Atmos. Meas. Tech. Discuss., 6, C389–C391, 2013
www.atmos-meas-tech-discuss.net/6/C389/2013/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Performance of diethylene glycol based particle counters in the sub 3 nm size range" *by* D. Wimmer et al.

Anonymous Referee #1

Received and published: 19 March 2013

The manuscript by D. Wimmer and coauthors on the "Performance of diethylene glycol based particle counters in the sub 3nm size range" reports procedures and results characterizing the counting efficiencies of different CPCs in the cluster regime under laboratory conditions. Two laminar flow type TSI 3776 CPCs are compared to two turbulent mixing type CPCs. Effects of seed particle composition, charging state and sample contamination are shown to significantly impact the counting efficiencies. Furthermore, insufficient particle activation and losses in the laminar flow CPCs were found to affect the shape of the counting efficiency curves considerably. In general the manuscript fits well into the scopes of Atmospheric Measurement Techniques and should be considered for publication. Before final acceptance I would still suggest adding/clarifying several issues discussed below and improve figure quality.

In the introduction on lines 23-25, page 2153, three different methods for the generation of supersaturated vapors are mentioned. As this is an instrument and technology related journal I recommend adding representative references for each method. Similarly, in section "General considerations" the authors correctly point out the problem of signal interference from homogeneously nucleated droplets at high saturation ratios. In this context it may be worth noting that apart from the proper selection of working fluid also the time-resolved monitoring of particle number concentration allows extension of the lower detection limit to sizes well below 2 nm (e.g., Winkler P., et al., Atmos. Res. 90, 125-131 (2008)).

Section 3.3 discusses methods and procedures for the generation of aerosol particles. On lines 1 and 2, page 2159, the authors say that particle free air from the laboratory was used as carrier gas. Was the removal of particles the only conditioning of the lab air? How about organic trace gases that are likely present in the air they were using? In fact, on lines 14 and 15 (page 2159) the authors even note that contaminant levels were considerably higher when using filtered lab air compared to pure nitrogen carrier gas. In view of the finding that organic contaminations play a crucial role in the counting efficiency, this should be discussed in more detail. An assessment of the sources for organic contaminations in such presumably clean laboratory surroundings would clearly enhance the impact of this paper. Furthermore, some emphasis is put on the operation of the high-resolution DMA in open and closed loop. I think it would be worth including a brief statement about the benefits/drawbacks of each method and why it has been done this way.

In section 4.1 the authors discuss possible reasons for the observed differences in detection efficiency. Apparently, positively charged particles showed lower detection efficiencies than others which is in agreement with available literature data. However, in Figure 5 positively charged WOx particles show significantly (on the basis of illustrated error bars) higher detection efficiencies in the range of the cut-off diameter and below (red triangles). Some Figure interpretation would be desirable. This figure (and

others as well) raise the question whether the shown error bars are reasonably determined. Why are the error bars smallest when one would expect highest measurement uncertainty? It even seems that the scatter of the mean values at full detection efficiency is larger than the uncertainty shown for the negative WOx particles below 2 nm. This certainly needs some explanation. Also the discussion on possible reasons for the charge sign effect on page 2162 is not clear to me. What do the authors mean when saying that working fluids are positively charged? How would the working fluid become charged? Are there arguments supporting this assumption? If so, what are those arguments? For instance, what is the influence of cosmic radiation on working fluid charging? I assume this could be quantitatively estimated.

Regarding section 4.3 I am not sure what it adds to the message of this paper. It is vaguely formulated and basically shows that in the absence of nanoparticles the CPCs agree nicely. Maybe a change in the section header would help directing the reader to what can be expected from this section. I would suggest something like “Instrument comparison in ambient conditions”.

Section 5 “Conclusions” largely sounds like a motivating section. I would also recommend putting the obtained results into context with other similar studies on DEG CPCs (e.g. Kuang C., et al., Aerosol Sci. Technol. 46, 309-315 (2012); Iida K., et al., Aerosol Sci. Technol. 43, 81-96 (2009); Jiang J., et al., Aerosol Sci. Technol. 45, 510-521 (2011)). How do the Frankfurt DEG CPCs perform in comparison to the Brookhaven or Minnesota DEG CPCs?

Figure 1: In my printout I did not find any labels, however, the electronic file did show them. Figure 3: Please enlarge figures. Shouldn’t the NaCl generator be added in Figure 3a? See text in section 3.3, page 2158. Figure 7: Please enlarge figures.

Typos: Page 2158, line 10: . . .changes. . .

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 2151, 2013.