Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-447-RC2, 2021
© Author(s) 2021. CC BY 4.0 License.

Atmospheric Measurement Techniques Discussions

Interactive comment on "Long-term aerosol mass concentrations in southern Finland: instrument validation, seasonal variation and trends" *by* Helmi-Marja Keskinen et al.

Anonymous Referee #2

Received and published: 8 April 2021

Overall comment: The manuscript shows a lot of work and many useful results for a certain community, but already by the title and the abstract it is doubtful that it is appropriate for a technical journal. The larger part of the manuscript is dealing with seasonal variation and trends of PM1, PM2.5, PM10 values in Southern Finland, often with respect to EU regulations, with various instruments and discussed in the style of a review paper. If the authors consider to keep it as a technical publication, it should be greatly revised and get much more focus on technical methods like bivariate fitting and its applicability for same or similar aerosol instruments, and other measurement sites. The extent of discussed examples of data can be reduced and shown, like in

some extent present in the text, with respect to limits and advantages of the validation, fitting, instruments uncertainties, maximum error and so on.

Having said that, I am not going into much detail in correcting sections, like seasonality and trends, which I suggest to be published as a separate publication or I would review in the next iteration. More specific comments:

Abstract, Line 26-28, "... can also be expected to be influenced..." too much "discussion style" in this sentence for an abstract section. Keep the abstract reduced to its purpose: what has been done and which results were obtained.

For my taste the introduction is too long and again mostly not refers to methodological or technical issues:

From line 36 to 91, quite long review of PM measurements.

Line 101 – 104: this could be actually the key part of the manuscript and get higher attention in the introduction.

P5, L120: All instruments should be described providing calibration/systematic and other uncertainties.

P5, L134: is 10 min time resolution the scanning cycle of the DMA?

P5, L143: the particle density is one of the most crucial parameters of this publication. I recommend to include more information about Saarikoski et al. 2005 already in the text. It should be mentioned that this reference is dealing only with one site in Finland. Preferably some geographical, boreal regions and the range of the aerosol densities should be given, at least for similar conditions.

P5, L146: the equation can be written without brackets

P5, L150: brackets can be removed

P5, L157: I suggest adding more information on the principles of the SHARP instru-

ment. Does the nephelometer count single particles? Later on, it is said it is designed to measure mass directly. I failed to find Goohs et al 2009 on the web. Please include DOIs in all references!

P6, L163: Very brief section for the most technical part of a technical publication. Please provide more information on:

- the mathematical algorithms of removing outliers. E.g. if one point is above 20 and the neighboring ones are slightly below 20, is would not be an outlier in my opinion. The variance of data points is important.

- How many data points were filtered out this way?

- A summary of time constants of the three instruments in one place, and the common averaging time and the averaging method.

- If possible, name exact fitting algorithms, Cantrell et al gives only a review of them, as can be guessed from the title.

P6, L175: ”somewhat lower“ reads not very scientific. Do you have estimates for the possible volatile mass fraction? Please state correlation values including June and July. From the largest difference such effects can be estimated. Summarize not only correlation values but largest possible discrepancies between the three measuring methods/instruments.

P6, L182: “are” uncertainties. Bad formulation, rephrase this sentence.

P6, L185: I’m missing fitting uncertainty/variances of the slopes/offsets and data points, this also applies to the rest of the text and all figures.

P6, L187: somewhat smaller value? Which values? “R2 values”. And remove “somewhat”

P6, L188, so, it seems that the value of 1.5 for density is used as a reference for this and some cited publications, to make it comparable in the case of Finnish boreal

sites. You should state it right at the introduction of the density, see L143. But does it make sense to report correlation values, if they are based only on the arbitrary fixed density? You could do the correlation based on the optimized value for density. Then the uncertainties of the Rˆ2 values are important. See P6, L185.

P6, L191: I find it contradictory “the best agreement was found between SHARP and impactor”, but in the line 185 the slope for that is 1.3, which is higher than the others, also looking at the offsets it is not the lowest value. This whole section if difficult to follow, think about how the ordering of values, “optical to weight, weight to weight” methods can be named and sorted out better and separated from the interpretation. Maybe a table to summarize all that will help.

P10, L288 remove “over” in over exceeding.

P10, L290. Exactly, measurement inaccuracies should be described in the instruments section.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-447, 2020.