of the two-moment LIMA microphysical scheme within the Meso-NH high resolution model (using the Arome model for initialisation/boundary conditions). The observations include vertical profiles of microphysical parameters within two fog cases. Whilst similar observations have been collected before, for example during the LANFEX field campaign, such data are still very rare, but of high value to fog research to better understand the characteristics and evolution of fog. The SOFOG3D data set therefore has significant potential to aid the development of fog modelling, such as in this study.

The paper concludes that the model does a reasonable job simulating two case studies, but certain issues are identified such as an overestimation in liquid water content, and also that the drop size distribution was not correctly modelled, due to limitations of a one-moment scheme. The discussion provides a useful analysis of the strengths and weaknesses of the model.

The paper is generally well laid out and written, though I feel there are some areas where clarification is required, together with some persistent grammatical issues, as pointed out below. Also I feel there could be more discussion on certain topics.

We thank the reviewer for their positive and constructive comments. We appreciate the recognition of the value of the SOFOG3D dataset and its potential for advancing fog microphysics understanding. We also clarify that this study is based on the two-moment LIMA scheme, not a one-moment scheme, as stated in the introduction and throughout the manuscript.
All specific comments have been addressed below, and the manuscript has been *improved*

accordingly. *It is worth noting that, thanks to the reviewer's suggestions, further discussions on sedimentation process, thermodynamic effects, turbulence, and the bottom-driven boundary layer have been added.*

Points to address

1) Model set up. Suggest slight clarification. What is meant by (line 170) '..with the configuration previously identified as the most satisfactory.': you mean as laid out in Cuxart, (2015)? Also, regarding the sizes of the nested grids, how sensitive were the results at the super site to these?

We agree with the reviewer that this point was not sufficiently clear. The reference to Cuxart (2015) only concerns the use of hectometric resolutions, which are relevant for simulating flows over complex terrain and are referred to as high resolution by this author. This has now been clarified as follows: 'The non-hydrostatic research model Meso-NH (Lac et al., 2018) is used in a 3D configuration with hectometre-scale resolution, which Cuxart (2015) describes as high resolution and has proven effective for fog simulations (Ducongé et al., 2020; Fathalli et al., 2022). '

The results at the supersite were not sensitive to the sizes of the nested grid but to the initial conditions, similarly to the fog forecast itself. However, this is not the focus of the present study and should instead be addressed in a companion paper. The word 'previously' has also been removed to avoid confusion.

2) Line 200. Suggest explaining why the fog deposition scheme does not also use Stoke's law. I would have liked to have seen more study on the fog deposition at the surface, given its potential to significantly affect the fog morphology. For example, the reported observed reduction in droplet concentration near the surface maybe principally caused by sedimentation. Given the large number of trees in the SOFOG3D region, one imagines that aerodynamic capture of drops (as suggested by Price 2025) could be large. A section dedicated to deposition would have been good, but it is presented more as an after-thought, in the discussion. At least the authors correctly identify it as a property that requires further study.

We fully agree with the reviewer's suggestion that further studies on fog deposition are needed, given its impact on fog morphology, as highlighted by the sensitivity tests we performed. This appears to be a research topic in its own right, and clearly deserves further investigation. In our understanding, Stokes' law describes the settling velocity of particles as a function of their diameter. In contrast, deposition refers to the capture of particles by surface irregularities and is influenced less by particle size than by near-surface dynamics. With this in mind, factors such as vegetation and turbulence are expected to have a strong influence on surface deposition, as mentioned in our discussion.

We add the following sentence in the text line 206-207: 'Droplet sedimentation is calculated using the Stokes law for cloud droplets and is active till the ground, while fog deposition is activated with a constant velocity of 2 cm/s according to Mazoyer et al. (2017) to represent the aerodynamic capture of drops.'

3) Line 310. Discussion of TKE. I understand that the BASTA radar was Dopplerised during SOFOG3D? If so, then would using this data have not added more direct insight into the dynamics within the fog? Vertical velocity variance measured there can be very useful. Also, I suggest that it is important to acknowledge that TKE measured during the stable phase of a fog is significantly influenced by the horizontal sloshing of fog from side-to-side, and does not represent turbulence as it is commonly understood, unlike during the adiabatic phase of a fog.

Thank you for your interesting suggestion — we agree with you. We do not currently have access to vertical velocity variance. As mentioned in the discussion and conclusion, a radar with enhanced capabilities, including measurements of vertical velocity variance, would be very useful.

For the moment only measurements of vertical velocities are available. Accordingly, the following sentences have been added in the discussion l645-647 : "Fog dynamics could also be better qualified using vertical Doppler velocity measurements from BASTA. Initial preliminary results (not shown) reveal a very distinct difference in behavior between fog and stratus."

As the TKE during the stable-thin phase of fog is influenced by horizontal sloshing in both the model and the observations, we used a 30-min moving average to ensure more comparable results.

We add the following sentence in the text line 354-355: "Fields are previously averaged over a 10-minute sliding window to reduce variability, except for TKE, which has a 30-minute temporal resolution in order to avoid the influence of side-to-side horizontal sloshing of the fog on TKE measurements during the stable phase.

4) Please check that all acronyms are defined.

We thank the reviewer to notice it. LANFEX, BASTA, HATPRO, LIMA, LWC definitions have been added. Intense was added to Observation Period for IOP.

5) Line 355. Explanation of radiation. Temperature errors would be the most likely explanation ?

We thank the reviewer for raising this interesting point. Although fog representation is clearly the primary factor, we note that the overestimation of LWnet may also result from other sources of error, as temperature biases can significantly affect radiation. However, temperature is not the only contributing factor. An overestimation of water vapour content can affect atmospheric emissivity, while the direct effect of aerosols, which is currently not accounted for, may also influence temperature evolution. The sentence l367-379 has been rewritten as follows:

'For IOP 6, the simulated LWnet is larger in absolute value than the observed values, which is likely due to cloud effects, with additional contributions from other sources of error, such as temperature errors, water vapour absorption or aerosol concentrations.'

6) Line 366. ‘vicious circle’ – suggest ‘positive feedback’.

Thanks for the recommendation. It is corrected.

7) Line 409. No need for a new paragraph.

Thanks for the recommendation. It is corrected.

8) Line 412. Meaning of paragraph not clear.

Thanks for the recommendation. We change the sentence to : "Although there are discrepancies between the model and the observations regarding drizzle and rain, the simulated droplet size shown later falls within the range of the CDP measurements."

9) Line 441: ‘primarily’.

Thanks for the correction.

10) Line 468 and elsewhere. Use of ‘aggregate’. Do you mean ‘average’? Also may be useful to clarify that ‘phase’ refers to the temporal phase of the fog and not of the DSDs! What is meant by ‘sub-phase’ (line 482 and elsewhere)?

Thanks for the recommendation. We change 'aggregate' by 'average'.

The sentence has been rewritten as follows: "Figures 9 and 10 present the vertical profiles of temperature, LWC, and Nc averaged over the different fog phases, as well as the corresponding DSD, averaged over different vertical layers, for IOP 6 and IOP 11, respectively."

The term sub-phase refers to internal variability within a given phase than canitself be sub-divided into distinct periods. These sub-phases are defined l480 and the sentence has been rewritten for clarity as follows: "Regarding LWC and Nc, IOP 6 exhibits significant variability during the thin-to-thick transition due to differences between the sub-phases, i.e. before and after 00 UTC."

11) lines 495/6. ‘dispersion’. Suggest, ‘Variation’?

Thanks for the recommendation. It is corrected.

12) Line 527. ‘small-to medium-sized droplets’. Please define the actual size range for this phrase.

They are defined l89-89 and l504 : 'up to 30 µm in diameter'. Meaning from the 10 µm mode to 30 µm in diameter.

13) Line 553. 'faults'. Suggest, 'discrepancies'?

Thanks for the recommendation. It is corrected.

14) Line 569. What is meant by, 'dryness inducing evaporation'? Where is the dryness from?

