**Interactive comment on "Ceilometer aerosol profiling vs. Raman lidar in the frame of INTERACT campaign of ACTRIS" by F. Madonna et al.**

**Anonymous Referee #2 Received and published: 30 January 2015**

► ***The authors of the manuscript "Ceilometer aerosol profiling vs. Raman lidar in the frame of INTERACT campaign of ACTRIS" by F. Madonna et al. would like to thank the anonymous reviewer #1 for his/her detailed review and its effort to improve the quality of the text and of the content of the manuscript in general. In the following, the authors provide an answer to all the reviewer's comments.***

Several types of ceilometers are widely distributed over the globe and have the potential to be used for quantitative studies of atmospheric aerosol properties. In this paper, the capabilities of three common ceilometer types are investigated by comparing their aerosol profiles to the ones of an advanced Raman lidar system during the inter-comparison campaign INTERACT conducted at CNR-IMAA Atmospheric Observatory in Potenza, Italy. This campaign lasted for about 6 month so that a sufficient number of observations appear to be available for this study.

The paper concludes, besides an overall agreement of each ceilometer and the lidar in aerosol properties, that there are discrepancies: 1) in the temperature stability of the ceilometers, 2) the water vapour interference for two of the instruments, 3) the stability of the overlap function in one case, and 4) they found that the difference in backscatter coefficient between the lidar and the ceilometers is proportional to the value of the retrieved backscatter coefficient.

However, these conclusions drawn are of more describing nature, than numbers and error bars.

► ***Conclusions have been modified to report all the numbers and the outcome of the statistics largely discussed in the text, but discussed in the conclusion section of the old version of the manuscript in terms of an overview of the results.***

***The manuscript is developed on a quantitative basis, with limitations due to lacking of access to the whole processing chain for the investigated instruments. Now, the conclusion should highlight in more details the quantitative numbers discussed in the text***

Detailed comments sorted by page and line:

p.12410, l.29: VAISALA or Vaisala?

► ***Vaisala, also following the comments of the reviewer #1.***

p.12411, l.18: The "idiosyncrasies of cloud base detection" is not studied in this paper.

► ***Removed.***

p.12411, l.25: Again: VAISALSA p.124114, l.27. Overlap for MUSA is 405 m or 330 as in Table 1?

► ***Overlap for MUSA is 405 m, corrected in the text.***

p.12415, l.17: normalisation constant – normalization factor may be more appropriate in this case.

► ***OK.***

p.12416, l.18: roto-vibrational Raman signal: of the lidar, I suppose. This may be confusing.

► ***This is now fixed.***

p12416, l.21: How is the calibration factor automatically selected by the ceilometer? I guess you mean the amplification of the detectors sensitivity for adjustment of the signal to the atmospheric conditions.

► ***Calibration factor is referred to the internal gain.***

p.12417, l.23: use: attenuated backscatter coefficient

► ***OK.***

p.12418, l.22: Zˆ2 is missing in equation (2), compare to equation (1).

► ***Corrected.***

p.12421: l.15-l.31: So, what is causing the large variability of 100% ? Did you find any reason besides the small WV contribution?

► ***Following also the suggestions provided by the reviewer #1, the section about the overlap stability has been structured in a different way to avoid the mixing of the variability of the ceilometer profiles due to the normalization on the lidar profile with the variability due to the overlap function.***

***To avoid any misinterpretation of the section, the authors have decided to limit the section to discuss two examples that show the differences between MUSA and the ceilometers as well as the instability in the region of incomplete overlap. To the authors opinion, these examples can still give a clear idea of the instability occurring in the region of incomplete overlap, though a statistical assessment of this instability is postponed to an expected follow-up paper also mentioned in the conclusions.***

***The authors think that in this way the section will be more in line with the reviewer's expectations opening the way to future and deeper studies on the topic.***

P12422, l.8: rather use overlap 'function' here.

► ***Corrected.***

p.12425, l.1: backscatter coefficient

► ***Corrected.***

p12425, l.9: Transmittance is T

► ***Corrected.***

p.12426, l.8: CIAO Lidar = MUSA

► ***Corrected.***

p.12428, l.22: backscatter coefficients, see comment above.

► ***Corrected.***

p. 12428-29: the advantage of the use of particle extinction at 355 nm instead of beta is not clear, see also Fig 11.

► ***As already done for the reviewer's #1 comment, the authors want to stress that the use of the extinction coefficient instead of the backscatter coefficient at 1064 should be preferred as retrieved from the Raman signal with very low assumptions. Moreover, the scope of the comparison, as already explicitly mentioned in the manuscript is (we cite): "The relationship between the 355 nm aerosol extinction coefficient provided by MUSA and the attenuated backscatter ß' obtained at 1064 nm by MUSA and by the three ceilometers, respectively, have been compared (Figure 11) to further investigate the ceilometers' performance and their sensitivity to different aerosol types, i.e. different extinction coefficients. "***

***Actually, also the outcome of the comparison reveals how the performances of the ceilometer might be influenced by the observation of different aerosol optical depths typically of the South Italy in summer or by temperature interferences and/or the insufficient dynamic ranges of the systems.***

Fig. 1 is not used in the text.

► ***Now, Fig.1 is mentioned in the manuscript.***

Fig. 2: The colours in the left panel seem to be mixed-up.

► ***This is fixed in the new version of the manuscript.***

Fig. 5: The legend outside the figure (TITO) is not necessary, since its only one data series, IWP, shown here.

► ***The legend outside the figure (TITO) has been removed.***

Fig. 9: Would an overlap correction really account for the discrepancies in the lower altitudes for the CHM15k? Backscatter coefficient

► ***See previous comments about the section of the manuscript dealing with the overlap.***

***Finally the authors did their best to improve the quality of the axis labels on all the figure of the manuscript, in agreement also with the comments of the reviewer #1.***