

Response to the review by Reviewer 2

*General Comments*

*The article addresses a scientific question within the scope of ACP, namely, the role that hydrated aerosols perform in light extinction in mists and fog. The paper uses an analysis of field observations made during November 2011 near Paris to make its points. The field observations are analyzed to learn about the underlying physical processes. The paper provides an interesting analysis of these observations that has not been done before, on a topic and with a field campaign already in the literature. Other studies done on the data collected during this field campaign are cited in the paper. The limitations of the instrumentation are discussed in depth and uncertainty quantification is presented. In general the structure and content of the paper is sufficient to communicate the authors' methodology, results and conclusions. Some clarifications are requested in the specific comments and presentation quality is addressed in technical comments.*

*Specific Comments*

*p. 7, lines 7-10: the sentence beginning "Consequently" is unclear, in particular the end of the sentence.*

Due to the general agreement between the particle counters and the diffusometer, we eventually chose to neglect the discussion on this possible under estimation by WELAS.

*p. 10, lines 1-4: provide more detail and reasoning on the contribution of the items listed to the 30% uncertainty estimate.*

We refer to computations made by Elias et al. (2009) which estimated a 30% uncertainty. Discussions are provided by Wex et al. (2002) on the impact on some of the aerosol properties. Uncertainties on scattering by Mie theory were evaluated to be  $\pm 20\%$ , and on absorption to be  $\pm 30\%$ . Discussions are also provided by Chen et al. (2012) which estimated 34% uncertainty on extinction.

*p. 10, first full paragraph: What is the instrument accuracy over the whole range? The instrument that provides the largest values is presumed to be right. Is this still likely an underestimation? Are certain ranges more likely to be accurate than others even within whichever instrument is chosen as "correct?" Are there ever cases where the instrument giving larger values is not the one with the greatest sensitivity in that region? Please clarify this paragraph and perhaps cite some more papers specifically dealing with the performance of the two instruments. There are selected bibliographies provided on the manufacturer website for the FM100 at least as a place to start:*

*<http://www.dropletmeasurement.com/products/ground-based/FM-120>*

We did not keep this sentence “the instrument giving largest values is assumed ... its greatest sensitivity”. As shown by Elias et al. [2009] and also suggested in new Fig. 1, WELAS alone could not reproduce extinction in fog, because of underestimation of the droplet size distribution. Given the agreement between WELAS and the diffusometer, and as suggested by the new Fig. 1, FM100 seems not reliable for smallest particles. Other details on accuracy depending on the size range are not treated here, due to the general agreements we found.

However data were screened out of situations with rain and drizzle, and of shallow fog conditions, as discussed in Sect 4.1. We also discussed the variability of the aerosol contribution to extinction by studying the impact of the main fog formation process.

Few references exist on WELAS. Advised papers on FM100 were read and more references added in the text, especially Spiegel et al. 2012.

***P. 11, Figure 1 description: Are these single 5 minute sampled values at the UT listed? Or are they averaged over the 15 minute period mentioned in Section 2? Are these meant to show different characteristics of the different fog events or is this level of variability also seen within the individual fog events? Why is only one of the instruments included in the plots?***

Only the WELAS size distributions are shown in Fig. 2 (old Fig. 1), because the WELAS alone is sufficient for the distinction between aerosols and droplets, covering the appropriate range from 1 to 10  $\mu\text{m}$ . Size distributions by both instruments are now shown in new Fig. 1.

As specified in Section 2.1, all data are averaged at 15 minutes. Even the size distributions of Fig. 2 (old Fig. 1) were averaged over the 15-minute period. Fig. 2 is meant to show the variability between fogs, while the variability in individual fog is now showed in new Fig. 1 (3-hour averages are used only in new Fig. 1).

***p. 12, lines 1--3: this is unclear. Why could the 1 km convention not be applied in order to distinguish between the two types and then the events further stratified by the droplet presence? This could be used to make a conclusion about the accepted definitions of fog/mist. The reasoning becomes clearer as the section continues but there should be a clear, succinct statement here, and the phrase “could not be applied” is misleading.***

First sentence was erased for clarity, and we rewrote some parts of the Section.

Literature does not show a net limit in visibility for fog definition. 1 km is usually accepted but some authors also define light fog or evolving fog at visibility  $> 1$  km. Consistently, with the LWC threshold, and therefore droplet presence, visibility could be larger than 1 km in fog.

With the 1-km definition, droplet cases would be included in the mist regime, that WELAS alone could not reproduce properly. Consequently, the agreement between WELAS and the diffusometer in mist would be eroded.

***p. 12, lines 9-10: Clarify this is because of the uncertainty in the available measurements in this field study and not because the uncertainty is too high in a general scientific sense.***

We added the precision that the uncertainty is too high with measurements made AT SIRTA. Indeed

Gerber [1991] for example presents measurements with sufficient precision.

***p. 13, lines 7-8: If the FM100 did not provide the correct LWC, what does that mean for its reported size distributions during these times?***

It is expected that under-estimation of LWC is associated with under-estimation of pec, but with a negligible impact on the slope between optical counters and the diffusometer.

***p. 18, lines 11-13: If filtering is being described here (wording “eventually agreed” makes me think that) please make it much clearer and more specific.***

We now mention that filtering in Section 2.

***Conclusions are clear and well stated.***

#### ***Technical Comments***

***The article has many grammar mistakes and some unclear language and sentence structure. There should be extensive revision to correct this deficiency. A few examples are provided here but the list is not exhaustive.***

***Examples:***

***p. 2, line 5: “which are the most efficient to interact” should be “most efficiently interact”***

***p. 11, line 8: “varying” should be “vary”***

***p. 11, last line: “chart flow” should be “flow chart”***

***p. 13, line 28: “associated to” should be “associated with”; remove “however”***

We corrected the mistakes you mention, and we revised the grammar of the paper.

***Other technical comments:***

***Table 1: Uncertainty is missing for the FM100 and the CPC.***

Values were added.

***Table 5: The columns don't line up correctly so it is hard to immediately tell which method goes with which columns.***

We will be careful with that matter during the editing process

***Figures 1,4,5,6,7,10: Increase font sizes for axes labels and tick marks.***

Font sizes were increased.

Wex, H., C. Neusüß, M. Wendisch, F. Stratmann, C. Koziar, A. Keil, A. Wiedensohler, and M. Ebert, Particle scattering, backscattering, and absorption coefficients: An in situ closure and sensitivity study, *J. Geophys. Res.*, 107(D21), 8122, doi:10.1029/2000JD000234, 2002.