Response to Referee #2

This paper describes a case study of two fog events on two consecutive days in Nanjing, simulated by WRF-chem, and proposes that aerosol-fog interactions in the first fog promote aerosol-fog interactions in the second. Most of the hypothesis is reasonable: the first fog influences boundary layer turbulence for the second fog, and that influence is affected by aerosols. However, I am not yet convinced whether the hypothesis that *aerosol-fog interaction* in the second fog is affected by aerosol-fog interaction in the first is adequately demonstrated by the simulations in the paper.

Despite this, the paper describes a useful and interesting study of aerosol-fog interactions, which in itself is well worth publishing in ACP. It is also well structured and well written, in general. I recommend that the authors either perform additional simulations to test their hypothesis, they weaken their definition of self-enhancement, or they change the message of the paper to simply highlight aerosol-fog interactions in Nanjing. Either way, in my assessment the article needs major revisions, but assuming the major comments can be addressed, it would be suitable for ACP.

Dear Referee,

Thank you for your positive and constructive comments. We have addressed your comments and the corresponding replies are listed below. Briefly, we have performed additional simulations, weakened the definition of self-enhancement and changed the message of the paper to simply highlight aerosol-fog interactions in Nanjing, according to your suggestions.

With regards,

Naifu Shao, Chunsong Lu*, and co-authors.

Major comments

1. The authors' summary of their evidence for their hypothesis of 'self-enhanced aerosol-fog interactions' is that by increasing droplet concentrations and by postponing the dissipation of the first fog and promoting the earlier formation of the second, aerosols increase the fog thickness and prolong its lifetime.

Reply: Yes, we agree with you. "ACI under polluted conditions postpones the dissipation of Fog1 owing to these two feedbacks and generates PBL meteorological conditions that are more conducive to the formation of Fog2 than those prior to Fog1. These conditions promote the earlier formation of Fog2, further enhancing the two feedbacks and strengthening the ACI in Fog2" (Page 2, Line 34-37).

2. Figure 7 shows the meteorological differences that arise during the first fog between clean and polluted conditions persist into the second fog. This figure is key. But would these meteorological differences still persist if the second fog, and the period between the fogs, were not also polluted? Can the authors demonstrate that direct aerosol-meteorology interactions during the clear-sky period between the two fogs do not lead to the meteorological differences in Figure 7 and the early onset of Fog 2?

Response: Thank you for your suggestion. We think it is possible to reply to this comment by examining the meteorological conditions before Fog 1, instead of examining the conditions before Fog 2. The reason is that there is no fog before Fog 1; all the differences of meteorological conditions before Fog 1 is caused by aerosol-meteorology interaction, which was the question the reviewer asked. "As shown in Table 5, RH_{2m} and PBLH before Fog1 on 25 November under clean conditions are 76% and 669 m, respectively, similar to those under polluted conditions (76% and 670 m, respectively). Therefore, it is unlikely that aerosol-meteorology interaction leads to the meteorological differences in Fig. 7. In addition, a previous study (Yan et al., 2021) also noted that aerosol–fog interaction was more remarkable than aerosol–radiation interaction." The above discussions are added in the revised manuscript (Page 13, Lines 293-298).

3. Assuming the authors can demonstrate this, their theory is aerosol-fog interaction in Fog 1 changes meteorology which enhances aerosol-fog interaction in Fog 2. They show the first part of this in Figure 7: aerosol-fog interactions affect meteorology. It's reasonable that this influences the formation time of Fog 2 in the simulations. But does it also influence aerosol-fog interactions in fog 2? The authors do show aerosol-fog interactions are stronger in Fog 2 than Fog 1 in their table 3. However, the authors don't demonstrate a **causal link** between the increased strength of ACI from Fog 1 to Fog 2 and the ACI in Fog 1. To show conclusively the aerosol-fog interaction is 'self-enhancing' in the simulations as per their own definition, I think the authors would need to show that the aerosols in the first fog affect the aerosol-fog interactions in the second fog. In principle, this could be done with a third simulation, in which the first fog was clean and the second polluted. In this simulation, if the AFIs were weaker in the second fog than in the simulation in which both fogs were polluted, I think the authors' hypothesis would be confirmed.

