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).

Given all this, I think this problem of disagreement in the inversion routines above 200 nm should be resolved and incorporated into this paper before publication.

We will re-run the inversion by using only one inversion routine to explore if the differences become smaller.

The lack of agreement in the measured distributions below 20 nm is a very different matter and will probably be much more difficult to resolve. The possible oversimplification of modeling losses (especially in the inlet and outlet of the DMA) as those in a tube of equivalent length may have something to do with the disagreements in this lower range as the measurement of losses and subsequent determination of the equivalent length may have been done using significantly larger particle sizes. Note that the losses in sequential tubes of the same length with mixing (i.e. reestablishment of a radially uniform aerosol concentration profile) between the tubes is not the same as a single tube of any equivalent length. Thus, this method of approximating losses by an equivalent length is even more crude and less accurate for the more complicated plumbing of the DMA aerosol inlet and outlet regions. This and other considerations make it too difficult to resolve this problem within the scope of this paper. However, I believe the topic of this disagreement deserves a somewhat more thorough discussion about the possible causes in the text of this paper.

I completely agree. Since we emphasize here mainly on the mobility size spectrometers for the size range above 10 nm, we will only mention that enhanced diffusional losses in bends are a great problem. We will also explain that we are going to investigate this in the next infrastructure project. We will discuss it in the text.

Specific Comments
page 5525, line 12: Define “long-term”.

Ok, we will do this

p. 5530, ln. 6-11: In this paragraph, the authors cite a few references for the various elements involved in calculating the electrical mobility distribution from SMPS (DMPS) data. Key original theoretical references have been omitted in favor of more recent references emphasizing practical application for the bipolar charge distribution and the DMA transfer function. However, the theory is the basis of these practical applications and those appropriate references need to be cited as well. At minimum this would include Fuchs for bipolar charging theory and Knutson & Whitby and Stolzenburg for DMA transfer function theory.

OK, we will do this

Also, the Wiedensohler fit for charge distribution is only good for a specific set of ion parameters. These may change with humidity and/or the level of pollutant gases in the sample flow.

There are basically no other bipolar charge distributions used for atmospheric measurements

The Birmili et al. method of determining the transfer function relies on two DMAs having identical transfer functions. This can only be assured when the transfer functions are ideal, producing the minimum spread in the TDMA response function. There is no assurance that two nominally identical non-ideal DMAs will underperform to the same degree (e.g. same mixing characteristics). The suggested test for this in Birmili *et al.* is not very sensitive so the equal assignment of the non-ideality to both DMAs may not be correct. Thus, though the

methods/formulae of the references cited here may be the most practical to use they are not necessarily accurate in all cases.

This is correct; we will mention this in the text, although we believe that this uncertainty is small compared to the other discrepancies we observed.

p. 5533, ln. 12: “These (temperature and absolute pressure) are preferentially monitored at the aerosol inlet of the DMA.” Why there? The aerosol or sheath air out would actually be more representative of the temperature in the DMA as the temperature of the sample in flow must equilibrate with the station temperature. Depending on the length of plumbing between the station wall (or ceiling) and the DMA aerosol inlet, the temperature may not fully equilibrate until it meets the DMA sheath flow.

You are correct, it is better to determine this preferably at the excess air

Of course, this is also true of the relative humidity of the sample flow. The measured absolute pressure may be the closest to that of the internal DMA pressure in any of the four flow lines entering or exiting the DMA, depending on which has the least pressure difference between the sensor and the classification section of the DMA. The aerosol outlet line may very well have the smallest pressure drop.

There might be only one 1 hPa difference between the sensor and the inner DMA. We believe that this seems to be not critical.

p. 5533, ln. 14ff: Why store particle number concentrations when the actual particle count per channel is needed to calculate uncertainties of measured concentrations? “Raw data” should be raw, number concentration is not raw. Store counts (# of particles) and delta time per channel as well as the CPC sample flow. In some SMPS systems, the CPC inlet flow rate is not the same as the DMA aerosol out flow rate. It may also be different from the actual CPC internal sample flow rate such as in ultrafine sheathed CPCs. Thus, four flow rates should be recorded: DMA sheath and aerosol flows and CPC inlet and sample flows. Some CPCs report these latter two flows (e.g. TSI 3025) such that they could be actively monitored during sampling. Thus, these flows should be recorded for every distribution.

