Dear Editor-in-Chief,

please find in this document our responses to the reviewer #1 of our manuscript.

We thank the reviewer #1 for his/her valuable and constructive suggestions, which led to significant improvements of the quality of our manuscript. Below we detailed how their comments are addressed in the revised version of the manuscript. The corrections made in the manuscript and cited in this document appear in italic.

Review of 'Role of thermodynamic and turbulence processes on the fog life cycle during SOFOG3D experiment',

by Cheikh Dione, Martial Haeffelin, Frederic Burnet, et al.

Summary:-

This paper presents an analysis of data collected during the SOFOG3D campaign in south-west France during 2019-2020. Four cases of fog, and their evolution are discussed with an emphasis on their adiabaticity, and how a conceptual model performs in nowcasting the evolution of each case.

The paper is relevant and interesting, but requires some significant clarification to the data and arguments presented. Some of the analysis presented is difficult to follow, overly complex, and the cause and effect of various processes may be confused. My recommendation is to publish after major revision.

Main points:-

- Description of instrumentation.

Regarding the Doppler Lidar data I would like to see more explanation of how the TKE is retrieved, with an estimate of uncertainty given. I believe that the Lidar must scan over a region of sky to retrieve 3D winds, which raises the likely-hood that air samples in the separate beams are not coherent. Can any independent verification of the calculation be presented here?

We agree with the reviewer on the need of more explanation of how the TKE is retrieved. We added the methodology used to compute the TKE based on Kumer et al., 2016. *"The TKE is computed as the sum of the horizontal velocity variances as in Kumer et al., 2016. Velocity variances are estimated every 30 minutes using the wind components at the high resolution."* Line 202-204

Kumer, V. M., Reuder, J., Dorninger, M., Zauner, R., Grubišić, V.: Turbulent kinetic energy estimates from profiling wind LiDAR measurements and their potential for wind energy applications, Renew Energy., 99, 898-910, https://doi.org/10.1016/j.renene.2016.07.014, 2016.

Secondly, since the Lidar beam is highly attenuated by liquid water, how much of the fog layer is actually sampled? The authors state between 40 and 220 m, but I believe this is the range of the lidar and not how far the Lidar can typically see into the fog? Other similar Lidars typically can see into around 100m of fog.

We gave the vertical range of the Lidar [40 - 220 m] as an manufactured information. This range can be reach in clear sky (see Figs. 4d, 6d, 8d and 10d). We are aware that the range of the Lidar is reduced by the liquid water content of the fog. This is visible on the wind height cross-section in figures (4d, 6d, 8d and 10d) and the discontinuity of the TKE time series in the 100-400 m layer (Fig 4f, 6f, 8f, and 10f). We further clarified the impact of fog on the vertical resolution of the Wind Lidar. *"The wind component are estimated every 10 minutes using a Carrier-to-Noise Ratio (CNR) at least -23 dB and a total data availability of at least 50 %. Note that the CNR depends on atmospheric turbulence characteristics and relative humidity (Aitken et al., 2012). In the presence of fog or low stratus, the Lidar vertical range become low. The TKE is computed as the sum of the horizontal velocity variances as in Kumer et al., 2016. Velocity variances are estimated every 30 minutes using the wind components at the high resolution."* Line 198-204

Regarding the Microwave radiometer, an uncertainty is given for absolute humidity, but not the LWP, which is the quantity presented in the figures. Please provide an uncertainty for LWP.

We agreed with the reviewer to add the uncertainty in the retrieval of the LWP. We added in the manuscript the followed sentences " *Martinet et al., 2022 showed that the LWP accuracy has been validated in clear-sky conditions and shown errors between 1 and 14 g m^{-2}. These error range is in the scope of that defined in the literature (Crewell and Löhnert, 2003; Marke et al., 2016)."* Line 184-186

Whilst the temperature error of the MWR is quoted as 0.5 degrees for the region of interest, it is clear that the profiles appear highly smoothed in the vertical (compared to what we expect to see from e.g. a tethered balloon profile). This might lead to erroneous conclusions regarding stability and phase of the fog. Were other sources of temperature profiles explored, such as radiosonde, mast or tethered balloon, before using the MWR data? It would be clearer if only the lowest 300m were plotted in the MWR temperature profile plots, and also if fog top were indicated on them at each time.

At the supersite, there was a tethered balloon with varying vertical ranges and temporal resolutions. Note that the temperature measured by this instrument are sensitive to water droplets deposition on the sensors. A few radiosoundings have also been launched at that site but their temporal resolution (3 hours) does not allow to characterize the different fog phases. The only instrument allowing to have a continue estimate of the atmospheric stability remains the MWR.

