This manuscript describes the intercomparison of four cascade impactors sampling in two European locations and from indoor and outdoor. The locations/seasons offer a wide range of particulate matter for the intercomparison study. The four impactors studied have the ability to collect ultrafine particles. The manuscript also offer some characterization if the aerosol size distributions and chemical composition. The BLPI with coated impaction plates is used as an internal reference. The study finds that all the impactors have good agreement for PM_1 and larger. As the cut-point size of the impaction stages is reduced the impactors measure larger mass concentrations than the internal reference. The errors are greatest for the PCIS, then the nano-MOUDI and then the nano-BLPI. The authors propose several hypotheses for the disagreement at lower size cut-points. The manuscript is well written and easy to follow. However, there are many discrepancies throughout the manuscript. It has been well established in the literature that impactors are prone to errors from both particle bounce and volatilization of particulate components, i.e. semi-volatiles and/or nitrates. The authors provide some inconclusive proof that these artifacts occur during their intercomparison study, but offer no new scientific insights on the artifacts. While I agree the study is important and would be an interesting addition to the scientific community, for the reasons provided below I do not support publication in ACP.

Comments:

1) The authors mention that the greater RH environment of the summer in Prague would reduce the amount of particle bounce (Page 17 line 9-12) and potentially explain the results shown in Figure 1. However, at each stage in the cascade impactors, the pressure is reduced. This reduction in total pressure will also reduce the RH according to $RH_i = RH_{amb}*P_i/P_{amb}$. Therefore, at each stage of impaction, the relevant RH to compare is the RH directly above the impaction stage according to the upper stage pressure. This number will be greatly reduced for the lower stages of impaction, regardless of the initial ambient RH. Therefore, particle bounce of ultrafine particles will be an issue for any cascade impactor.

2) The authors state they use a thin layer of grease (page 7 line 27 – 31) on the internal standard (BLPI). This should be stated in the abstract and conclusion. However on page 11 line 19-20, while discussing the sampling at Barcelona using BLPI (the internal standard), the authors state that the foils were not coated with grease. Which is it? I would not trust an impactor that was not coated with grease to be used as an internal standard. This would explain the agreement in $PM_{0.25}$ – PM_2 observed in Figure 2 and Figure 3 for the Barcelona samples. But it would also negate the fact that you had an internal standard, as the Barcelona sampling with BLPI would be prone to particle bounce artifacts.

3) I do not understand Figure 3. Why do the authors lump the mass concentrations into categories that do not match with the impaction cut-points? There is no cut-point at 250 nm for the nano-MOUDI and there is no cut-point at 1.0 μm for the BLPI. So how did the authors arrive at mass concentrations for $PM_{0.25}$ and PM_1 for these two cascade impactors? This should be stated directly in the manuscript. The authors state on page 15 lines 7 -9, that differences in

Figure 3 can be attributed to difference in cut-points. This figure is then misleading to the readers and of no use as an intercomparison. As it is not a difference in collection efficiency or potential collection artifacts, but how the instruments are designed to operate.

4) Figure 4. In order to assess the ability of the impactors to accurately assess the collection of ultra-fine and fine particles, one needs to know what the actual mass concentrations of these particles are. It would certainly benefit the study to have some form of external validation that did not rely on a cascade impactor, such as filter sampling, or SMPS (or DMPS) to provide some external validation points for Figure 4. In addition, the authors suggest that this sampling for Barcelona was for 4 weeks (Page 17 lines 25) while Table 1 does not seem to agree.

5) The use of IC to determine the mass of ion concentrations on each stage seems like a great idea. However, the authors provide very weak conclusions from these studies. Page 21 lines 3-5, the authors provide all ranges of explanations, so what is the use of the IC study? Again, it would be useful to know what the actual mass concentrations of the ions are in order to determine any artifacts from impaction sampling.

6) If the temperature of the nano-MOUDI does indeed increase and effect volatilization of nitrate as the authors suggest, why is this not observed in the indoor samples? There is not a clear decrease in the nitrate signal in the indoor samples, as the authors suggest on page 20 line 18. In addition, there are no error bars on these data points, making any scientific interpretation difficult. There is also a decrease in the sulfate signal in the indoor samples, which is not expected to volatilize, suggesting an overall decrease in collection efficiency perhaps and not an effect of increased temperature.