We mean dryness above fog layer as show in figure 5.d. that induce evaporation as shown in figure 12.d. We change the text accordingly : 'The main causes are the dryness above fog layer inducing evaporation and the excessive stable temperature profile'

15) Lines 578-9. (and L606-7) High humidity for one case and cold temperatures for the other, cannot alone, account for the excessive liquid water production.

High humidity excesses, such as 0.5 g/kg excess in figure 3.b can have a very strong impact (~0.6 g.m-3 if fully converted into liquid water). However, in this case, the effect is compensated by higher temperatures during IOP 6. Conversely, a negative temperature error of 1 K would induce an impact of about 0.5 g/kg on the saturation mixing ratio r_v,sat, but this effect is compensated for by lower humidity during IOP 11. We believe this is one of the main reasons why fog is so difficult to forecast. Nevertheless, we also expect that other factors discussed in the manuscript, such as the sedimentation of large droplets and droplet deposition, strongly affect the liquid water budget.

The following sentence has been added in the discussion l627-631: "It should be noted that humidity excesses of about 0.5 g.kg−1, as simulated during IOP 6, may substantially enhance liquid water content (0.6 g.m−3 if fully condensed), although this effect is compensated by higher simulated temperatures. Conversely, a negative temperature bias of 1 K, as observed during IOP 11, leads to a change of about 0.5 g.kg−1 in the saturation mixing ratio, although this effect is compensated by lower simulated water vapor mixing ratios. This strong sensitivity to small thermodynamic biases likely explains, at least in part, why fog remains particularly difficult to forecast.'

16) Lines 584. 'The sedimentation process mainly contributes to the shift towards larger droplets in the DSD...'. Where is the proof for this statement? If this is an assumption then the statement needs re-wording as a proposition.

The reviewer is right. Clarification was needed. During the thin-to-thick transition phase, figures 11.e.f and 12.e.f show that sedimentation contributes positively to both the mixing ratio and the droplet concentration within the fog layer for both IOPs. In addition, figures 9.d.e.f and 10.d.e.f show a shift towards larger droplets close to the ground. However, figures 9.b.c. and 10.b.c indicate that Nc remains approximately constant with height, while rc ~~i~~ncreases toward the surface.

Furthermore, Figure R0 illustrates the temporal evolution of drc/dt/rc and dNc/dt/Nc, providing insights into the growth of rc and Nc based on their current values. The data indicates that rc experiences a more pronounced increase due to sedimentation compared to Nc, implying an expansion toward larger diameters.

We therefore think that sedimentation mainly contributes to a broadening of the gamma distribution toward larger diameters through an increase in rc while Nc remains nearly constant. The sentence has been rewritten as follows: "The sedimentation process should mainly contribute~~s~~ to the shift toward~~s~~ larger droplets in the DSD when moving downward within the fog layer for both IOPs (Fig. 9 and 10)."

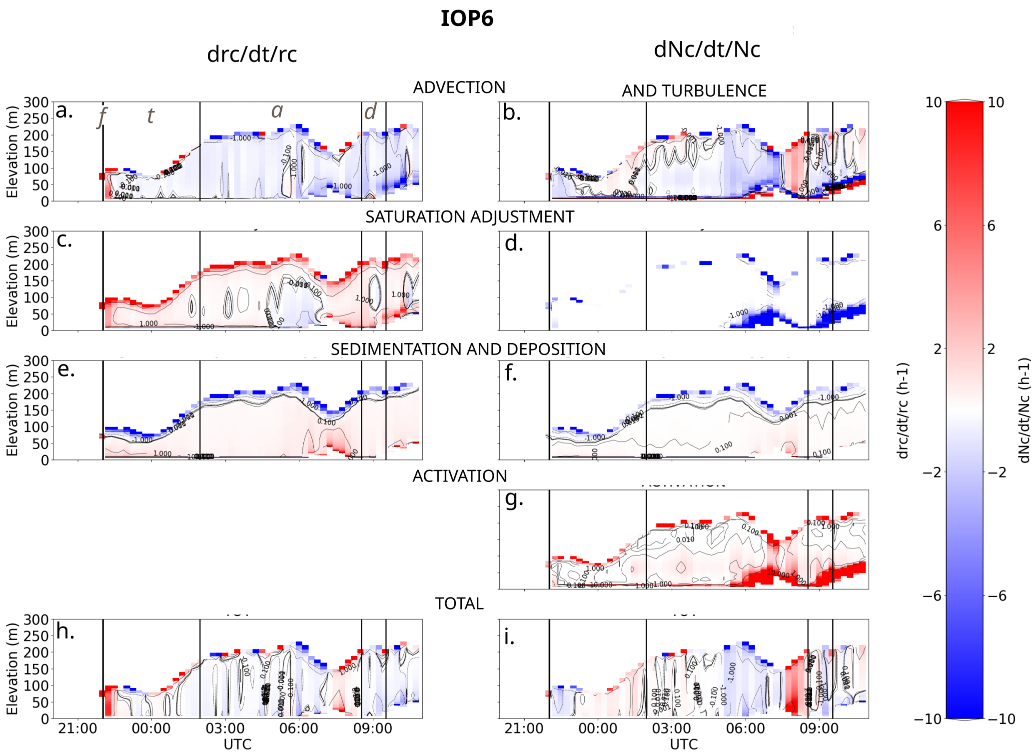

FIGURE R0: IOP 6: Vertical budget of rc and Nc over rc and Nc respectively during the fog life cycle: evolution (a: rc, e: Nc), due to advection (b: rc) and turbulence (f: Nc), saturation adjustment (c: rc, g: Nc), sedimentation and deposition (d: rc, h: Nc) and activation (i: Nc). The black vertical lines separate the fog phases.

17) Line 613. The study by Price (2025) considered deposition of fog droplets at the surface.

Thanks for the correction.

18) Line 674. Paragraph. Droplet sedimentation/capture at the surface would seem like a good candidate for causing the gradient in Nc near the surface. Note also that adiabatic fog with low

saturation excess could exhibit sub-saturated conditions near the surface if e.g. it advects to a lower altitude, or during the morning period when it starts dissipating (but still remains as fog). Both of these could affect Nc in the lowest part of the fog.

We agree with the reviewer. However, our sensitivity tests on droplet deposition did not affect the Nc gradient near the surface; instead, they influenced the entire fog layer, which may be related to the excessive turbulence simulated by the model.

Regarding subsaturated conditions, Price et al. (2011) reported that surface warming of the fog occurred in all the cases they examined; similar results were obtained by Mazoyer et al. (2022), leading to subsequent evaporation in the lowest part of the fog.

However, Figures 6.c and 7.c show that Nc remains relatively constant in the lowest levels sampled by the balloon measurements, whereas the Nc gradient appears at the end of the transition phase, before the onset of adiabatic behaviour. This suggests that surface evaporation may not be the dominant mechanism responsible for the observed Nc gradient. Alternatively, the excessive TKE simulated by the model may lead to excessive mixing and thus contribute to the development of this gradient.

19) line 739. 'dew deposition', suggest 'dew and droplet deposition'.

Thanks for the correction.

20) Throughout the paper there are examples of verbs and adjectives that are not qualified, and force the reader into making assumptions about what the authors are talking about. For example, line 460, 'concentrations', and line 539, 'bottom'. Concentrations and bottom of what? Please add appropriate verbs and adjectives to reduce ambiguity throughout.

Thanks for the correction. Concentrations refers now to droplet and bottom to layer.

21) The paper states that 140 profiles were taken collecting microphysical information during SOFOG3D (Costabloz et al. 2024). Looking at the figures I estimate around 40 profiles or partial profiles are presented here. What are the reasons that the majority of the data has not been used in this study? Can we look forward to more model comparisons in future from this data set?

The reviewer is right: only two IOPs from SOFOG3D were studied in the present paper. In Costabloz et al. (2024), 12 IOPs were analysed. We focused on two IOPs in order to enable a more detailed investigation. We hope to include more model comparisons in the near future.