- Data analysis. Below are examples where I found the presentation of results requires clarification.

L319: - 'thermal turbulence': you mean, 'thermally driven turbulence'?

We mean by thermal turbulence, the turbulence generated by heating. We corrected in the reviewed version of the manuscript. *"The fog dissipation phase is induced by the increase of the vertical mixing generated by the thermal (solar heating) and mechanical turbulence associated with TKE values larger than 0.4 m^2 s^{-2} (Fig. 4f)."* Line 377-379

L389::- It looks from the figure showing the MWR data that fog starts becoming adiabatic from 2400 or 0100 hours which is inconsistent with the statement here?

We agreed with the review that for these case study, fog starts becoming adiabatic from 00:00 UTC as indicated in the figures illustrating this IOP. We reworded this part of the manuscript following the suggestion of the reviewer #2.

L393:- According to figure 6f sigma w^2 reaches 0.04 by 0300 hours, much more than the figure quoted.

σ_w^2 reaches 0.04 $m^2 s^{-2}$ during the fog adiabatic phase. In line 392, we explained the processes driving the fog stable phase. The values indicated refer to Table 2 of the manuscript.

L418-423:- I doubt the conclusion made here. The assertion is that a phase change in the fog caused a reduction in observed LWP, the evidence being 'frost' seen on the balloon cable. It is common to see ice and rime on such things when temperatures are below freezing in fog due to contact-freezing, but this is not evidence of ice or snow in the fog itself. Ice does not generally form in clouds until temperatures become much lower than seen here.

We understand the reviewer's doubts about the explanation we gave for the sudden loss of liquid water of the fog. Considering the evolution of the LWP, near surface temperature, the observations reported by the scientists operating on the supersite and weather reports from Météo-France at Biscarrosse and Bergerac located western (~60 km) and northeastern (~100 km) the supersite (https://www.ogimet.com/cgi-bin/gsynres?lang=en&ind=07503&decoded=yes&ndays=2&ano=2020&mes=01&day=06&hora=12, https://www.ogimet.com/cgi-bin/gsynres?lang=en&ind=07530&decoded=yes&ndays=2&ano=2020&mes=01&day=06&hora=12), we have explained this process by the formation of a freezing fog. We reworded this part following the suggestion of the reviewer #2. Line 399-403

Section 3.3:- generally difficult to follow. Examples below.

L455:- Fig. 8a indicates a weak inversion at 2100 before the stratus lowers into the fog.

Following the suggestion of the reviewer #2, we reworded this part of the manuscript by proposing a short description of the case studies 2, 3 and 4.

L458-459:- How does slowing down the cooling create a thin layer of temperature inversion?

We means "slowing down the wind speed" instead of "slowing down the cooling rate". We agreed with the reviewer that this sentence is unclear. We reworded this part of the manuscript following the suggestion of the reviewer #2.

L476:- Turbulence levels are very low for this case so how was the transition driven by turbulence?

The transition is driven by mechanical turbulence generated by the brisk winds (wind shear) observed at around 23:30 UTC. It is after that time that the turbulence started to decrease.

L458:- What is 'sustainable dissipation'?

We mean by "sustainable dissipation" " *a definitive dissipation of fog"*. We corrected the review version of manuscript. Line 450

L489. Why would a warming allow a deepening of the fog layer? I suggest the dissipation of this layer can be more simply put: Increasing wind aloft brought warm drier air over the top of the fog that then mixed into it, evaporating fog droplets, reducing RLWP to negative values and causing the fog to lift into low stratus.

We accepted reviewer's suggestions and reworded this part of the manuscript as *"Increasing wind aloft brings warm drier air over the top of the fog that then mixing into it (TKE = 0.33* $m^2\ s^{-2}$ *and* $\sigma_w^2 = 0.07\ m^2\ s^{-2}$*), evaporating fog droplets, reducing the RLWP to negative values and causing the fog to lift into low stratus. The fog dissipation phase is thus driven by the advection of warm air at the supersite. "* Line 427-431

L518:- 'triggering of the ultra-low stratus being the fog'. Why not just say this was 'stratus fog'?

Suggestion accepted. Line 477

L576:- Low stratus is not fog.

We corrected in the reviewed version of the manuscript as *"The combination of advection and radiative cooling favours stratus fog formation at about 150 m a.g.l followed by a rapid (less than 30 min) lowering of it base height to the surface triggering the onset of the fog in an unstable (case 3) and neutral (case 4) surface atmospheric boundary layer (Fig. 11c and 11d)"*. Line 476-479