Referee report for “Design of Wide Particle Size Range Aerodynamic Inlet System with New Pre-focus Structure” by J. Huang et al, 2024

The study by Huang et al. proposes a new aerosol inlet design that is a miniaturized version of the Du et al., 2023 inlet, yet gives similar performance (though that is never clearly stated). The scientific contribution is significant, and the subject is appropriate for AMT. The organization is good (with the exception of Fig 3 and 6 being far separated), but the writing is often difficult to follow and contains extensive grammatical and repetition problems. Most often the meaning is conveyed, although some sentences are indecipherable.

Overall, this manuscript was submitted 1 or 2 editing versions too early. It contains two major discrepancies and extensive minor problems. Many labels are missing, figure captions are inadequate, critical details are excluded. A thorough effort in editing and re-writing is necessary to achieve the quality of its predecessor paper, Du et al. 2003.

That said, the contribution of the study (miniaturized aerosol inlet design with impressive performance) is significant enough that the authors be given the opportunity to resubmit the paper when the Major and Minor problems detailed below are addressed.

Major comments

1) There are multiple lines of evidence that put the experimental TE results of Fig 7, and the uncanny agreement with simulation, into question. A) Most of the values are exactly 100%, which is highly suspect, and include no error bars showing variability. B) As written, the reference system for submicron sizes (a CPC without a DMA) is incapable of accurately counting PSL generated by nebulization, assuming that many (typically *very* many) non-PSL (surfactant) particles are also generated. C) The simulated particle trajectories in Fig 6 show that particle beam widths at the detection laser(s) are much larger than a typical SPAMS laser beam width (Du et al., 2024). Therefore a large fraction of the simulated particles would not be detected by SPAMS, and experimental TEs cannot be at or near 100%. Furthermore, it is very likely that these simulated beam widths provide a lower limit, and the actual particle beam widths are probably much larger.

The authors need to re-analyze the Fig 7 results and/or convincingly justify the presented values by including a detailed data table, statistics, formulae, and a discussion of points A,B,C above.

2) There is much confusion surrounding the presentation of results for “new” and “old” systems. The new design for the current study is presented in Fig 1, the “PFW-Alens”. The old system is that of Du et al., 2023. However, the simulations in Fig 5 present what is apparently a mix of the Du 2023 design (large buffer region, skimmer style VI) and the current design (5 focusing stages) for all five panels, despite that these are described in the text as the PFW-Alens. “New” and “old” designs are compared in Fig 5a & 5b, but these are again just the Du design with and without a pre-focuser. Confusingly, the final Du 2023 design contains a pre-focuser, so it’s unclear what Fig 5b (“old”) refers to. Consequently, the simulated transmission efficiency (TE) results of Fig 3 are also in question. Which design was simulated for the “Present Study” lines?

The authors need to make the results in Fig 3, 4, and 5 consistent. Simulation results for the present study/new design should all be based on the design in Fig 1.

Minor comments

General. The text needs another editing pass with a focus on sentence structure and verb tense. Examples of grammatical problems and repetition are lines 29-31, 89-92, 196-199, 232-233, 241-245, 303-305, although the problems occur throughout. Also, it is strange that simulated transmission efficiency is discussed in two separate places: section 3.1/Fig 3 and section 3.3/Fig 6. Consider combining those two Figures, e.g., as a single figure with two panels, along with their associated text. Also use consistent terminology throughout, e.g., choose either “inlet” or “injection system”.

Line 18, 271, 309. Clearly this newly designed aerodynamic lens is not a “PM2.5” lens – it transmits much larger particles! Omit this terminology or explain.

Line 28. Define APS.

Line 52-60. The brief summary of previous aerosol inlets gives helpful context. However, the size ranges given seem arbitrarily chosen. For instance, while the Zelenyuk inlet transmits the given size range of 125-600nm at near 100% transmission efficiency (TE), their inlet transmits a much wider size range out to ~1.5um or larger at lower but still useful efficiency for SPMS studies. Modify the text to clarify the given range, for instance, if they denote the peak of the TE for each inlet, or the range where TE approaches 100%.

Line 104. It would be much clearer to the reader if the authors stated clearly that the purpose of the study is to design a miniaturized version of the Du et al., 2023 inlet that gives similar performance.

Line 125 Fig 1 shows a disc at the downstream end of the separation cone that provides an inner diameter reduction. What is the effect of that diameter reduction (presumably to 1.6mm – please add a label)? Does it provide a pressure drop so that separation cone acts as a better virtual impactor? Or is it a skimmer? Later in the manuscript (section 3.1) and also in the abstract, the authors refer to a virtual impactor, but it is not shown in Fig 1. Add a description of how the cone acts as a virtual impactor, and perhaps how it compares in design to Du 2023, which is quite different, and update your terminology in Section 2.1 accordingly.

Line 129. It seems important to mention that the accelerating/downstream nozzle has a tapered cone just upstream. Presumably, this reduces impaction, and it will also affect the trajectories of particles entering the particle sizing region. State any known effects of this converging nozzle design.

Section 2.1. It appears that Du et al., 2023 used a differentially pumped region between the aerodynamic lenses and the downstream nozzle, whereas the current study does not. State any ramifications of this design difference (is it just simpler?).