22) Figure 2. Reference to the various panels seems incorrect.

Thanks for the correction.

23) Figure 4. Panel h) shows a bottom-driven boundary layer, but we would expect a top-driven one in this situation?

The issue raised by the reviewer is highly relevant. Figure 4h shows a bottom-driven boundary layer with the strongest TKE (resolved+subgrid) values occurring near the surface, whereas the observations in Figure 4g indicate a more homogeneous TKE profile, including some profiles that are more consistent with a top-driven boundary layer. In addition, the comparison of surface TKE in Fig. 6e shows an overestimation by up to a factor of two in the simulation.

Figure R1 shows the mean and standard deviation of subgrid TKE, the TKE budget and the Richardson gradient over an area of 1 km^2 simulated at Charbonnières at 04:00 UTC during the adiabatic phase. The TKE profile (Fig. R1b) confirms a bottom-driven boundary layer, with active dynamical production of turbulence (Fig. R1c, blue curve) at the top due to wind shear, balanced by TKE dissipation (Fig. R1c, grey curve). Thermal production is negative at the fog top and therefore opposes the positive shear production: this is due to strong stratification, which is overestimated compared to observations (Fig.4 a,b). The Richardson gradient is also negative within the fog layer (Fig. R1d), indicating convective behaviour within the layer.

In contrast, Mazoyer et al. (2017), using an LES configuration with a 5 m horizontal resolution, found positive thermal production of turbulent kinetic energy at the fog top during the adiabatic phase (their Fig.7d). In addition, the resolved vertical motions at the fog top were significant (their Figure 6.d). They also reported strong TKE values near the fog top from the thin-to-thick transition (their Fig. 7a), mainly produced dynamically by wind shear (their Fig. 7c,d).

Therefore, the discrepancy at the fog top may also be related to limitations of the turbulence scheme in accurately representing the thermal production near the top of the boundary layer.

To summarise, land-surface heterogeneities combined with an overly strong temperature inversion favour a bottom-driven boundary layer in the simulation during the adiabatic phase. Although the lidar measurements are limited by droplet attenuation, the observed vertical TKE profiles are generally more homogeneous, with some profiles showing slightly enhanced TKE near the fog top, theefore being more consistent with a top-driven boundary layer. The turbulence scheme may also contribute to the lack of turbulence simulated at the fog top. However, further investigation is required to better understand the physical mechanisms responsible for the discrepancies between observations and simulations.

Thanks to the Reviewer question, we propose to add a sentence as follows:

"The simulation favours a bottom-driven boundary layer, with higher TKE values near the surface, whereas the observed vertical TKE profiles, although limited by droplet attenuation, are more homogeneous, with some profiles showing slightly higher TKE values near the fog top, thus being more consistent with a top-driven boundary layer. These discrepancies are probably due to the overestimated fog-top inversion, and to excessively strong surface fluxes. It may also be linked to the turbulence scheme’s difficulty in accurately representing thermal turbulence at the top of the boundary layer. An LES configuration, where turbulence is by definition mainly resolved, such as that used by Mazoyer et al. (2017), does indeed allow for a better representation of the dynamics at the top of the boundary layer."

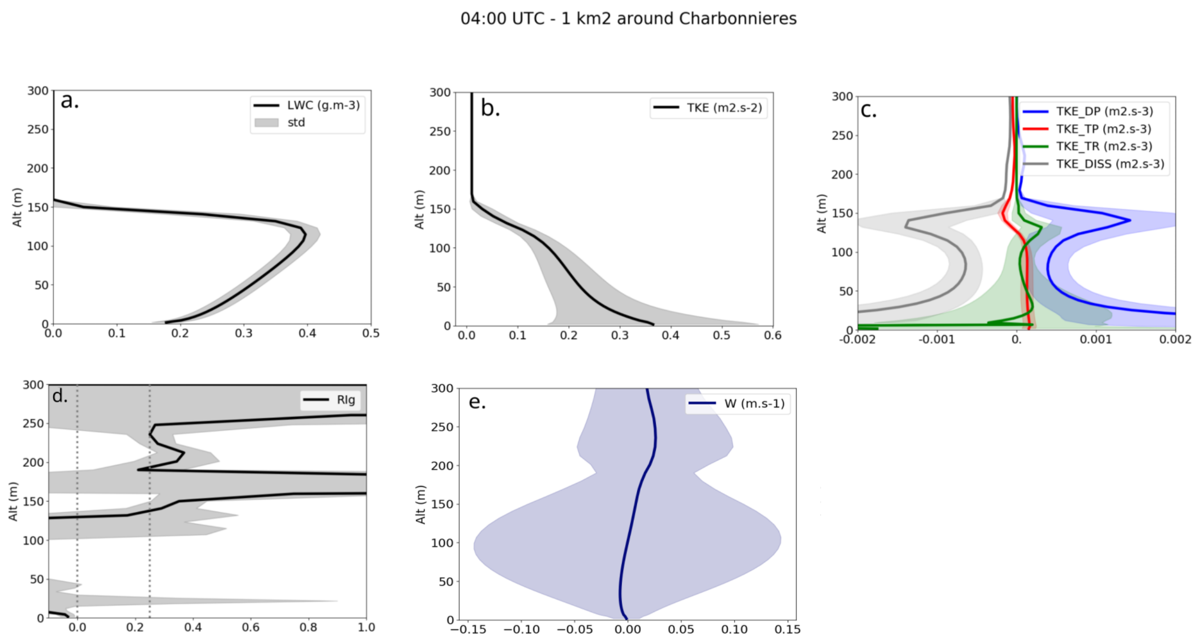

Figure R1. IOP 6 mean and standard deviation of LWC (a), subgrid TKE (b), TKE budget (c), gradient richardson (d) and vertical velocity (e) at 0400 UTC on a 1 km2 around Charbonnières.

24) Figure 5, panel e). Wind vectors too small to read on A4 printed material.

Thanks for the correction. Size of the figures has been increased.

REVIEWER 2:

Overview

This study presents a high-potential evaluation of fog microphysics using a sophisticated dataset (SOFOG3D) that combines tethered balloon observations with remote sensing. The use of the Meso-NH model with the two-moment LIMA scheme is state-of-the-art and addresses a significant gap in our understanding of the vertical structure of fog.

However, the manuscript currently lacks a clear link between the described dynamic causes (e.g., advection, initialization) and the microphysical evidence (e.g., reflectivity vs. liquid water content). Specifically, the "reflectivity paradox" identified in IOP 6—where the model overestimates LWC but underestimates reflectivity—points to a fundamental constraint in the droplet size distribution (DSD) parameterization that requires deeper discussion. Additionally, inconsistencies in model initialization between the two cases and the treatment of measurement biases need clarification before publication.

We appreciate the Reviewer's positive and constructive comments and address his/her main concerns below. Due to the reviewer's comments, the manuscript has been enhanced in the following ways: Figures 11 and 12 were revised to enhance clarity in budget readings, a dedicated subsection was added to provide deeper insights into the model and observation comparisons, and a bullet-point list was included at the end of the discussion to better highlight the key takeaways.

Major comments

1) **Unclear attribution of model biases to microphysics vs. dynamics — needs stronger evidence and clearer separation.**

Explanation: the manuscript frequently attributes the LWC and Nc biases both to thermodynamic/dynamical biases (e.g., wet/cold/TKE biases) and to structural microphysics limits (single-mode gamma DSD). These arguments appear repeatedly (e.g. discussion of LWP/LWC overestimate and TKE link). The causal chain is plausible but not demonstrated strongly enough: is excess condensation producing high Nc, or is excessive activation due to turbulence producing excess LWC? See discussion and budget summaries. add (or more clearly present existing) quantitative diagnostics that separate dynamical from microphysical causes. Show time/height cross-correlations (or regression) between modeled TKE perturbations and activation rate / Nc production to support the "TKE → activation → Nc → LWC" chain. (Add a short figure or table with correlation coefficients or a simple sensitivity test.) If available, present a sensitivity experiment (or at least a quantitative estimate) where only microphysics is changed (e.g., change activation parameter) and separately where only dynamics (e.g., TKE or surface fluxes) is nudged — to demonstrate which change reduces the LWC/Nc bias most. The manuscript mentions sensitivity tests (not shown) — if you ran them, include a concise table/figure; if not, state explicitly that these are planned.