To store the raw counts is indeed the real raw data, but we agreed to use the concentration due to following reasons. 1) Everybody can use this data to recalculate the number size distribution without knowing anything about the specific aerosol flows and counting times etc. 2) There are DMPS systems that have variable counting times as function of the concentration, and this cannot anymore be considered in the metadata.
Therefore, we would like to keep this format. However, we will explain this better in the text.

p. 5533, ln. 20: “The DMA dimension (…) sheath and aerosol and flow rates … should be supplied in the meta-data.” The sheath and aerosol flow rates are being monitored and recorded with each distribution; there is no need for them in the meta-data. In the underlined portion of this sentence, perhaps the second “and” is a typo or perhaps you meant to say “… and CPC flow rates …”. Please clarify and note that the CPC flow rates should be monitored and recorded with the distribution data, not in the meta-data. Some SMPS systems collect both upscan and downscan data. This parameter should also be recorded with the distribution data. The DMA to CPC detector delay time should also be recorded in the meta-data.

We will modify and clarify this paragraph

p. 5535, ln. 3: Upon close inspection of the lower plot of Fig. 2b and noting the 5% deviation of the "GRIMM ISO 15900" and "TSI ISO 15900" curves, I believe a more accurate description would be: "Apart from "Old Grimm", all inversion routines agreed within 5% over the size range 5 – 300 nm. Only toward the upper end of the size range (300 - 500 nm), some discrepancies occur probably due to uncertainties by of transferring …". Your following suggestion as to the cause of these discrepancies does not make sense. Why would there be any uncertainties in transferring the data to different formats? It ought to be a straightforward, clearly-defined process. If there were some sort of problem, why would it only affect the upper end of the size distribution? It would seem more likely that this may be due to differences in implementation of the multiple charge correction. If this is the case, this problem needs to be addressed.

We will rephrase the sentence, and we will check, if the multiple charge correction is a problem, or if these increased uncertainties are related to splining

p. 5538, Sec. 4.3.1: The DMA is a primary standard. That is, we calculate the mobility size from the basic parameters of dimensions, flows and voltage. When we measure PSL of a certified size with the DMA, we are checking that the DMA is performing properly and that the calculated size is the same as the PSL size. We are NOT calibrating the DMA size. We either accept the theoretically calculated size because it agrees closely with the PSL size, or, when it doesn't, we find the source of the problem and fix it. We do NOT create a "measured" size to voltage relationship based on the PSL measurements. That would be a "calibration". We simply conduct a performance check of the DMA sizing capability. It either works or it doesn't. (This is not strictly correct for the ultra-high sheath flow DMAs of de la Mora and co-workers. There the sheath flow is initially unknown and it is determined by fitting the calculated size to a known molecular size. Still, this is a calibration of the sheath flow, not the particle size.)

Ok, I agree with you. It's a size check, not a calibration. However, I do not agree that the DMA is a primary standard. We can use the DMA as a secondary standard, if it can reproduce the size of a primary standard as PSL spheres.

p. 5538, ln. 14: "… +/-1% …". See note for Fig. 4 below.

OK

p. 5540, ln. 7: Clearly, some of the curves in Fig. 5 deviate outside the shaded +/-10% range between 20 and 200 nm. In particular, as the size decreases, the IFT TDMPS and UHEL curves deviate more than 10% starting at about 60 nm and the PSI curve at about 40 nm. Over much of the range from 20 to 60 nm there are deviations by 20% or more. Thus, you are considerably overstating the agreement between the distributions in this range. You must find a more accurate way to describe the agreement in the curves here and in the abstract.

OK, this is true. I need to describe this better.

p. 5540, ln. 13: The sharp decline of the NILU and TNO size distributions below about 15 nm is clearly not normal. Both systems use CPCs that detect 80% of the particles at 10 nm so this is not a CPC counting efficiency issue assuming they continued to perform the same at the third workshop compared to the measured efficiencies at the second workshop. Any other losses would not drop so sharply. A possible explanation is that the DMA voltages were not properly maintained at this very low voltage. However, TSI personnel say that this is definitely not normal behavior for the standard TSI 3034 and that the DMA voltage should be steady and accurate in this range. Was the IFT scanning software used by TNO with this instrument also controlling the voltage? If so, then perhaps there was some problem with that. For both the NILU and TNO instruments this is a glaring problem in their performance in the lower size range and most likely indicates some sort of problem that can be fixed if the

time were taken to diagnose the cause. As the TNO instrument is a commercial product, it is important that you clearly state in the text that this is not normal behavior for the original instrument as shipped by TSI. Similarly, I assume the NILU instrument has been used in a number of studies and the performance you have measured in this workshop is not necessarily representative of its performance in other studies. Of course, the best thing would be to determine the cause(s) of these problems so that it can be demonstrated that this is not normal behavior. But I assume it is too late for that. In any case, you must address in some manner in the text this abnormal behavior. It should not be simply lumped in with the characterization of the other sub 20 nm deviations. In addition to losses and uncertainties in DMA voltage, low counting statistics (due to the very low charge fractions) below 20 nm may play a role in the variability of the size distributions from the other SMPS systems.

