

Interactive comment on “Urban aerosol size distributions over the Mediterranean city of Barcelona, NE Spain” by M. Dall’Osto et al.

Anonymous Referee #2

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This work discusses about the source and time variability of atmospheric aerosol particle number concentrations in Barcelona. Thanks to the extended dataset of hourly averaged size distributions two different techniques (K-means clustering and Positive Matrix Factorization) have been applied. providing interesting piece of information. The paper is clearly written and properly supported by summary Tables and Figures; supplementary material provided online is useful for a complete understanding of the results, as well. However, there are a few points that need some explanation, Overall I believe that the paper is worth publication after minor revisions that take into account the following comments.

Page 16462, line 7: “a software programmed at the JRC”. I presume that JRC is the EU Joint Research Centre: maybe it'd better to avoid the acronym.

Page 16462, lines 7-8: “scans of seven minutes each” Does it mean that you have a complete size distribution every 7 minutes?

Page 16463, line 6: “with DIGITEL PM1025 inlets”. I suggest using “PM10 and PM2.5 inlets”

Page 16463, Section 2.4. If I correctly understand PM mass concentrations were derived as follows: first, 1-hour PM mass readings from Grimm 1108 instrument have been averaged to obtain daily mean values; then, these daily values have been compared with concurrent gravimetric data from Digitel instrument; finally, the C-factor in the Grimm instrument was determined based on best data fitting. I guess that this procedure (if I got the point, of course!) was separately repeated for PM10 and PM2.5; the question is how did you calibrate PM1 without PM1 gravimetric data?

Page 16464, lines 8-9: For the reader’s ease it would be useful to provide some information (and reference) on the meaning of the 0.20 values obtained for the Hopkins Index.

Page 16464, Section 2.7.1: “were subsequently normalised by their vector-length”. Does it mean that size resolved concentration data have been normalised for the total number concentration? It is not clear whether for clustering purpose only DMPS data have been used or also meteorological and criteria pollutant data have been used altogether.

Page 16464, lines 10-13: “The Dunn-Index for the results of the K-means analysis for different cluster numbers showed a clear maximum for 14 clusters. By carefully looking at the clusters, these were reduced to 9 as the difference among some of them was minimal.” The 9-cluster solution the authors present in the result section looks reasonable; however, I find a some mismatch in the abovementioned sentence while stating “clear maximum” and “minimal difference”. What’s the Dunn-Index for a 9-cluster solution?

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Page 16464, line 25: "...only 5000 can be cluster analysed at a time.." Please check this sentence. Probably, I did not get the point. I was expecting that the PMF was applied to each single size distribution, regardless for the results of the cluster analysis.

Page 16465, lines 13-15: "A less distinct enhancement is also seen in the afternoon, likely to be due to the photochemical nucleation events occurring during summer time". To better support this latter sentence I suggest to provide Figures for the temporal profiles on seasonal basis (cold and warm season). Furthermore, an enhancement apparently occurs for particles in the 30-40 nm range too; have you any clue for this behaviour?

Page 16465, line 20: "both high volume (V) and particle number (N) distributions". Actually, table 2 reports particle mass concentration data and not particle volume data. I suggest replacing "volume" with mass"

Page 16467, line 20: "They lie towards the top right of the N15–50 vs. NO_x plot". It should be N13–50

Page 16470, line 10: "observing the onset of much larger increases of Q." Q is not defined: I suggest avoiding the symbol and explaining what Q stands for.

Page 16472, lines 23-27: In table 5 the authors compare the relative contributions to the three modes obtained by PMF and by lognormal fitting of k-mean clusters size distributions. Actually, for these clusters the authors could also provide the measured relative contributions for the three size intervals.

Page 16472-16473: I suggest inserting a comment concerning the results of this study concerning the sources affecting particle number concentrations with those obtained by the cited work of Pey et al., 2009, where 7 emission sources were recognized to have influence on size-selected particle number concentrations.

Page 16482, Table 2: Why do you present N13-50 only? Particles below 100 nm are usually considered as UF, why not presenting more data for the particle number

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concentration? (for instance: N13-50, N50-100, N100-800 or N13-50, N13-100, N13-800)

Page 16492, Figure 7: I suggest to keep the same scale for the Score axis. For the reader's ease, I suggest to explain what the wind roses report.

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