We thank the reviewer for bringing up this intriguing point.

However, we believe the effect of TKE on Nc is mentioned only in lines 597-598 and again in the discussion (lines 684-689) when introducing the aforementionned test. The influence of thermodynamics/dynamics on LWC is referenced only at line 503, within the discussion (lines 610-611), and in the conclusion.

LWC and Nc production originates from different processes. Among microphysical processes, LWC is produced by saturation adjustment while Nc is produced by activation. Saturation adjustement depends on the excess water vapor compared to saturation (no sub-grid condensation here) while Nc depends on the maximum diagnosed supersaturation which depend mainly on vertical velocity plus TKE and diabatic cooling/heating rate including radiative (see Vié et al. 2024 for more insight into the supersaturation calculation).

Regarding the saturation adjustment as mentioned to reviewer 1 *and added in the discussion* :

'It should be noted that humidity excesses of about 0.5 g.kg−1, as simulated during IOP 6, may substantially enhance liquid water content (0.6 g.m−3 if fully condensed), although this effect is compensated by higher simulated temperatures. Conversely, a negative temperature bias of 1 K, as observed during IOP 11, leads to a change of about 0.5 g.kg−1 in the saturation mixing ratio, although this effect is compensated by lower simulated water vapor mixing ratios. This strong sensitivity to small thermodynamic biases likely explains, at least in part, why fog remains particularly difficult to forecast.'

Nevertheless, the evolution of DSD that relies on Nc (from the whole layer during the thin phase and from the top during the optically thick phase) will influence both the intensity of fog-top cooling and the generation of LWC there. Figure 6 shows that TKE rises in the lower layer once the fog becomes optically thick (i.e., when the near-surface inversion collapses), suggesting a reduced impact on the TKE → Nc → LWC pathway during the adiabatic phase as TKE impact the lower layer and not the fog top, but potentially a stronger effect during the thin phase. During the adiabatic phase, radiative cooling is considered the primary driver of LWC production through saturation adjustment at the top.

A test disabling the TKE contributions to supersaturation calculations was carried out. Because it was performed and compared against a non-final version, it will not be examined in detail in the paper. Figure R2 below shows this comparison. When TKE is omitted, the thin-fog phase exhibits slightly lower LWC *what would contribute to a lower LWP, based on this criteria the thin-fog phase would persist longer (no proper evaluation).* In the adiabatic phase, Nc is reduced by half what lead to a strong impact on LWC. *In fact, following the well-know chain, a smaller number of droplets results in larger ones, boosting sedimentation while lowering the LWC.*

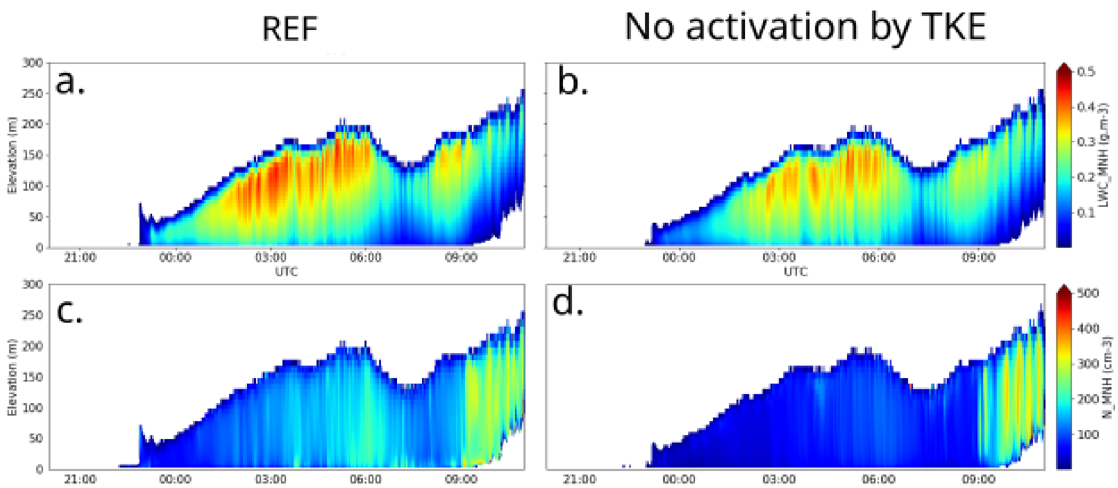

Figure R2. IOP 6 : vertical profiles of LWC (a,b) and Nc (c,d) simulated by Meso-NH with the TKE contribution in the supersaturation calculation (a,c) and without (b,d).

During this experiment, we wondered how the TKE affected the vertical gradient of Nc. Only a minimal improvement was observed *(see from 0300 to 0900 UTC)*, which still clearly does not match the observations.

The overestimation of TKE may also lead to a too strong mixing in the fog.

From our perspective, improving the Nc→LWC chain will *also* stem from a more refined diagnostic of maximum supersaturation, which depends on factors beyond just TKE. Nonetheless, as mentioned in our last paper sentence, gaining additional observational insight into TKE behavior within the fog layer and at its top will undoubtedly be beneficial.

2) **Budget analysis is promising but hard to follow — improve clarity and quantitative presentation.**

Figures 11–12 and the text describe rc/Nc budgets, but the reader struggles to see magnitudes, relative contributions, and integrated tendencies. The text presents qualitative descriptions but few numbers. Add a compact table that reports integrated (over height or over the fog layer) mean tendencies for each budget term during each phase (formation, transition, adiabatic, dissipation). This will let the reader see which process dominates in each phase.

We agree with the reviewer. Writing that section was quite challenging. Thank you for the helpful suggestion. We considered adding the following table for IOP6, for example, and it seems to us that; since it is meant on the whole fog layer, it makes the information difficult to interpret. Dividing it into layers creates a lot of information as well.

Therefore we decide to add a labeled contour plot to Figures 11–12 to better visualize small values, enabling clearer observation of magnitude, relative contributions, and integrated tendencies.

IOP 6		**Formation**	**Thin-to-thick transition**	**Adiabatic**	**Dissipation**
RC	ADV		0.06 (-0.24/0.98)	-0.02 (-0.58/1.43)	-0.04(-0.45/1.03)
(g/kg/h)	SAT ADJ		0.058 (-0.61/0.94)	0.021 (-1.44/1.22)	0.025 (-0.81/0.48)
	SEDIM AND DEPO		-0.04 (-2.9/0.5)	-8e-5 (-1.97/0.12)	0.005 (-0.37/0.07)
NC	ADV AND TURB		12 (-3 009/2 865)	-9.9 (-6 465/4 479)	-48 (-142 25/8 828)
(cm^{-3}/h)	SAT ADJ		-27 (-1 188/0)	-94 (-5793/0)	-454 (-8 980/0)
	SEDIM AND DEPO		-27 (-2 904/88)	-30 -(3 943/59)	-9 (-1 740/53)
	ACTI		56 (0/3 107)	152 (0/6 156)	633 (0/17 489)

Table 3. IOP6 : Mean values of budget processes (in brackets the min/max). ADV stands for advection, SAT ADJ for saturaton adjustment, SEDIM AND DEPO for sedimentation and deposition, TURB for turbulence and ACTI for activation.

