Reply to the comments by Mark Stolzenburg

Dear Mark, I would like to thanks you for this really detailed review. It will certainly help to improve this article.

General Comments
This is a very well-written and reasonably comprehensive paper on a topic of growing significance. In the past and still to a significant extent today, many researchers have concentrated on aspects of the number size distribution beyond the simple dry distribution. In particular, they have looked at the hygroscopicity and volatility as a function of size. This has been of great value for isolated studies that do not involve a whole network of stations. However, as particle mobility size spectrometers become more reliable, user-friendly and potentially somewhat less expensive, they will more often be deployed at multiple sites in long-term monitoring programs and their intercomparability will become much more important. Modelers are likely to make liberal use of this data and it is important that the quality and comparability of it is assured.

The authors here have laid a fairly solid groundwork as a first step in achieving this goal. I applaud the no doubt heroic efforts they made to get all these disparate researchers and their instruments together in one place, not once, but three times. For those organizing all of this (Wiedensohler) it must have taken an incredible amount of time and dedication to schedule and run the workshops as well as collating all the necessary data from the different groups to write this paper. For this incredible effort I am quite grateful. It has made this paper the landmark work that it is.

Thank you very much Mark

That said, I would like to see the extra effort put in to make this as good as it can be. Because of this, I have been very rigorous in my evaluation below. Do not be put off by the length of my comments. There is very little that will require significantly more work.

We will do our best to improve the article as much as we can. However, we cannot repeat the workshops.

However, I am disappointed/concerned about the lack of agreement above 200 nm and below 20 nm. Even the degree of agreement within in this range is overstated (see below). My personal experience is that it is significantly more difficult than one might think to obtain +/-1% uncertainty in flows even under controlled laboratory conditions, much less in the field.

I agree. And we are also disappointed.

The lack of agreement in the inversion routines (Fig. 2) above 200 nm is particularly disturbing. Unlike what the authors suggest, I feel that this must be due to differences in how the multiple charge correction is applied in the various routines. (A different possibility that just occurred to me is that the disagreement at the upper end may have something to do with use of splines that carry over information from the lower size bins to the upper size bins. Different size bins and splines could cause variations in the carry over effect.) Whatever the problem is, it should be a fairly straightforward problem of resolving and correcting it (if it is not a spline problem). I think the authors could probably decide on a best practice in applying the correction or at least guidelines as to what method to use in which situation. As this is all computer work, this should be resolvable without the need of gathering the instruments and the investigators all together in one place again. Furthermore, if the Fig. 5 has been stored according to the recommendations in this paper, then it should be possible to rerun the processing of the data for Fig. 5 using the corrected routines, though it looks doubtful that it would make much visible difference given the way it is plotted here (i.e. linear vertical scale).