The problem of this behavior we found later. It was caused by setting the high voltage. The power supplies had a negative off-set, which lead to zero or low voltages. For larger particles, the effect becomes negligible. We need to explain this better.

p. 5541, ln. 10-12: Measuring at the peak of the distributions in Fig. 8, the shaded area represents +/-20% (not 25%) range around the reference spectrometer. Replace “25” with “20” twice in this range of lines. Also, the sentence beginning on line 10 should begin as “This time the shaded area …” since this is the first time the 20% bounds are used.

Ok, actually I wanted to have a +/-25% range .. lets see, either are extend the shaded area or I change the text.

p. 5541, ln. 24: From Fig. 9 at 40 nm I calculate that the mean of the shaded area is at about 4750 and the curve for the LB system is at about 5000, which is about 5% above the mean, not 3% as written on this line in the text.

Ok, I will check and improve this

p. 5549, ln. 21: “… (+/-1%) …”. See note for Fig. 4 below.

OK

Table 2: Can you provide any references for the inversion routines which currently do not have any?

There might be no other references, but I will check this.

Table 2: The way in which the calculations are presented makes it difficult to compare and contrast the different inversion methods. Part of the problem seems to be that consistent language is not used to describe the same thing in different inversion routines. For instance, some routines say dN/dlogZp using real/ideal area of the DMA transfer function where others say something about linear inversion. Essentially, it seems that most of the routines use the usual monodisperse approximation in evaluating the integrated response function used to calculate the CPC concentration. Thus, dN/dlogZp only depends upon the area of the transfer function. Others (NILU, UHEL/FMI, PKU?) seem to imply that they may do something else. But this is not at all clear from the descriptions as they are now and it should be. In the case of the PSI routine, the first steps listed are the obvious ones that none of the other routines even bother to mention. Then when it comes to the important part of the PSI inversion routine, the description is too brief. Since the TNO system (as noted in Table 3) uses the IFT scanning software the same as the IFT-SMPS except for a switch in charge polarity, it should be included in the description of the IFT routine with an indication of the polarity difference. This makes it obvious that they are the same routine such that the TNO routine is not shown separately in Fig. 2. All in all, I think Table 2 could benefit from a total rework, possibly including a change of format. Much of the material in this table could be

readily displayed in a table with columns corresponding to particular inversion routines and rows corresponding to particular features of the Input or Calculation. The entries in the table would then be simply which choice of each feature was used. Special cases that don't fit this pattern could be explained in notes. Thus, the options for the scan data input would be (number concentration or CPC count rate or CPC counts & count time) vs. (mobility diameter or DMA voltage). Features for the calculation could be
- whether or not the standard calculation of dN/dlogZp is used with notes for those that do not use it
- what transfer function or ideal vs. real area is used
- charge correction option
- use, or not, of interpolation/spline and rebinning in log mobility space
I think the conversion from Zp to Dp is the same for all routines and need not be mentioned.
- use, or not, of interpolation/spline and rebinning in log mobility diameter space
- which corrections are applied: CPC efficiency, diffusion losses

OK, I understand the confusion. I need to harmonize table 2 and clarify your points.

Table 2: Why did some routines correct for CPC counting efficiency while others did not, even though the routine had the option to do so? Particularly, why is the IFT routine correcting for neither CPC efficiency nor diffusion losses when the IFT-TDMPS system, which uses this routine, supposedly measures down to 3 nm where these corrections must be quite significant?

In some routines, counting efficiency curve can be included. For the IFT systems, always a post-processing in terms of diffusional losses and CPC counting efficiency is done. We will clarify this in the text.

Table 5a,b: As noted above, CPC inlet and sample flow rates need to be recorded (mandatory). Also as noted above, raw CPC counts should be recorded. If this is not done, then Standard Deviation – raw number concentration (in cm-3) should be recorded. Due to low count rates, this is often of far greater importance than Standard Deviation – median particle mobility diameter (in nm).