Response: Thank you for your suggestion. We design the third simulation called EXP3, as you suggested. Fog1 occurs under clean conditions (5% of emission from the MEIC database) and Fog2 occurs under polluted conditions (the default emission from the MEIC database). In particularly, according to Fog1 dissipation time, clean condition are set before 11:00 LST on 26 November 2018 and polluted conditions are set after 12:00 LST on 26 November 2018. In Table 4, two fog events in EXP1 are both under polluted conditions. EXP2 represents that the two fog events are both under clean conditions. The response of fog optical depth to the change of droplet number concentration ($\Delta \ln \tau_c / \Delta \ln N_d$), from EXP2 to EXP3 is 1.17 in Fog2, smaller than 1.32 from EXP2 to EXP1. Therefore, the aerosol–cloud interaction (ACI) in Fog1 affects ACI in Fog2.

We have revised the simulation design accordingly (Page 7, Lines 164-168) and the analysis accordingly (Page 10, Lines 236-239).

Table 4. Quantitative estimation of ACI strength in two fog events (Fog1 and Fog2), including the responses of fog optical depth (τ_c), liquid water path (LWP), and fog effective radius (R_e) to the changes in fog droplet number concentration (N_d). In EXP1, both fog events occur under polluted conditions, and fog events in EXP2 occur under clean conditions. In EXP3, Fog1 occurs under clean conditions and Fog2 occurs under polluted conditions. The ratio represents the relative change between Fog1 and Fog2, calculated as (Fog2 − Fog1)/Fog1. In the fourth and sixth columns, Fog1 occurs under clean conditions in both EXP2 and EXP3.

	EXP1 vs EXP2			EXP3 vs EXP2		
	Fog1	Fog2	Ratio	Fog1	Fog2	Ratio
$\Delta\ln\tau_c/\Delta\ln N_d$	0.98	1.32	34.7%	–	1.17	–
$\Delta\ln LWP/\Delta\ln N_d$	0.76	1.08	42.1%	–	1.00	–
$-\Delta\ln R_e/\Delta\ln N_d$	0.22	0.24	9.1%	–	0.17	–

4. The authors also need to show how the absolute PM2.5 concentration varies with time through the two fog events (preferably both in simulations and observations). Otherwise, the results in Table 3 are not useful, as the AFIs might get stronger simply because aerosol concentrations get higher. Furthermore, for the same reason, it would be useful to show the timeseries of aerosol number concentrations (perhaps > 100nm diameter) in the two simulations.

Reply: The timeseries of PM$_{2.5}$ mass concentration and aerosol number concentration are shown in Fig. S4. PM$_{2.5}$ mass concentration is similar before Fog1 and Fog2 formation, and aerosol number concentration before Fog2 is less than that before Fog1 formation. Therefore, changes in aerosol concentration are not the main reason for the increase in aerosol-induced changes in the two fog properties. The above discussions are added (Pages 10, Lines 235-239), and Figure S4 is added in the supplement.

