General Comments

Interesting work, pretty well written and focusing aspects not so common in literature. The main goal is to demonstrate the feasibility of a new instrument, lidar based, to monitor the vertical profile of Liquid Water Content (LWC) in fog. In this perspective, a 1970s support algorithm is verified and the problem of fog droplet effective refraction index is addressed. In my opinion the measurements used in this work allow a discussion on other arguments also, for example a statistical analysis of fog particle size distribution (PSD), especially considering what reported in section 2 on composite PSD respect measures. Of course this is beyond the scopes of this work and should be considered as a suggestion for a future one. Two are the principal limitations of this work, both of them correctly addressed. The first one is that measurements where acquired in a single site: this limits the validation of the proposed algorithm. The second one is that refraction index discussion is based on mathematical analysis and not on field measurements: this approach is acceptable within the purposes of the work, and can give interesting insights on the argument. My principal concern is relative to verification of the 1970s linear relation between LWC and extinction coefficient: reported data allow affirming that relation hold at 11 μm but at 4 μm is more problematic. In the perspective of a new instrument, this could not be a real problem; nevertheless paper discussion on this limit have to be improved, and in same extents conclusions reviewed.

Specific Comments

- Abstract - Quite effective, but I disagree with the comment on refractive index sensitivity to radius, because figures and discussion suggests just the opposite (sensitivity is bigger within a range at smaller radiuses, while is near null at bigger ones).
- Section 1 - In my opinion is a little too much focused on airport applications; it could be nice to indicate also other examples (e.g. agriculture, car traffic…)
- Section 2 - I suggest authors to uniform instruments description notations (e.g.®, caps, acronyms, italics). The distinction between "real" and realistic PSDs in my opinion is not necessary, while it should be improved the description of the PSDs selection procedure, for example indicating the other support instruments (e.g. how do you determinate fog type and optical thickness, as said on pg. 9628 line 21?). Also "manually" fitting is quite arguable. Finally, should be indicated that the two instruments are not only calibrated but also time synchronized, to justify PSD combine and table I. As indicated in "General Comments" a PSD statistical analysis could be object of a very interesting future work.
- Section 3 – All units have to be indicated. A reference for extinction efficiency definition should be cited while applicability conditions for eq. 4 should be more detailed. The way in which W is obtained should be described also in the text. Finally I have some problems with eq. 7: can you check?
- Section 4 – References indicated at the beginning of the section are suitable, nevertheless in my opinion could be indicated also one or two review works. Affirmation on fog composition (pg. 9632, line 7) should be supported by a reference. Description of last figure on pp. 9632-9633 is not sufficiently clear, especially considering that figure shows water content and not radius. Description of the way in which the refraction index values are computed should be reported also in the text. Saying that at these wavelengths fog absorption dominates extinction (pg. 9633, line 13) is quite arguable, as indicated by the work itself. Discussion on figure 11 should be improved, because interpretation is not as simple as reported. Comment on pg. 9634 line 10 should be supported by a

reference. Finally I suggest authors to add an equation linking water content, extinction coefficient and refraction index, to improve understanding of this section.

- Section 5 – Comment on pg. 9635 line 2 should be supported by a reference
- Fig. 12 – In my opinion, plot should be fully changed, because unclear and with a wrong font. I suggest authors to plot all the 5 curves on the same graph (i.e. a single plot instead of four plots). Legend should be revised accordingly.

Technical Comments

- Pg. 9633, line 5: The correct figure is Fig. 8.
- Pg. 9634, line 5: I suggest citing Table 2 also.
- Pg. 9635, line 8: LWC misspelled (LCW)
- Table I – Study cases should be ordered.
- Figs. 2,3,4,5 – In my opinion legends should be improved
- Figs. 6, 7 – Radius range is excessive and lead to think that error is big and near unacceptable. I suggest authors to give more evidence to the effective realistic radiuses.
- Fig. 8 – Wavelength should be indicated
- Figs. 8,10 – Plots are not so readable, I suggest to add grids at least
- Figs. 8, 9, 10, 11 – Plot legends should be different for dots (figs. 8, 10) and lines (figs. 9, 11).
- All figures – If possible, all exponents should be corrected