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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Notably improved inversion of Differential Mobility Particle Sizer data obtained under conditions of fluctuating particle number concentrations" *by* B. Mølgaard et al.

Anonymous Referee #1

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General comments:

The Authors present an algorithm, providing the code with data, to improve the measurements of particle number size distribution for DMPS devices, especially for the ones deployed in urban sites where the size distributions are more likely to fluctuate due to the vicinity of the particle sources. They propose to use a Gaussian process to model the evolution of the particle size distribution during each scan. Overall the manuscript is clearly written with a detailed example in the introduction that makes clear, since the beginning, the main strength of the work. The results show more reliable measurements than the measurements obtained using a constant particle size

distribution during each scan in a situation of particle size distribution fluctuation. Moreover, the idea is interesting, reasonable and it is within the scope of AMT. The results are well explained. However, the presented results are limited to a few hours during two days (26th of February 2015 from 3:40 UTC+2 to 3:50 UTC+2, and 2nd of March 2015 from 10:30 UTC+2 to 12:00 UTC+2), besides they used this instrumentation for two weeks.

My suggestion is that the paper is accepted for publication in AMT, pending the following changes and responses:

1) In Section 2.5.1 (Data from the SMEAR III station in Helsinki), the authors say that "At some wind directions, traffic emissions affect the sampled aerosol substantially" (lines 3-4). It would be informative to complement the results figures with wind direction and wind intensity plots. I checked the SMEAR project web page, and the wind information seems to be available. More in details, I would like to see the wind plots for the whole day of 2nd of March 2015 (to link with Figure 1), and a detailed one for the 2nd of March from 10:00 UTC+2 to 12:00 UTC+2 to be linked with Figures 2, 6 and 8-10. For me, the wind plots can be placed either in the manuscript or in the supplementary material.

2) I expect to see correlations between wind measurements and high UFP number concentrations (from CPC) in rush hours. I would also like to know if there are other cases during the two week of measurements, in which the authors found similar situations of the wind and high UFP number concentration (from CPC) especially in rush hours or in periods of intense traffic. If yes, I would like to know how their algorithm works and, if the results showed in the paper are an isolated case or not. For example, in Figure 1 at around 6:40 UTC+2 there is a yellow spot at the same Dp[m] of the light blue spot that you have considered in your discussion. Is this a similar case, in which the old algorithm performs worse than the new one?

3) For a better comparison between the new and old algorithm, I would like to see

Figure 6 complemented with an extract from Figure 1 with the same time intervals (10:30-12:00 UTC2), same Dp[m] and same color code for the concentrations.

Minor comments:

Abstract:

Page 10284 line 11: "located in Helsinki" should be "located at an urban background site in Helsinki" (to be consistent with the Methods Section). Page 10284 line 14: "the overall agreement was good", please quantify this expression (correlation for the whole period is 0.98 for the new algorithm and 0.97 for the old one).

Introduction:

Page 10285 line 24: "appears to be low (Fig. 2)" please add a link to the light blue spot (low concentration) in Figure 1, if I am correct.

Methods:

The authors used a stationary covariance function in a non-stationary process. They are aware of this limitation as discussed in the Result and Discussion Section (page 10299 lines 26-29), and they state that implementing a non-stationary covariance function with the desired features is not straight-forward. I agree in the difficulties of implementing such covariance function. However, for the future works, I would suggest the authors to test a covariance function based on the addition of a linear and rational quadratic functions. The sum of two functions allows to model the data as a superposition of independent functions representing different structures. The linear function is a non-stationary (it neither depends on the relative position of the two inputs nor their absolute location). The rational quadratic function can be seen as a sum of many squared exponential functions with different characteristic length scales and can accommodate several length-scales (see the Mauna Loa Atmospheric Carbon Dioxide example in Chapter 5 in Rasmussen and Williams (2006). Moreover, Duvenaud et al. (2013) proposed a grammar and an automated search for the covariance structure

using different base functions.

Ref: Duvenaud, D., Lloyd, J.R., Grosse, R., Tenenbaum, J.B., Ghahramani, Z., 2013. Structure discovery in nonparametric regression through compositional kernel search. In: Proceedings of the 30th International Conference on Machine Learning, pp. 1166e1174.

Page 10293 line 4: "This reconstruction may be inexact for particle counts above 40" here it is missing the unit and I think it should be *below* 40.

Results and discussion:

Page 10300 lines 7-9: "At 10 min resolution the correlation between the means from the two instruments was 0.984. For comparison, when processing the DMPS data with the old inversion algorithm, the obtained correlation is somewhat lower: 0.967". I do not think that the two correlation coefficients are statistically different.

Conclusions: Page 10203 line 17: "suburban location" should be "urban background" to be consistent with the Method Section.

Figure 4: I would color the train points of the two DMPSs differently. In the caption what does it mean "second axis"?

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 10283, 2015.