Line 139. I don’t quite follow the pressure descriptions. Please clarify. Are the two pump-out lines on the “separation cone” fixed at 600 Pa? Is 300 Pa a fixed pressure, or is that the approximate pressure aerodynamic lens pressure that results from the calculation? Other parameters of interest to include would be the volume flow rate through the upstream critical orifice, the lens pressure, and the pressure drop down the lens stack.

Line 143. Presumably the critical orifice region contains non-laminar and supersonic flow. Does the viscous model treat this region appropriately? Also state whether any symmetry was enforced for the simulations.

Lines 143-147. Several sentences are poorly written. Please re-write to clarify. What is DPM? What are cloud images? What is the UDF that you refer to?

Section 2.3. Mention why the additional airflow in the upper right of Fig 2 is needed and how it is used your TE calculation.

Line 154. Give a model number or describe the ICPMS atomizer.

Line 167. Define APS 3321. Describe how aerodynamic diameter is measured.

Line 169-170. The PMT detections for which laser, the first or second? State what % of particles detected by the first PMT are also detected by the second PMT.

Line 172-173. Presumably, you are also accounting for the flow rates into the instruments (and any dilution flow)? State all the flow rates.

Line 175. Nebulized PSL solutions typically produce polydisperse particles containing surfactant, which can greatly outnumber the PSL particles. How are these non-PSL particles accounted for in the TE calculation, both for the supermicron and sub-micron setups, particularly since the CPC cannot distinguish between PSL and surfactant particles? Can you show a size distribution of the generated particles?

Line 185. It is not clear what the authors refer to as a "virtual impactor", particularly an inlet design that does not contain one. For the "without VI" case, what part was removed in Fig 1? Note that in general, *any* type of transverse pumpout design like the separation cone presented here will act as a VI to some extent for large particles. Please clarify VI and without VI throughout this section.

Section 3.1. Since virtual impactor is mentioned frequently, state the approximate particle size above which the VI becomes effective, ie, where nearly all particles are transmitted downstream and very few are being pumped out. From Fig 3, it appears that ~90% of 100 nm particles are transmitted downstream and are *not* pumped out, correct?

Fig 3. State that these are all (presumably?) simulations, not experimental results. Add references for the without VI and without pre-focus lines. How is TE defined here, eg, counting the particles exiting the nozzle, or those crossing the laser beams…? It is difficult to see the data points that are on top of one another.

Line 237-239. The argument about how particle acceleration in the pre-focus stages affects downstream divergence angle is not convincing, and I believe it is incorrect. In both the Du and present designs, the particles achieve the same maximum velocities when passing through the critical hole, regardless of whether the pre-focus stages accelerate them or not. Consider instead the difference in radial distributions between the Du pre-focus design and the current one (Fig 5a). The strong downstream divergence in the Du design is due to the wider radial distribution and higher incident angle as particles approaches the critical hole. Reconsider these arguments and update text accordingly, in this section and the abstract.

Line 239. Clarify “degree of change”

Fig 4. Axial velocity varies considerably across the diameter. Are these average velocities, centerline velocities, … ?
Also, add labels for the different parts of the inlet system at the appropriate axial ranges.

Fig 5. Can you comment on particle divergence in the vacuum region (downstream of the last nozzle), and how it changes with size? Presumably the converging nozzle design is advantageous here.
Also, add a velocity color scale, with consistent scaling for all panels.

Line 260. Why 110mm?

Line 265. State how the particle beam width compares with the beam waist (at focus) for both detection lasers.

Fig 6 & 7. Are the black lines the same as Fig 3 black line? Why are the simulated TE values different between Fig 6 and 7? The data are plotted versus “aerodynamic diameter”, which is probably determined under the *vacuum* conditions of the SPAMS instrument, rather than *continuum* aerodynamic diameter as is typically defined. Clarify.

Line 291. Remove all-caps. List vendor. Also, mention something about the experimental sampling setup – is it identical to Fig 1 green lines.

Fig 8. Are the lines calculated as the average over seconds/minutes/hours? State in the caption. The APS line appears smoothed – is it? Clarify the y-axis – is it normalized dN/dlogD or normalized counts? Clarify “aerodynamic diameter”, which I suspect is *continuum* for the APS and *vacuum* for SPAMS. If a conversion was done, what density and shape factor were applied? In general, it is reasonable to plot normalized size distributions as the authors have done here. However, it would be even more informative to plot absolute concentration, say as dN/dlogD, which then provides another demonstration of TE but now for non-spherical particles. Optional, at the authors’ discretion.

Line 307. Add Du 2023 reference, just to be clear what is meant by “traditional”.

References

Du, X.; Zhuo, Z.; Li, X.; Li, X.; Li, M.; Yang, J.; Zhou, Z.; Gao, W.; Huang, Z.; Li, L. Design and Simulation of Aerosol Inlet System for Particulate Matter with a Wide Size Range. Atmosphere 2023, 14, 664. https://doi.org/10.3390/atmos14040664

Du, X., Xie, Q., Huang, Q., Li, X., Yang, J., Hou, Z., Wang, J., Li, X., Zhou, Z., Huang, Z., Gao, W., and Li, L.: Development and characterization of a high-performance single-particle aerosol mass spectrometer (HP-SPAMS), Atmos. Meas. Tech., 17, 1037–1050, https://doi.org/10.5194/amt-17-1037-2024, 2024.