3) **Representation of the DSD: single-mode gamma assumption limits conclusions — quantify the impact and discuss alternatives.**

The text repeatedly notes that LIMA's single-mode/gamma DSD prevents realistic bimodality and under/overestimates large droplet population (e.g. missing 10 µm mode or >30 µm tail). This is a central limitation that affects reflectivity and sedimentation conclusions. Add an explicit sensitivity test or offline exercise: take observed LWC and Nc and compute the gamma-law DSD (you have the "pink" distributions already). Then compute radar reflectivity and compare to observed reflectivity to quantify how much of the reflectivity bias is due to shape vs. other sources. Discuss possible microphysics remedies (multi-mode DSD, explicit coalescence parameterization, drizzle scheme) and estimate (qualitatively or quantitatively) which would be most effective.

The reviewer is correct. While using KHKO (Geoffroy et al., 2008) shows that the parameters of the gamma law (α and ν) have a significant impact on the momentum of the distribution and consequently on the representation of stratocumulus clouds. With regard to fog, similar studies were conducted by Mazoyer (2016) in her PhD thesis (see Figure 4.5, p. 138), where she demonstrates that the impact on fifth-order momentum (LWC sedimentation) is highly dependent on the choice of these parameters.

Therefore, we performed a test using non-definitive simulation, changing α from 3 to 1 and ν from 1 to 3 for the DSD representation. Figure R3 shows the impact on the DSD during the IOP6 adiabatic phase. Adjusting α and ν allows a better capture of the observed DSD shape.

Figure R4 illustrates the effect on reflectivity, which is pronounced because reflectivity depends on the sixth moment of the DSD. As discussed, DSD shape strongly influences reflectivity. In Figure R4 (c.d.e.f), both LWC and Nc exhibit reductions, potentially attributable to sedimentation, which relies on the fifth and second-order moments of the DSD, respectively. Nevertheless, this sensitivity test did not adjust the Nc profile. The test is referenced in lines 632-639 of the manuscript. Lines 640-677 discuss the limitations of the current representation and the most effective microphysics remedies. As mentioned in the discussion, performing a global fit on the (α and ν) values would be highly advantageous. Observations from CDP of FM-120 and radar (reflectivity and Doppler velocity) should be particularly useful for this task. It could be done using AERIS and Cloudnet observations over SIRTA by example and by comparing momentum of the distribution as derived from the obs and as calculated from the model.

The assessment suggested by the reviewer was performed, with the outcomes illustrated in Figure R5. In panel b, reflectivity was derived by applying the LIMA gamma law to the observed LWC and Nc. The findings distinctly show a reduced reflectivity compared to those computed directly from the DSD (panel a). Notably, the 'pink' distributions reveal that, when employing the gamma law with the actual (α and ν) parameters, the largest droplets are underestimated.

As noted, a multimodal DSD that includes a drizzle mode could provide a more insightful representation of the reflectivity but also of the radiative behavior at the fog top and the sedimentation of both the smallest and largest droplets, as well as the coalescence, obviously.

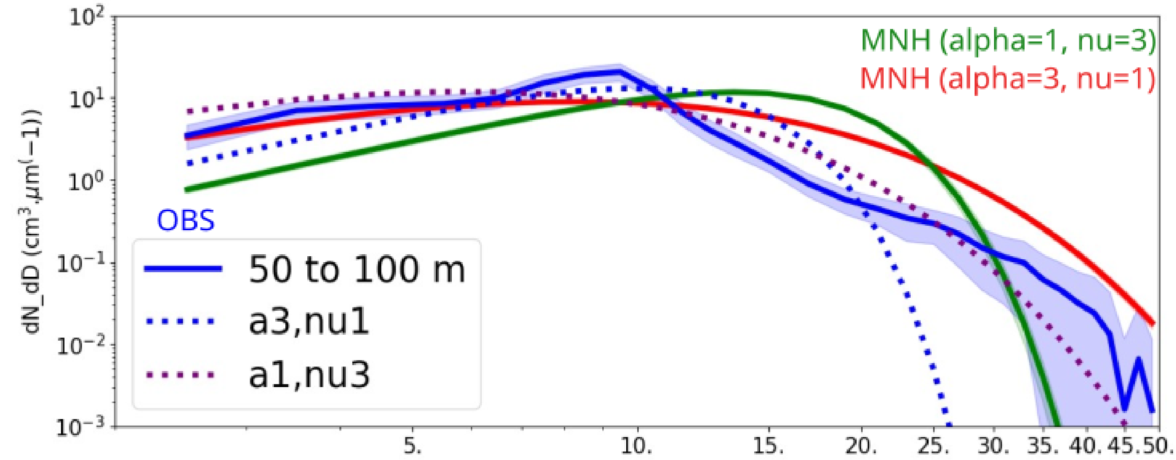

Figure R3. In the IOP6 adiabatic phase between 50 m and 100 m AGL, the average observed DSD (blue) together with its ±0.5 × standard deviation (shaded blue) are displayed. The dashed blue curve represents the gamma law that would be obtained from the measured LWC and Nc combined with the α and ν parameters of LIMA. The dashed purple curve illustrates the gamma distribution using α = 1 and ν = 3 for the observed LWC and Nc. The solid green curve shows the distribution generated by the model, and the red curve corresponds to the model distribution with α = 1 and ν = 3.

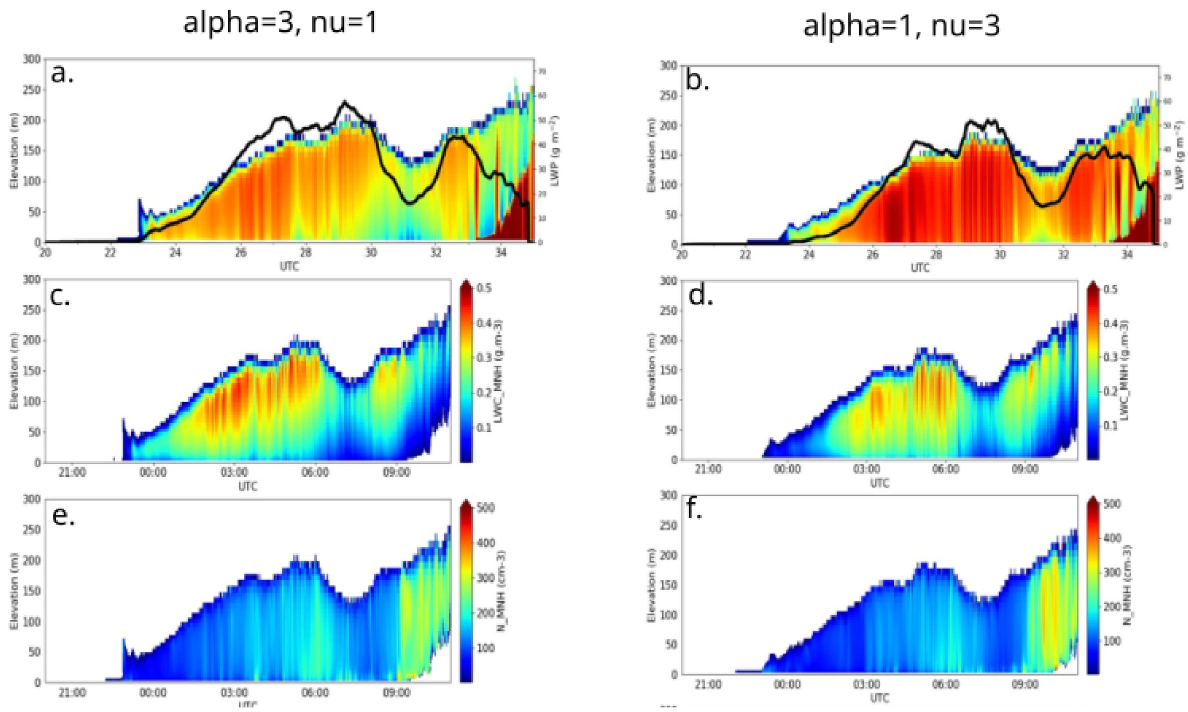

Figure R4. Temporal evolution of vertical profiles of (a.,b.)radar reflectivity and LWP (black line), (c.,d.) LWC and (e.,f.) droplets concentration for IOP 6 simulated with Meso-NH using the standard gamma droplet law (a.) and with α = 1 and ν = 3 (b.)

