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Comment on acp-2021-1027

Anonymous Referee #1

Referee comment on "Experimental study on the evolution of droplet size distribution during the fog life cycle" by Marie Mazoyer et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1027-RC1, 2022

General:

This paper examines a reasonably large data set of microphysical measurements made in fog at the SIRTA observatory on the outskirts of Paris, and describes the microphysical properties of those fogs, suggesting some implications for the numerical prediction of fogs.

The data set is of sufficient size to add useful knowledge to the literature and help to characterise surface conditions in fogs, and also support numerical studies of fog. However, I would like to see more analysis on the dynamic effects of the meteorology on the fog, in particular the effects of larger scale non-local dynamics. In Addition, certain important aspects of fog development do not appear to be discussed properly.

Main comments

Firstly, a fuller description of the SIRTA site and its surroundings would be useful (e.g. show a topographic map?). It is well known that stable boundary layer flows and fog are affected by topography, and at SIRTA we have the possible further effect of a change in roughness length between the urban and rural environments. These characteristics may affect the fog development and I believe should be discussed.

This leads onto my main point, which is that it has been well demonstrated in the literature that fog development is very often profoundly affected by larger scale (non-local) effects (indeed the authors refer to Duconge et al who discuss these in some detail), yet these do not appear to be considered much in this analysis. Instead, it appears to assume that all the fogs analysed have developed locally. However, examining data presented in their figures indicates there is likely to be a non-local element in their evolution.

For example, both figure 2 and figure 5 show sharp discontinuities in visibility, temperature (some converging to adiabatic from stable) and often winds, at fog onset. This is normally a characteristic of a well developed fog that has advected over the observation site, usually in the form of a gravity current. Clearly these events can complicate the interpretation of microphysical data and therefore should be discussed. If the authors wish to discuss the in situ evolution of microphysical processes, then clearly, choosing cases from their data base least affected by advection would be sensible.

In relation to the above, I believe that a common mis-interpretation of the data is that it is sometimes reported that the appearance of stronger winds and turbulence is responsible for the vertical development of shallow fog. However, in these cases, examination of larger scale data, when available, normally shows that the vertical development is associated with advection of a deeper fog over the site, bringing stronger winds and turbulence with it (i.e. the casual effect is opposite to that proposed). I believe this may be the case for F2, presented on page 6 (line 10).

Following on from these points I am not sure that adhering to the four-stage fog model presented (formation, development, mature, dissipation) is very helpful. For advecting fog cases it is misleading to label the arrival period as ‘development’ since that occurred elsewhere previously. Similarly for lowering stratus. I believe it would be more useful to relate microphysical changes to dynamic or thermal properties or their changes, within the fog. An example would be for the dissipation phase. Fog may dissipate through a variety of mechanisms; mixing, radiation, sensible heating, so it seems sensible to try to relate microphysical changes to these processes. It can also be noted that we expect to see microphysical changes at the surface due directly to dynamical processes. For example, in cases where a shallow stable fog evolves into a deeper adiabatic one, we normally see a significant change in the droplet spectra measured at the surface. This is due to the fact that in a stable fog, the greatest LWC is normally near the surface, but once transformed into an adiabatic fog the maximum LWC is higher up and the region next to the ground is normally then the least saturated region of the fog. Thus it is common to see a decrease in LWC (and sometimes N_d) at this transition. For these reasons I believe the paper requires a much more in-depth discussion of fog dynamics in order to interpret the microphysical data.

As a consequence, my feeling is that some of the conclusions made are not really supported by the analysis.

Regarding the observations there is no discussion of instrument errors or characteristics. Considering the Welas-2000, can the authors guarantee that the hydration of aerosols has not changed between ambient and sampling environments? I presume that any significant changes in hydration will have undesirable consequences for your analysis?

Also, the ‘wet-critical’ threshold of 3.79 microns seems rather precise, given the accuracy of sizing of the spectrometers (for the F100 I expect it will be a micron or two)?