This is a good point, which I will discuss with people at the data center

Table 5a,b: For the last optional system parameter, Sheath air status, the recirculating flow is often driven by a blower as in the TSI SMPS and the error condition indicates the flow is not stable. I suppose "not critical" could be applicable to a sheath flow driven by a pump preceded by a critical orifice but I don't see how it applies when using a blower.

Ok, I need to think about this, how to make this clear

Table 5b: Since there are essentially two independent SMPS's (except for a common inlet and synchronized scan periods) in a dual-DMA spectrometer, the Level-0 raw data should consist of essentially two independent sets of data. The start and end times are in common but all other parameters and measurements should be independently recorded for each SMPS system. In particular, there are independent temperatures and pressure for DMA1 and DMA2 as well as independent saturator and condenser temperatures for CPC1 and CPC2. These parameters have not been properly allocated in Table 5b. The only possible exception might be the DMA absolute pressure if it is measured at the aerosol inlet to the DMA. Then, one might expect there to be negligible difference between DMA1 and DMA2 such that only one sensor may be needed. Also, absolute pressure sensors are expensive. More important even than these system parameters, there should be no elimination of size bins in the overlap region of the two SMPS's at this level and the number of size bins in each SMPS data set should be recorded separately. It would not be uncommon to decide after the fact that the data from one SMPS is compromised while data from the other can be trusted. Also,

the agreement, or lack thereof, of the two SMPS's in the overlap regime is an important indicator of the quality of the data. All in all, I think the data format would be more user-friendly at this level if the data from the two SMPS's were recorded totally separately as two distinct sets of data of the form given in Table 5a.
I am not sure if this is better to have the two systems separated, since they are inverted at one. However, I will discuss this with people from the data center. I check also the allocation of the system parameters.

Table 6: Following the lead from Level-0 in the case of a dual SMPS system, I think the Level-1 data should consist of two distinct size distributions with associated system parameters plus a third data set in which the two size distributions have been combined with some sort of smoothing in the overlap region. Again, this would allow investigators to observe the degree of agreement in the overlap region and make their own decision about how much of the data to trust. This same procedure would then be followed if SMPS size distribution data were combined with size distribution data from another type of instrument such as an optical particle counter. Also, uncertainty information about the size distribution sizes and concentrations should at least be optional at this level as noted in the text at the bottom of page 5545. Are you thinking that whatever rebinning of the size distribution data there is to be done is complete at this level? This should be made clear.

I do not agree with this. 1) The two data sets are inverted together (this is different to the TSI approach, which leads to a wrong multiple charge correction of the nano-SMPS(DMPS)). 2) There is only one size distribution, and this one is then given in level-1.
Your last point is not clear to me yet. I need to discuss this with people from the data center.

Table 7: As in Table 6, the uncertainties in size and concentration of each size bin should be listed as optional here. Note that these uncertainties are different from what is indicated by the percentiles already listed.

Again, I need to discuss this with people from the data center.

Table 8: The appropriate references should be included in the caption here. I hope you have thoroughly checked the values in this table this time.

Ok, I check this

Fig. 1: In many situations there will be a significant temperature difference between outside and inside the field station. Care must be taken to assure the sample aerosol flow has acclimated to the inside temperature prior to drying and measuring its flow and temperature. Nothing has been said about this here or in the text. The RH/T sensor in the sample aerosol flow will help determine that this acclimation has occurred; however, perhaps it should be upstream of the Nafion dryer or, at least, the flow measurement which is temperature dependent. As noted earlier, the RH/T sensor in a DMA outlet flow (sheath out here) should be used to represent conditions inside the DMA. Thus, the sheath out sensor should not be moved to the sheath in line as the aerosol in and sheath in flows may not have the same RH and T such that the RH and T inside the DMA is something in between these two.

The first priority is to have the sensor in the excess air. Here, we agree. We could have alternatively a second sensor in the sheath-in flow. I will let modify the plot.

Fig. 1: For a recirculating sheath flow, the dryer in that loop is not really needed since all the sheath air ultimately comes from the already-dried aerosol sample flow. By putting a dryer in this loop there is the potential of the entering sheath flow being drier than the entering

aerosol flow. Thus, a particle may shrink slightly while traversing the DMA, resulting in some uncertainty in its size. Of course, if the system is started up with moist air somewhere in it, it will take longer to get all the flows dry if the sheath air dryer is eliminated. Drying of the HEPA filters, which are often hygroscopic, also needs to be considered here. These things should be addressed to some extent in the text and reasons provided for preferring one dryer configuration over the other.