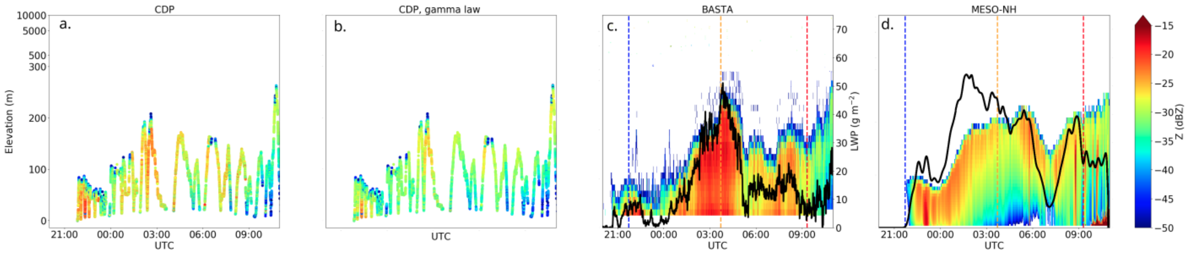

Figure R5. Temporal evolution of vertical profiles of radar reflectivity for IOP 6 (a,b,c,d) derived from CDP observations under the tethered balloon (a), from CDP observations under the tethered balloon but calculated using the gamma law used by LIMA (b), from BASTA radar (c) and the 100 m simulation (d). Black lines represent the observed LWP from HATPRO (c) and the 100 m simulation (d). Vertical coloured dashed lines refer to the RS start time presented in Fig. 3. The vertical axis follows a logarithmic scale above 500 meters.

4) **Instrumentation and representativeness caveats need clearer placement and stronger emphasis.**

The CDP measures 2–50 µm; the manuscript notes that droplets >50 µm could be important and that BASTA/CDP reflectivity mismatches suggest missing large drops. But this important measurement limitation is discussed intermittently rather than in one explicit limitations paragraph; it is central to confidence in comparisons. Create a short dedicated subsection "Observational limits and impact on comparisons" that lists instrument ranges/uncertainties and then explicitly states how these affect each diagnostic (LWC, Nc, reflectivity). Provide a short table summarizing instrument vertical coverage, diameter ranges and uncertainties (you already have Table 1 but expand to explicitly link limitations to specific model comparisons).

Thanks to the reviewer suggestion, we add a short dedicated subsection "Observational limits and impact on comparisons"

"This section analyzes the effect of observational constraints regarding microphysics and their influence on comparisons. Table 1 presents the instrument uncertainties and measurement ranges; refer to the "Measured Variable" column. It is important to note that the CDP measures only within the diameter range [2–50] µm, a range widely used in fog modeling comparison studies (Price et al., 2018; Gultepe et al., 2021; Wagh et al., 2023). In the model, the LIMA microphysical scheme distinguishes between droplet and rain categories based on Cohard and Pinty (2000) and assumes a separation threshold at 82 µm in diameter. However, since LIMA is a bulk scheme, only the first moment (number concentration) and the third moment (mass) of the distribution are predicted. Therefore, a thorough evaluation of the model's cloud droplet spectra compared to observational
spectra is essential before any comparison. This ensures that equivalent diameter ranges are compared, which will be addressed in the remainder of this study (see section 5.3). Further insights into cloud characteristics are offered by BASTA, capable of identifying particles larger than 50 µm. Comparisons between the reflectivity measured by BASTA and the reflectivity derived from the 6th-moment of the distribution measured by the CDP may consequently reveal

the existence of particles exceeding 50 µm, that will be explored in a specific subsection of this paper (see section 5.1). Nevertheless, the model calculates reflectivity by accounting for all particles, enabling a precise comparison with the observed reflectivity."

More observational validation work is certainly required, at present, the caveat remains unknown. As noted throughout the paper, it is possible that drizzle is absent, but currently we have no means to confirm this with the observations used in the study.

Nevertheless, 'the simulated droplet size shown later falls within the range of the CDP measurements', which gives us strong confidence in our comparison. The present study only compares droplets from the model and observations within the 0–50 µm diameter range. For additional details on the the CDP's limitations, refer to Fathalli et al. (2022), Dominutti et al. (2022) and Costabloz et al. (2024).

5) **Phase definition thresholds and sensitivity — be explicit about robustness.**
Thin-to-thick transition is defined by several thresholds (TKE, dT/dz, LWnet/LWP, CTH) and the manuscript notes sensitivity to averaging windows and layer definitions. This affects phase timing and subsequent aggregations. Provide a concise sensitivity test (e.g., show how transition start/end shift when using alternate thresholds or averaging windows) or at least a quantification of uncertainty (± hours). A short table or supplementary figure would suffice. This will strengthen confidence in phase-aggregated comparisons.

We share the reviewer's view. As illustrated in Figure 6, phase determination is highly sensitive to the chosen threshold. Nevertheless, because the same thresholds are applied to both IOP and the model-observation comparison, a meaningful assessment remains feasible.

For our concerns, the more important point is to be comparable. Further discussion on the threshold definition and sensitivity can be found in Dione (2023) and Costabloz et al. (2025).

Further investigation is certainly warranted and should be the focus of a decicated paper, ideally incorporating greater inter-parameter dependencies. Measuring TKE within the fog layer could help achieve this goal.

6) **Conclusions: strengthen practical takeaways for model developers/operational users.**
The Discussion lists useful suggestions (e.g., drizzle probes, radar Doppler) but the Conclusions are relatively general. The paper will be more impactful if it gives concrete, prioritized recommendations for model improvement and for observational follow-ups. Add a short bullet list in Conclusions: (1) short-term model changes to try (e.g., adjusting deposition velocity, implementing two-mode DSD or drizzling scheme), (2) diagnostics to routinely output in future Meso-NH runs, and (3) highest-priority observations to collect in future campaigns.

We appreciate the reviewer's insightful remarks which strongly improves the manuscript practical takeaways. We add the following list at the end of the discussion :

"Based on the present results, several priorities emerge for future fog studies:

Short-term model developments:

– Implement bimodal or drizzle-capable microphysics schemes in order to better represent the observed DSD variability
and evolution, and the onset of light precipitation. At the same time, revisit the assumptions about the gamma DSD
parameters for fog (α, ν).

– Evaluate alternative deposition parameterizations, that may involve dependencies to other parameterizations (such as
turbulence), and are known to strongly influence fog characteristics.

– Revisit CCN activation in fog, especially the link with vertical movements, turbulence and radiation.

Priority observations for future campaigns:

– Measure droplets larger than 50 µm and drizzle occurrence to evaluate bimodal or drizzle-capable microphysics schemes.

– Measure fog-top radiative fluxes to assess bimodal microphysics schemes.

– Measure aerosol concentrations and properties at different locations, together with supersaturation, to further investigate
fog sensitivity to aerosol loading.

– Add Doppler spectral radar functionalities to retrieve DSD, vertical velocities and TKE within the fog layer and improve
understanding of fog dynamics.

– Perform more frequent vertical profiles of turbulence, DSD, and thermodynamic variables to robustly evaluate the mean
state and the variability of generalized gamma DSD parameters (α, ν) and the link with other paramters.

– Measure deposition fluxes to evaluate alternative deposition parameterizations.'

Minor comments

1) Abstract — be explicit about sample size and limitations. o Where: Abstract (p.1). add "two IOPs (IOP 6 and IOP 11) and N tethered- balloon profiles (state N if possible)" and a one-line note that aerosol data were missing for these dates (if true) so readers know sample size up front.

Thank you for the suggestion. In IOP 6 we had 23 thin-to-thick, 16 adiabatic, and 3 dissipation tethered-balloon profiles available, while in IOP 11 the available profiles comprised 5 formation and 11 thin-to-thick tethered balloons. Unfortunately, the abstract's word limit makes it difficult to include this highly relevant information. During these periods, no CCN data were available. As noted in the SOFOG3D case studies section : "Due to technical issues, aerosol measurements were not available for these dates." Consequently, we applied a fit to the entire campaign.