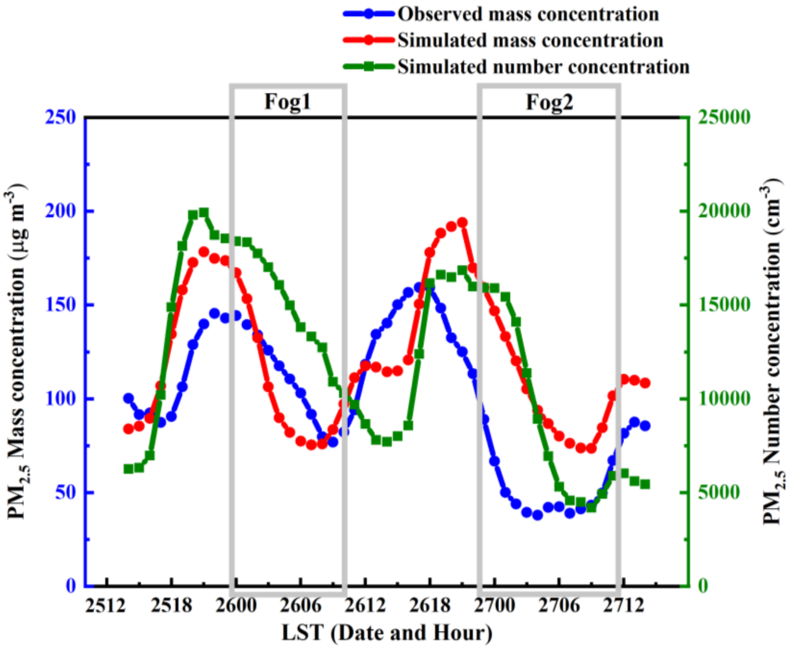

Figure S4. The timeseries of $PM_{2.5}$ mass concentration and aerosol number concentration in Nanjing (the blue line: observed $PM_{2.5}$ mass concentration, the red line: simulated $PM_{2.5}$ mass concentration, and the green line: simulated $PM_{2.5}$ number concentration). Fog1 and Fog2 in the light grey box are the two fog events. Time ‘2512’ indicates 12:00 local standard time (LST) (LST = Universal Time Coordinated + 8 h) on 25 November 2018. The other time expressions follow the same logic.

5. Figure 4 is very hard to interpret quantitatively. Is LWP from Himawari available as it is, for example, from MODIS, GOES or SEVIRI? Could it be used instead of the visible light images?

Reply: LWP is not available in Himawari products, but COD is available. The monitoring time of MODIS satellite is too late because fog events have dissipated. The monitoring range of geostationary satellites GOES and SEVIRI cannot cover the fog area in our study. Therefore, we use the COD products to replace the visible light images in Fig. 4.

We revised the sentences (Page 8, Lines 185-192): “Figure 4 shows the evaluation of fog spatial distribution. The simulated fog optical depth distribution is compared with the Himawari-8 cloud optical depth products and ground-based observations (black circles in Fig. 4) at 08:00 LST on 26 and 27 November 2018, respectively. Qualitatively, the spatial distribution and magnitude of the simulated fog are generally consistent with satellite and ground-based observations. Similarly, Lee et al. (2016) evaluated fog distribution simulations against satellite-derived cloud optical depth from satellite and concluded that the distributions of simulations and observations were generally comparable to each other.”

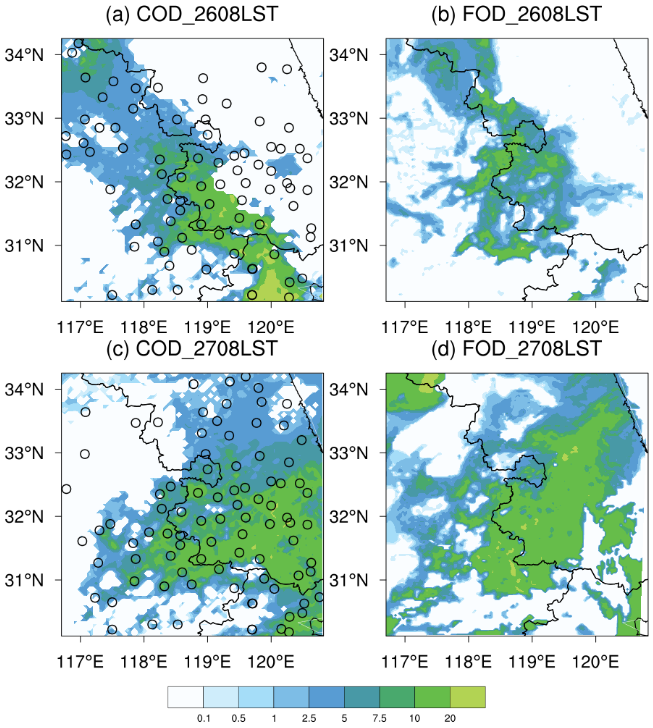

Figure 4. (a, c) Distributions of ground-based fog observations (the circular points) and cloud optical depth from Himawari-8 products at 08:00 LST on 26 and 27 November 2018. (b, d) Simulated fog optical depth distributions in domain 03 at the corresponding time of observations. Time ‘2608LST’ indicates 08:00 local standard time (LST) (LST = Universal Time Coordinated + 8 h) on 26 November 2018. The other time expressions follow the same logic.