Ok, I will discuss this in the text to make this clearer

Fig. 3: These efficiency curves do not show a lower tail that is typical when using DMA aerosols for the calibration as they are not really monodisperse. Were these curves somehow corrected for the finite dispersion of the DMA challenge aerosols?

No, they were not correct for the DMA dispersion.

Fig. 3: The TNO SMPS (TSI 3034) uses an internal CPC that is almost identical to a TSI 3010, as indicated. However, according to the manufacturer, the dT for this CPC is 25, not 17. This is clearly in agreement with the grouping of the TNO CPC performance curve with those of the other CPCs with □T=25, rather than with the curves of the CPCs with dT=17.

Yes, you are right. We will correct this

Fig. 4: Your reasoning about the acceptable spread in the peak locations in this graph is incorrect. First of all, curves (possibly quadratic) should be fitted to the regions around the peaks of the data curves to get a more accurate estimate of the true peaks of the responses. Then, note that all spectrometers see the same PSL and therefore their peaks should register within +/-1% of their mutual mean according to your estimate of instrument uncertainty. That mean, in turn, should lie within 2.5% of the nominal PSL size according to the manufacturer of the PSL. Leaving aside the TNO instrument, the mean of the other curves appears to meet the latter condition. However, it appears that the spread in the peaks, even after determining the peaks more accurately, will significantly exceed the estimated +/-1% instrument uncertainty. Thus, you need to reevaluate the level of instrument uncertainties, particularly the sheath flow calibrations.

Mark, we cannot repeat the experiment anymore. But it seems to that the measurement of sheath flow was not correct at few systems. Since these measurements were done by the individual users, it might be that the flow measurements were not performed at the correct pressure. This directly gives a shift in the PSL peak. We need to formulate this correctly, to provide the best advice to the users in field.

Fig. 4: The widths of the peaks in this graph appear to be scattered somewhat randomly according to theory. For ideal DMAs (i.e. no mixing) the width of each peak should be somewhat greater than the width of the corresponding transfer function of the DMA that produced the peak. The full relative width at half maximum of the transfer function in mobility space is just the ratio of the aerosol flow to the sheath flow. For most of the DMAs here that ratio is either 0.1 or 0.2 while that for the TNO instrument is 0.25. Given that 200 nm is in the transition regime, the corresponding relative widths of the transfer functions in diameter space should be a little more than half of that in mobility space. The relative widths of the peaks in this graph should then be a bit more than that according to the width of the PSL distribution. In particular, the UHEL instrument has a flow ratio of 0.2 so its peak in this graph should have a relative width of a little more than 0.1. For the unsmoothed version of the curve here, it looks like the width may be fairly close to that predicted. The UHEL curve has the smallest width of all the measured peaks here. However, theoretically the FMI, UBIR and PSI curves should be even narrower because their flow ratios are half that of the UHEL instrument. Yet, they clearer are significantly wider. Other instruments with flow ratios of 0.2

also have response curves that are significantly wider than that of the UHEL instrument. This graph, then, is clearly giving us a measure of the non-ideality (i.e. mixing) of these DMAs similar to what a TDMA experiment would do. This should be discussed to some degree in the text as well as its implications for measurements of size distributions.
We will discuss this in the text. It seems to be that the given flow ratio of the UHEL DMPS is wrong.

Fig. 4: As noted earlier, this measurement of certified PSL by these DMAs is not a calibration of size for the DMAs and should not be referred to as such in the caption of this figure. Also, you refer to "modified … spectrometers". What modifications are you referring to?

OK, we clarify the text. It's confusing.

Fig. 8: As noted earlier, "The shaded area marks the +/-20% range …"

OK

Technical Corrections
page 5522, note 8: Table 9 lists this organization as "University of Lund"; make them the same.

OK

p. 5526, line 10: "…TSI Inc. (Shoreview, MN, USA) and or Grimm GmbH …"

OK

p. 5526, ln. 17: Define acronym here. "… World Meteorological Organization (WMO) issued …"

OK

p. 5526, ln. 19: Add date to citation. "… (WMO-GAW Report 153, 2003), but …"

OK

p. 5526, ln. 23: This reference is missing. "… (Dahmann et al., 2001)."

OK

p. 5527, ln. 5: If possible, please give an English user-friendly reference for this. "… a custom-made TDMPS (Twin-DMPS) …"

OK

p. 5529, ln. 2: This needs a reference. "… the equilibrium ion pair concentration, which is believed to be approximately 107cm-3."