We have added the following sentence to the introduction: "During IOP 6, 42 tethered-balloon profiles will be analyzed and 16 during IOP 11."

2) Line in Intro: "To the authors' knowledge, this is the first study..." — soften or justify. o Suggestion: Either cite LANFEX/LANFEX-like earlier tethered work explicitly (you do elsewhere) or replace with "one of the first" unless confident it is unique.

We are fairly confident that 'this is the first study to examine simulated vertical microphysical profiles alongside observations across the entire fog life cycle.' However we add reference to Nurowska, 2025 in the introduction. They did analyse fog life cycle but only had thin fog.

3) Section 2.3 (Model set-up): clarify subgrid condensation treatment for 100 m vs 500 m runs. o p.7–8. o Suggestion: briefly state whether any of the presented results are sensitive to the (non)use of subgrid condensation (e.g., was a test done to verify negligible impact at 100 m?). If not tested, state as caveat.

Reviewer is correct. We did not carry out any tests on the use of a subgrid condensation scheme at the 100 m resolution, as was done in Smith et al. 2021. The sentence has been revised to:

"At the 100 m grid length, no subgrid condensation scheme was used; sensitivity tests could be performed in future studies, as in Smith et al. 2021."

4) Table 1 — make instrument uncertainties consistent and add CDP D-range explicitly in table column. o Table 1 (p.24). o Suggestion: add a column "measurement diameter/range" and show that CDP is 2–50 µm (already in text) so readers scanning the table see limitations.

It appears to us that the CDP-D range is already explicit in table 1.

5) Figure captions: add sample counts (N profiles) and units. o Figs. 7–10 (pp.32–36). o Suggestion: in each caption include "N = ... profiles aggregated" and ensure LWC units g m^{-3}, Nc cm^{-3} and averaging window used.

We thank the reviewer. The LWC and Nc units have been added to the legend. The number of averaged profiles is indicated in the legend.

6) When referring to "excessive LWC" give typical bias numbers (e.g., +0.15 g m^{-3}). o results p.14–17. o Suggestion: quantify the bias ranges (mean ± std) for each phase in a small table for clarity.

We thank the reviewer for the insightful suggestion. As indicated, the quantitative description appears in lines 421–460 for the overall comparison of the vertical profiles. Accordingly, we have added quantitative details for the vertical profiles and the phase-averaged DSDs related to the above table.

The following text have been added :

(IOP6) 'While the model captures the structure, it overestimates the LWC at all levels (mean bias: 0.17 ± 0.04 g.m−3), which is consistent with the wet bias identified in Fig. 3.'

(IOP11) 'While the model captures the profile shapes, it overestimates LWC (mean bias: 0.006 ± 0.04 g.m−3) due to the cold bias and significantly underestimates Nc.'

(IOP11) '. The model displays strong variation and overestimates both LWC (mean bias: 0.1 ± 0.09 g.m−3) and Nc (mean bias: 103 ± 43 .cm−3), particularly at the fog top.'

IOP 6

transition : 0.17 +-0.04 g.m-3 ; -22 +- 57 #.cm-3 (mean bias +- std)

diabatique : 0.09 +- 0.03 g.m-3 ; 38 +- 60 #.cm-3

dissip : 0.02 +- 0.02 g.m-3 ; -23 +-111 #.cm-3

IOP11

formation : 0.006 +-0.04 g.m-3 ; -42 +- 17 #.cm-3

transition : 0.1 +- 0.09 g.m-3 ; 103 +- 43 #.cm-3

7) Clarify the treatment of aerosol/radiative effects of aerosols. o model setup/radiation (p.8). o Suggestion: you note that aerosol radiative effect is not included — briefly discuss whether including aerosol optical effect would be expected to change LWnet/LWP diagnostics.

We thank the reviewer for raising this unclear point. We change the sentence to :

"Note that the radiative effect of aerosols is only considered trough a monthly climatology in these simulations"

Real aerosol loading can directly affect fog development during daytime, it may diminish solar radiation reaching the surface, cool the surface, and possibly cause earlier nocturnal fog formation. If black carbon resides above the fog or near the ground in daylight, it can absorb radiation, modify the vertical profile, strengthen the inversion or warm the surface when close to the ground, and thereby hasten or postpone fog formation.

Considering these points, we added the following sentence: “Although this is beyond the scope of the present study, future research should assess the direct and semi-direct effects of aerosols on fog lifetime, as has been done in Ding et al. (2019); Al Asmar et al. (2022).”

8) Grammar/typos: small issues to correct (examples): o p.1 line “realisically” → “realistically” (check spelling throughout). o p.12 “thin,to thick” → “thin to thick” (remove stray comma).

Corrected.

9) Make the “pink line” DSD exercise more visible (either move to main figures or add a small panel). o DSD discussion (p.16–18). o Suggestion: place a small explanatory inset showing the observed vs gamma- reconstructed DSD and the consequences for reflectivity.

We appreciate the reviewer’s suggestion. It appears that we have already addressed this point in major comment 3).

10) Clarify how CCN/SMPS data were averaged and applied to activation parameterization. o model set-up p.8. o Suggestion: state the time window used to produce the “mean activation spectra” and comment on representativeness when aerosol measurements were missing for the IOPs studied.

We thank the reviewer for raising this point. CCNC/SMPS data were averaged according to the method described in Mazoyer et al., 2019. As stated ' To specify aerosol loading, the characteristics of the activation spectra Cohard and Pinty (2000) were calculated according to the κ-Köhler theory (Petters and Kreidenweis, 2007) implemented in LIMA, using all data collected during the campaign with the SMPS (aerosol size distribution from 10.6 to 496 nm) and the CCNC (activation spectra from 0.06 % to 0.28 %), following the methodology in Mazoyer et al. (2019)'

CCNC completes a cycle every 20 minutes, whereas the SMPS provides a 5-minute time resolution.
Aerosol loading was characterized during the hour preceding the fog onset.
The FM-120 measurement has a 1-minute time resolution and was characterized during the hour after the fog onset.

An aerosol-loading sensitivity test (with the concentration halved), referenced in l613 of the paper and displayed in Figure R5 using non-definitive simulations, showed that reducing aerosol loading lowered Nc, with impact on LWC but little on the Nc profile. This change had a moderate effect on the fog life cycle. However, the DSD is shifted toward larger diameters, which raises the simulated reflectivity and brings it closer to observations, analogous to the test performed on the DSD shape parameter described in l641-644.

We add the following sentence in the SOFOG3D case studies section : 'Due to technical issues, aerosol measurements were not available for these dates, a sensitivity study to aerosol loading is consequently described in the discussion.'

One possible explanation on the moderate impact of aerosol of fog life cycle may concern how aerosols are represented in the model. Introducing a bimodal droplet distribution could enhance their impact, emphasizing either many small droplets (for top radiation) or fewer large droplets (for sedimentation). Indeed Boutle et al. 2018 used a bin microphysical scheme for aerosols and droplets while showing the impact of aerosol activation on fog development. Furthermore, the activation of very large aerosols— which are not considered in this study— as measured by the OPC in the game (500–1100 nm), could influence the fog life cycle if they are directly incorporated into a second mode of the DSD, as mentioned in lines 665-667.

