Discussion of APC-2014-750:

Summary -

This paper deals with the optical extinction due to hydrated aerosol in mist and fogs, which is a topic already dealt with previously by others in the literature. However, this paper is significant, because it presents additional evidence of this effect for a larger selection of mists/fogs in a near urban environment near Paris, France.

The scientific quality is generally good in that it deals reasonably well with two particle spectra probes that demonstrate inaccuracies over their sensitivity ranges. Comparison of extinctions integrated from the particle spectra with co-located visibility measurements shows reasonable agreement on the average supporting the extinction conclusions.

The presentation quality can use improvement because of the use of the English language. Sentence structure needs help, and word usage has issues. This leads to difficulty in understanding in places what the authors had in mind. A careful review of the paper by someone more proficient in English is recommended.

Discussion Points -

1. The calculation of pec_M sums up the contribution from the WELAS and the FM100 particle size spectrometers, with the latter only using part of the size contributions larger than 7 um, because the FM100 is thought to underestimate small drops according to the paper. There is a strong contribution of fog drops to the ratio pec_M/pec_k calculated and discussed at length in the paper, so that the FM100 measurements need to be accurate. First, Table 1 needs an uncertainty value which is left out for the FM100. Second, the use of the FM100 data in this paper should be described taking into account the thorough evaluation of the this probe by Spiegel et al., 2012: Atm. Meas. Tech., 5, 2237-2260. Further, this evaluation should also be applied to the given pec ratios and their uncertainties.

2. Pg. 297, lines 5-10: It is not clear to which of the two particle spectrometers this section refers to. This becomes clearer on pg. 300, lines 27-28. The clarity should already appear on pg. 297.

3. Pg. 300, lines 13-14: “Instrument giving largest values is assumed to provide the most reliable measurements” This assumption on the choice of WELAS vs FM100 data made by the authors is risky and should be removed unless it can be backed up with some evidence.

4. Pg. 302, lines 7-10: “The uncertainty is too high to use RH to detect aerosol activity”. This is incorrect, since accurate RH measurements between 95% and 105% have been made some time ago in fog, see Gerber (1991: JAS, 48, 2569-2588). The saturation hygrometer used for those measurements had an accuracy of ~0.02% at RH=100% and response time of a few seconds. A couple of other papers by Gerber are listed in the 1991 paper describing even earlier such fog measurements and details of the hygrometer design.

It is interesting to digress a bit and note that the closely related and published paper on Paris fogs by Hammer et al., 2014: ACP, 14, 10517-10533, in which some co-authors of the present paper also appear, does list Gerber (1991) in the reference list, but not in the text. Perhaps the comment in the Hammer paper that “short supersaturation spikes - - - - are irrelevant” was addressed to the findings of Gerber (1991) where RAD/EVP fogs showed SS transients and droplet spectra with sizes up to somewhat larger than 10 um; even though the fog had a mean RH ~ 100%. Hammer et al also note that "...cooling of air parcels below dew point results in formation of cloud or fog". The fogs in Gerber (1991) appeared to form differently by mixing near-saturated parcels at different temperature causing supersaturations. This raises the questions: How relevant are the values of SSpeak discussed in Hammer et al when turbulence and mixing dominates fog formation as in Gerber (1991)? See also, for example, Rodhe (1962: Tellus, 14, 49-86) for this fog-formation mechanism. Is it necessary to know the fine details of fog formation including SS, or is the use of SSpeak sufficient to produce realistic droplet spectra? A good test is to use SSpeak values and CCN spectra to calculate fog droplet spectra and compare them to <u>accurate</u> measured droplet spectra. Unfortunately, the latter is still appears to be somewhat of an issue for ground-based measurements. It seems more effort is needed to properly address relationships between CCN, hydrated mist particles, fog droplets, and fog (and cloud) dynamics.

5. Pg. 303, lines 7 -10: Unclear what is meant here.

6. Pg. 303, lines 3 -4: Unclear.

7. Pg. 304, lines 20 - 21: Unclear. Is "LWC factor" larger or lower?

8. Pg. 304, lines 23 - 25: Rewrite. Are you trying to indicate a range from 93% to 99%?

9. Fig. 1: This figure shows a plot of measured WELAS volume size distributions and fitted log-normal curves. The fitted curves show a dip identified in the paper as the transition particle size between hydrated particles and the first fog drops. Are the transition sizes described in the paper from these volume spectra? Is so do they not overestimate the actual transition sizes? Hammer et al uses a similar approach but surface area spectra for which they correct an overestimate of the transition size. Do you need to do the same for your sizes?

Given your comments about the shortcomings of both the WELAS and FM100 spectrometers, it would be desirable to show a comparison plot of the two probes which the paper does not have; only the WELAS spectra are shown. Such a comparison is especially pertinent for the FM100 spectra which must contribute heavily to pec_M.

The font x and y axis legends on the plots in Fig. 1 are too small.

10. Fig. 2: The lettering appears too small.

11. Fig. 8: Looks like your data points on this plot are for RH > 99%. Is that realistic?

12. Table 3: The lettering might be too small.

13. Table 5: Include a notation that the values in this table are average values.