Let's see..

p. 5529, ln. 8: "… annular slit closed to the outer electrode …"

OK

p. 5529, ln. 18: "… to an annular slit …"

OK

p. 5530, ln. 5: The year for the Schladitz et al. reference conflicts with that given in References.
OK, we will check and correct this

p. 5530, ln. 7: Correct the spelling of “Knutson”.

OK

p. 5530, ln. 20: Some SMPS inversion routines also calculate particle diameter from DMA voltage. Reword this sentence to accommodate calculation in either direction.

OK

p. 5531, ln. 1: “Ambient air samples can increase considerably their RH …”

OK

p. 5531, ln. 7: “..minimizes diameter changes due to hygroscopic growth to typically …”

OK

p. 5532, ln. 8: I have difficulty properly interpreting this sentence. “… the aerosol flow should not deviate systematically more than 5% from the set-point on daily average.” Perhaps you could reword it.

We will try to do so

p. 5534, ln. 3: The correct year of the Wiedensohler reference is (1988).

Yes

p. 5534, ln. 26: “… around the results of the IFT inversion routine.”

OK

p. 5535, ln, 1: “To see small differences as a function of particle size, …”

OK

p. 5535, ln. 6: “Note: the “Old Grimm” inversion routine is not any used anymore …”

OK

p. 5536, ln. 4: “… in the frame of the third EUSAAR DMPS/SMPS intercomparison workshop …”

OK

p. 5536, ln. 5: “… are shown here as an example.”

OK

p. 5536, ln. 8: “… we achieve after the DMA …”

OK

p. 5536, ln. 9: This sentence would be clearer if written as “Due to the relatively small degree of polydispersity of the aerosol from the furnace …”

OK, we will do this

p. 5537, ln. 3: “… may also differ by a few nanometers …”

OK

p. 5537, ln. 15: The acronym ACCENT has been previously defined and adding “programmes” at the end makes it read better. “… in the frame of the EUSAAR, ACCENT, and WMO-GAW programmes.” You should really decide if you are going to use American or British English spellings of words. e.g. program/programme appears both ways.

We will harmonize this to American English

p. 5538, ln. 2: “… mobility size spectrometers …”

OK

p. 5539, ln. 1: “… and the TNO instrument works well again.”

OK

p. 5542, ln. 1: “… represents the accuracy …”

OK

p. 5542, ln. 2: “… the current the state of knowledge …”

OK

p. 5543, ln. 17: Give the model number(s) of the 85Kr bipolar chargers. I assume they are from TSI. They have a variety of configurations for their 85Kr chargers having very different losses I would imagine.

OK, but the IFT chargers have custom-made housings

p. 5549, ln. 15: What is the “absolute detection efficiency” of a CPC?

We will correct this

p. 5549, ln. 16: “… checked on a monthly basis.”

OK

p. 5550, ln. 2: “… is not available, also the total number …”

OK

p. 5550, ln. 10: “… for several size distributions.”

OK

p. 5552, ln. 2,3: “… maximum variation around setpoint: +/-_% …”

OK

p. 5553, ln. 14: “… median values of the averaged the particle number size distributions …”

OK

p. 5555, ln. 19: “… counts can either been accumulated …”

OK

p. 5556, ln. 1: “… determined by the geometric mean …”

OK

p. 5556, ln. 6: “… data format indicated as particle number …”

OK

p. 5556, ln. 9: “… for the institutes or and universities …”

OK

p. 5562, Table 1, third entry line: The reference cited here “(Jiang et al., 2011)” has a different year than in the References.

We will check and correct this

p. 5562, Table 1, fourth and fifth entry lines: Given that the nominal lengths of these straight Permapure Nafion dryers are 24” and 12”, respectively, I am surprised that their equivalent lengths are more than four times longer. Is this correct? In that the equivalent lengths scale directly with the actual length, this would indicate that the losses occur within the tubes rather than at the entrances and exits. I would not think the slight curvature of the Nafion tube within its sheath would be enough to enhance the losses that much. Were these measurements possibly done with charged particles because there are probably some significant static electric fields associated with the Nafion?

We need to cross check this again

p. 5562, Table 1, last entry line: Give the type (e.g. 85Kr) and model number of this charger.

OK

p. 5563, Table 2, IFT Input: According to Table 3, the IFT-SMPS (scanning) and the IFT-TDMPS (stepping) both use the IFT software. Therefore, shouldn’t there be an IFT input corresponding to “- Selection: Scanning or Stepping”?