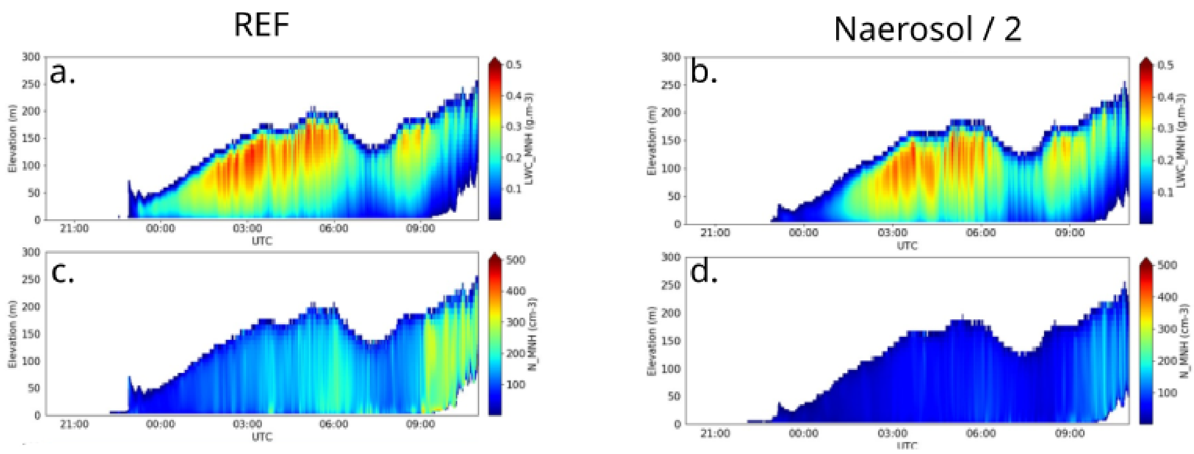

Figure R5. IOP 6: vertical profiles of LWC (a, b) and Nc (c, d) simulated with Meso-NH (a, c) and with the aerosol loading reduced to half the original concentration (b, d).

11) In Section 3.2 (radiometer/lidar): discuss the 0.5 K cold bias of HATPRO (already mentioned elsewhere) and its impact on phase classification. o p.9–11. o Suggestion: quantify whether shifting radiometer T by 0.5 K would change transition times.

The reviewer pointed out an interesting aspect: the cold bias of the HATPRO could affect fog-phase classification if temperature evolution were considered, but it is the gradient that we use because we are interested in atmospheric stability. Phase classification relies on a vertical gradient measured between 10 m and 50 m, with the soil point excluded. We except the bias to have little evolution between 10 m and 50 m following Martinet et al. (2022) (and Cimini et al. (2025)). Moreover vertical gradient is only an indicator among the four other. Considering five indicators allows to minor the instrumental of modeling bias impact.

12) Line referencing: when you give times for IOP phases in Table 2, add UTC offset and explicit dates to avoid ambiguity for readers. o Table 2 (p.24).

We appreciate the reviewer’s suggestion. It had been integrated.

13) Discussion: expand on whether LIMA’s activation parameterization (Vié et al.,2024 fully accounts for radiative cooling activation — you mention improvements but could be clearer. o : Intro and model description (p.4–8).

Thanks for raising this point. As stated l201-203 - 'Recent improvements in the CCN activation parameterization in LIMA, as developed in Vie et al., 2024, take into account the growth of existing cloud droplets by condensation.' but also temperature tendency term has been modified to only consider lagrangian process and exclude the advection contribution. Accordingly we add the following sentence : 'Recent improvements in the CCN activation parameterization in LIMA, as developed in Vié et al. (2024), exclude the advection contribution to the temperature cooling term and take into account the growth of existing cloud droplets by condensation, thereby mitigating the overestimation of cloud droplet activation.'

14) Data availability: the CDP files are "being finalized" — for reproducibility, give expected DOI or state that data will be provided upon acceptance. o Data availability p.23–24.

Corrected.

15) Reference to "Price, 2025" — verify final reference details (year 2025 cited for an in-prep source?) o several places (Intro, etc.). o Suggestion: ensure all cited works are available or clearly marked as "in press"/"in review".

Checked. We were referencing 'https://rmets.onlinelibrary.wiley.com/doi/10.1002/qj.4908' published in 2025.

References

BOUTLE, Ian, PRICE, Jeremy, KUDZOTSA, Innocent, *et al.* Aerosol–fog interaction and the transition to well-mixed radiation fog. *Atmospheric Chemistry and Physics*, 2018, vol. 18, no 11, p. 7827-7840.

Cimini, Domenico, et al. "Atmospheric stability from numerical weather prediction models and microwave radiometer observations for onshore and offshore wind energy applications." *Atmospheric Measurement Techniques* 18.9 (2025): 2041-2067.

Costabloz, T., Burnet, F., Lac, C., Martinet, P., Delanoë, J., Jorquera, S., and Fathalli, M.: Vertical Profiles of Liquid Water Content in fog layers during the SOFOG3D experiment, Atmospheric Chemistry and Physics Discussions, 2024.

Dione, C., Haeffelin, M., Burnet, F., Lac, C., Canut, G., Delanoë, J., ... & Toledo, F. (2023). Role of thermodynamic and turbulence processes on the fog life cycle during SOFOG3D experiment. *Atmospheric Chemistry and Physics*, *23*(24), 15711-15731.

Dominutti, P. A., Renard, P., Vaïtilingom, M., Bianco, A., Baray, J.-L., Borbon, A., Bourianne, T., Burnet, F., Colomb, A., Delort, A.-M., et al.: Insights into tropical cloud chemistry in Réunion (Indian Ocean): results from the BIO-MAÏDO campaign, Atmospheric Chemistry and Physics, 22, 505–533, 2022.

Fathalli, M., Lac, C., Burnet, F., and Vié, B.: Formation of fog due to stratus lowering: An observational and modelling case study, Quarterly Journal of the Royal Meteorological Society, 148, 2299–2324, 2022.

Geoffroy, O., Brenguier, J.-L., and Sandu, I.: Relationship between drizzle rate, liquid water path and droplet concentration at the scale of a stratocumulus cloud system, Atmos. Chem. Phys., 8, 4641–4654, https://doi.org/10.5194/acp-8-4641-2008, 2008.

Martinet, P., Unger, V., Burnet, F., Georgis, J. F., Hervo, M., Huet, T., ... & Thomas, G. (2022). A dataset of temperature, humidity, and liquid water path retrievals from a network of ground-based microwave radiometers dedicated to fog investigation. *Bulletin of Atmospheric Science and Technology*, *3*(1), 6.

Mazoyer, M. (2016). *Impact du processus d'activation sur les propriétés microphysiques des brouillards et sur leur cycle de vie* (Doctoral dissertation, Institut National Polytechnique de Toulouse-INPT).

Mazoyer, M., Burnet, F., Denjean, C., Roberts, G. C., Haeffelin, M., Dupont, J.-C., and Elias, T.: Experimental study of the aerosol impact on fog microphysics, Atmospheric Chemistry and Physics, 19, 4323–4344, 2019.

Mazoyer, M., Lac, C., Thouron, O., Bergot, T., Masson, V., and Musson-Genon, L.: Large eddy simulation of radiation fog: impact of dynamics on the fog life cycle, Atmospheric Chemistry and Physics, 17, 13 017–13 035, 2017.

Mazoyer, M., Burnet, F., and Denjean, C.: Experimental study on the evolution of droplet size distribution during the fog life cycle, Atmospheric Chemistry and Physics, 22, 11 305–11 321, 2022.

Nurowska, Katarzyna, Przemysław Makuch, and Krzysztof Mirosław Markowicz. "Measurement report: Microphysical and optical characteristics of radiation fog–a study using in situ, remote sensing, and balloon techniques." *Atmospheric Chemistry and Physics* 25.20 (2025): 13493-13525.

Price, J. (2011). Radiation fog. Part I: observations of stability and drop size distributions. *Boundary-layer meteorology*, *139*(2), 167-191.

Smith DK, Renfrew IA, Dorling SR, Price JD, Boutle IA. Sub-km scale numerical weather prediction model simulations of radiation fog. QJR Meteorol Soc. 2021;147:746–763. https://doi.org/10.1002/qj.3943

Vié, B., Ducongé, L., Lac, C., Bergot, T. & Price, J. (2024) Importance of CCN activation for fog forecasting and its representation in the two-moment microphysical scheme LIMA. Quarterly Journal of the Royal Meteorological Society, 150(764), 4217–4234. Available from: https://doi.org/10.1002/qj.4812