Minor Comments

1. In the abstract the authors say "AFIs in the first fog…result in higher [droplet] number concentration …in Fog 2 than in Fog 1. For this to be true, my first thought was that AFIs in the first fog would have to reduce scavenging of aerosol and result in higher aerosol concentration in the second fog than would have been the case if the first fog hadn't formed. The authors don't show this. They do show that Fog 1 changes meteorological conditions, which might indirectly affect droplet concentration in Fog 2 by changing LWC in Fog 2, but starting the list at line 21 with droplet concentration rather than LWC implies (to me at least) that the main mechanism is an aerosol one: aerosol-fog interaction in Fog 1 affect aerosols in Fog 2, which then change droplet concentration in Fog 2, which then changes LWC and lifetime (the classic ACI pathway). The authors don't have any evidence for that (the mechanism is meteorology, not aerosols).

Reply: Sorry for the misleading sentences. We agree with the reviewer that the mechanism is meteorology, not aerosols. Therefore, we have revised the abstract (Page 1, Lines 21-25): "Our simulations indicate that the PBL conditions conducive to Fog2 formation are affected by ACI with high aerosol loading in Fog1; subsequently, PBL promotes ACI in Fog2, resulting in a higher liquid water content, higher droplet number concentration, smaller droplet size, larger fog optical depth, wider fog distribution, and longer fog lifetime in Fog2 than in Fog1."

2. Line 45 "proven" – I would say "showed" – a 'pivotal role' is not a mathematical concept so it is not really 'proved'.

Reply: We have revised the sentence accordingly (Page 3, Line 52): "The critical roles of aerosols and the planetary boundary layer (PBL) in these processes have been shown (Boutle et al., 2018; Niu et al., 2011; Quan et al., 2021). "

3. Line 70 – what is the ‘critical turbulence coefficient’? The reader should not need to look up the literature unless they are very unfamiliar with fog.

Reply: “The critical turbulence coefficient was the turbulence threshold for diagnosing whether turbulence suppressed fog or not. When the turbulence intensity within the fog did not exceed the critical turbulence coefficient, the fog persisted; however, when it surpassed its threshold, the fog dissipated (Zhou and Ferrier, 2008).” The above description is added (Page 4, Lines 83-86).

4. Line 115 -the innermost simulation still has quite coarse spatial resolution. How well can this resolve the turbulence? Is there a sub-grid cloud parameterization in the model, or does the Grell 3D cumulus scheme lead to sub-grid variability in fog?

Reply: Turbulence is parameterised in the planetary boundary layer scheme. Based on closure theory, turbulent fluxes are calculated from gradients and parameterised vertical mixing coefficients. Besides, the parameterised vertical mixing coefficient also affects the vertical distribution of meteorological elements by the heat diffusion equation. Considering the consumption of computing cost, this kind of planetary boundary layer scheme is widely used in mesoscale numerical models (such as WRF), though its accuracy is not as good as the large eddy simulation.

There is a sub-grid cloud parameterisation in the MYNN2.5 planetary boundary layer scheme, instead of the Morrision microphysics scheme or the Grell 3D cumulus scheme. The sub-grid cloud parameterisation is found in the reference paper (Chaboureau and Bechtold, 2002), which is consistent with the source code in MYNN2.5 planetary boundary layer scheme. The sub-grid cloud water content is derived from a function of the normalized saturation deficit. So, sub-grid cloud parameterisation is considered in our paper.

We revised the sentences (Page 6, Line 147-148): “Turbulence is parameterised in the MYNN2.5 scheme and there is also a sub-grid cloud parameterisation (Chaboureau and Bechtold, 2002) in the MYNN2.5 scheme.”

5. Line 165- what would be a perfect HSS score? Is the score calculated using each gridbox as input? Please be clearer about how this evaluation was done.

Reply: “A perfect HSS score is 1.0, indicating that simulations are identical to observations. We used the fog occurrence at the observation stations and the closest model grids as input for HSS score. In our study, the HSS score are 0.34 and 0.36 in Fog1 and Fog2, respectively, which are close to previous reports (Mecikalski et al., 2008; Xu et al., 2020; Yamane et al., 2010).” The above description is added in the revised manuscript (Page 9, Lines 202-205).

6. Figure 9: Is ‘fog optical depth per unit height’ the same as “average extinction coefficient through the fog”? It might help the reader to explain this in the caption.

Reply: Yes, ‘fog optical depth per unit height’ is the same as “average extinction coefficient through the fog”. We have revised the sentences in the revised manuscript (Page 45, lines 887-888; Page 15, Lines 337 and 344).

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