No, we have the same “raw data” format

p. 5563, Table 2, IFT Input: “- Consider of simple dynamic shape factor …”

OK

p. 5563, Table 2, IFT Calculation: The order of adjectives makes a difference here. “Equidistant” operates on the “logarithmic” mobility scale so it should come first. Also, doesn’t the interpolation come first followed by splitting into bins? Thus, “- Interpolate and split into discrete equidistant logarithmic mobility … bins”. This also applies to similar entries in the ULUND, ISAC, JRC and TNO sections of Table 2.

We will check and improve this

p. 5563, Tble 2, NILU Calculation: “- Total DMA DMPS? counting efficiency …” DMAs don’t count.

OK sure, will be corrected

p. 5563, Table 2, NILU Calculation: “- Charge correction (Wiedensohler, 1988)”

OK

p. 5563, Table 2, NILU Calculation: Write as “- Problem formulated … by discretizing (finer) in particle size independently of number of measured size bins” or put a closing parenthesis at the end of this entry.

OK

p. 5564, Table 2, UHEL/FMI Input: It is customary to write (dependent variable) vs. (independent variable). Thus, “- DMA Number concentration versus DMA voltage”

OK

p. 5564, Table 2, UHEL/FMI Calculation: The inversion method is a choice; it belongs in the Input section. A note explaining a bit about how these different inversion methods work or differ would be helpful.

OK

p. 5564, Table 2, PSI: In the Input section it is indicated that the CPC efficiency correction was not applied for this study. However, the Calculation section indicates that the raw measurements were corrected for the CPC detection efficiency. Which is correct?

It was not applied in this study, we clarify this

p. 5564, Table 2, **Note: The note is not an input; it should not be preceded by a dash “-”.

OK

p. 5564, Table 2, *Note: A note corresponding to “*” needs to be included. Apparently, it marks the selection used in this paper.

OK, I will check this

p. 5565, Table 2, ULUND Input: Are the charge and spline type actually choices (i.e. negative or positive, and cubic or linear, respectively), or are they fixed at the indicated values and therefore not really inputs? The same question applies to the last four entries of the JRC Inputs on the next page.

OK, I need to check this

p. 5565, Table 2, ULUND Calculation: It is indicated that dN/dlogZp is calculated using the ideal width of the DMA transfer function. However, the usual method of calculating dN/dlogZp assumes that the width is negligible compared to the variation of the other factors within the integrated response function for the CPC concentration and therefore it does not enter into the final expression for dN/dlogZp. Thus, use of the ideal width should be irrelevant to the calculations unless they are not using the standard equation for dN/dlogZp. If the latter is the case, then more information is needed here.

I need to discuss this with the people from LUND

p. 5565, Table 2, ISAC Input: Insert comma between “count number” and “counting time” for the first entry.

OK

p. 5565, Table 2, ISAC Input: The last entry in this section is actually a note that applies to the entire column. It should be placed at the bottom of the column as “* Selected for …” with no dash “-” in front of it.

OK

p. 5565, Table 2, LAMP Calculation: “Integrate between set mobility’s transfer functions”. This does not make any sense. Instead of “Integrate”, do you mean “Interpolate”? What would be the purpose of integrating between transfer functions? Nor does it make sense to integrate across an ideal transfer function as that is already a known quantity.

You are completely correct. I need to find a person in Clermont who can correct this, or I will do this

p. 5566, Table 2, UMN Calculation: Was the transfer function actually “experimentally determined” or was it actually just determined to be within experimental error of the ideal width for larger particles? Again, only the area or penetration is used in the standard linear inversion method; other details of the shape of the transfer function (e.g. width) are not used.
p. 5566, Table 2, PKU Calculation: “Iterative” is misspelled.

You may probably know this better than I do. I have to ask Pete McMurry

Table 3: Table 3 also has a problem with consistency of notation. The three critical dimensions – R1, R2, L – should be given for all DMAs, including the commercial ones. There are many “Hauke-type” DMAs listed here with identical, or nearly so, dimensions. Are all these actually manufactured by Hauke or some other single entity? If so, is there a model number associated with these that could be referenced for all these different systems that seem to be using the same DMA? If so, then perhaps the dimensions need only be given once.

The Hauke DMA used in the European network are always custcopies manufactured by different commercial or institute machine shops. I guess the dimensions are not the same, at least for the outer electrode, IFT change this diameter. We will make this clear.

