Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2016-1054-RC2, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the functional form of particle number size distributions: influence of particle source and meteorological variables" *by* Katia Cugerone et al.

Anonymous Referee #4

Received and published: 2 June 2017

This manuscript describes the applicability of the Johnson SB distribution for fitting particle size distributions measured by optical particle counters operated at two sites in Italy. The paper first focuses on assessment of the merits of the expression form by examining the fraction of measurements that lie within the region on the skewness-kurtosis plane that is bounded by the Johnson SB envelope of possible solutions. The patterns of data point clusters on that plane are then linked with meteorological and environmental conditions to suggest the more general use in describing the sources and processing responsible for an observed size distribution. The manuscript is reasonably well-written but would require some editing prior to publication.

I have identified several specific concerns I have with the manuscript below. More generally though, this simply does not seem to be appropriate for ACP. The dataset described is very limited and rather uninteresting when not complemented by other aerosol and trace gas measurements. More importantly, the dataset is not really the focus of the paper, but rather the technique to describe the dataset is. Thus, in its current form this would be more appropriate for a journal such as AMT. If the authors chose to shift the emphasis more toward the size distributions I still feel that because of the limitations of the dataset this would be better suited for another journal. It could be that collaboration with researchers involved in more comprehensive measurement campaigns could be valuable for evaluating the utility of the techniques described here for understanding influences on size distributions.

It seems the authors have considerable experience with statistical methods and data analysis, but not with air quality. The relevance of this is that there is far too much text describing rather fundamental details about aerosol sources and sinks and meteorology.

The averaged and example size distributions shown in Figures 1 and 3 reveal a common characteristic of distributions measured by OPCs - erroneous peaks and troughs that are often linked with features in the scattering intensity vs. size relationship for the optical geometry of the instrument. The fact that they are retained in the distributions suggests the authors didn't invest much time in calibration of the instruments and processing of the data. But more relevant for this paper, those features will influence any fit of the distributions and the location on the S-K diagrams. There is no discussion of these features or their impact.

The authors argue that the Johnson SB distribution is more appropriate for fitting the particle size distributions than more commonly used forms such as the lognormal. But they neglect to discuss the utility of the lognormal because of the direct connection of the parameters describing it with physically meaningful elements of the aerosol distribution (i.e., N, Dp_mean, SD) and the ability to describe variation of those parameters

accompanying things such as atmospheric processing. Furthermore, the manuscript largely dismisses lognormals based on the difference between the data points and the single lognormal point on the S-K diagrams. But does the representation as a point presume that only one lognormal is used to fit the distribution? In practice, multiple lognormals are almost always used.

Minor issues:

Page 4, line 1: Grimm model what?

Page 4, line 7: What is the basis for the assertion that the composition is different between the two sites. It undoubtedly is, but this still needs some support.

Page 4, line 26: Nitrogen dioxide and nitric oxide are not aerosol compounds.

Page 6, line 24: Total particle count is meaningless to readers. I assume the authors simply need to divide this by the product of flow rate and sample time to report it in concentration. Additionally, it seems there is confusion about the upper threshold value because it is written both as 10^4 and as 100000 (=10^5).

Page 10, top: The NO2 to NOx ratio will be largely dependent on time of day, which will confound the interpretation of its influence on the patterns in the S-K diagrams.

Figure 3: The use of an unnecessarily large y-axis range obscures the information in the distributions and the quality of the fits.

Figure 4: The differences among these graphs are pretty modest.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2016-1054, 2017.