Table 3: Though most are listed in Fig. 3, the dTs for all CPCs should be listed in Table 3 since many use a non-standard dT.

OK

Table 3; If the 85Kr bipolar chargers are from TSI, give the model numbers. If any of the other chargers are commercially manufactured, give those model numbers as well.

OK, we will write what it is

p. 5568, Table 3, IFT-SMPS: "… heat exchanger for closed loops, …" (only one loop)

OK

p. 5568, Table 3, NILU-DMPS: Is this DMA really 285 mm long? Just 5 mm different from that of the IFT, UHEL, FMI, JRC and ISAC setups? Otherwise, this would seem to be the same DMA.

I cannot believe this, I will check

p. 5569, Table 3, UHEL-DMPS: There is no entry for Software. Table 2 indicates that it is the same as the FMI-TDMPS system. This should be listed here.

OK

p. 5569, Table 3, PSI-SMPS: This DMA appears to a TSI 3081 with a slightly shorter length. The radial dimensions given are actually the inner and outer radii, not diameters as stated. To be consistent with the other entries of DMA dimensions the true diameters (x2) should be given. Also, "… heat exchanger for closed loops, …" (only one loop)

OK, I will improve this

p. 5569, Table 3, FMI-TDMPS: Judging by the length, it appears that DMA1 may actually be a Hauke 3/150. If that is the case, please give that model number. Also, " FUG positive power supply (x2) (up to …)"

I will check this, but I believe that it is a custom-built DMA

p. 5570, Table 3, JRC-DMPS: "… heat exchanger for closed loops, …" (only one loop)

OK

p. 5570, Table 3, LAMP-DMPS: This DMA appears to be a TSI 3081 as the dimensions are exactly the same except for the precision given. If that is the case, state this model number. Also, use the same precision as the manufacturer in stating the radii and give them as diameters to be consistent with the other entries in the table.

I believe that it is a custom-built copy, but I have to check this

p. 5570, Table 3, NUIG-DMPS: I believe the manufacturer of the high voltage power supplies is actually "Bertan" as I could find no listing of the given company name on-line.

OK

p. 5571, Table 3, ISAC-DMPS: The radial dimensions of the DMA should be given as diameters for consistency. Also, "Other hardware: aerosol flows: Nafion dryer; …" (Presumably only the sample aerosol flow (singular) has a Nafion dryer.)

I check this

p. 5571, Table 3, TNO-SMPS and UBIR-SMPS: Though these are commercially-built systems the information given about them should be as complete as possible, including the

measured size range, the DMA dimensions, the fact that the CPC in the 3034 is essentially a TSI 3010 as it is listed in Fig. 3 and details of Other Hardware that is part of the base TSI system.

OK

p. 5576, Table 7: The column for the 15.87 percentile should read “Particle size Bin 01 dN/dlogDp 15.87 percentile (cm-3)*”

OK, will be changed

p. 5578, Table 9: This table should also provide information about the location of each of these entities. Most of them, though not all, have corresponding entries in the footnotes providing affiliations of the authors on pages 5521-5522. Perhaps this could be used somehow to reduce the duplication of information. The footnote number could perhaps be included in this table with additional footnotes on this page for those entities not associated with any author. As mentioned earlier, the University of Lund or Lund University should be listed the same way in both locations. Also, the acronym TNO should be defined.

I will check and improve this

p. 5579, Fig. 1: The symbols used for the critical orifice and pump are not obvious in terms of which is which. Therefore, the labeling of these should clearly indicate that the orifice comes before the pump. Also, the absolute pressure measurement between these two as noted in the text should be shown in this figure.

Ok, I need to check this..

p. 5580, Fig. 2: The acronym “BOL” in the key of Fig. 2a needs to be defined. The symbols used on some of the non-IFT curves in the upper graphs should also be used on the corresponding curves in the lower graphs, making it easier to distinguish one curve from another. The last sentence of the caption should read “The lower graphs show the ratio of the …”. These graphs are rather small and some of the grid lines are fairly faint such that I had to use a magnifying glass to see what I wanted to see.

OK

p. 5582, Fig. 4: More use of symbols on the curves would make it easier to distinguish between those that have very similar colors, such as IFT REF 1 and LAMP. Also, in Fig. 3 it is IFT SMPS whereas here it IFT REF 1. Are these actually different instruments? If not, use the same notation.

Thanks, this has to be corrected

p. 5583, Fig. 5: “Intercomparison of the measured particle number size distributions …”

OK