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Interactive comment on "MEMS-based condensation particle counter for real-time monitoring of airborne ultrafine particles at a point of interest" *by* Seong-Jae Yoo et al.

Anonymous Referee #2

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This manuscript reports on a small and seemingly simple condensation particle counter. The work appears sound, but the reporting fails the basic standard of scientific discourse - that the work be described in sufficient detail that a knowledgable researcher in the field could reproduce the work based upon the information provided in the manuscript. Key information is omitted from the paper that is essential to understand the work performed. It is not publishable in its present form, but could likely be made publishable with more thorough documentation. I also note that the statistics of the measurements need to be more thoroughly documented.

The CPC that is reported is a low flow (0.15 LPM) instrument that interfaces a clever,

3-D printed saturator and condenser with what appears to be a simple, off-the-shelf optical particle counter. No information is provided about the detector, which is a key component of the instrument, so I can only guess what it is. My suspicion is that this is one of the class of optical dust sensors that have been used as PM2.5 surrogate sensors. Such a sensor might yield quantitative data in this application, if the performance could be demonstrated to be stable over time. I note, however, that the flow paths through the instruments of this kind that I have examined are ill defined, and that the view volume and portion of the sample flow that is passed through the view volume are poorly constrained.

Therefore, a critical requirement for further consideration of this manuscript is explicit identification of the optical detector (both manufacturer and model number), or detailed description if the authors have developed it as part of their instrument, and clear and complete documentation of how the flow from the condensational activation core of the CPC is interfaced with the optical detector. If this can be done, the authors may be reporting on an instrument that solves a very real need for an affordable CPC. I note that the sketch in Fig. 1 shows a more elaborate OPC design than in the simple optical detectors about which I have read or with which I have worked. Moreover, it shows a forward scattering instrument, but does not show a beam dump that is required for this geometry, suggesting that the cartoon does not represent the actual instrument.

I further note that no mention is made of the pump employed, or the approach used for quantitative flow control. The instrument should be documented fully. A critical requirement for publication in any reputable scientific journal is full disclosure. I note that reports on the performance of commercialized instruments may fit within the scientific literature without such detail because they are are used by many groups and such documentation informs those users of issues in their use and the interpretation of the data they generate. Incomplete reports on a one-of-a-kind prototype instruments have little value and should not be published.

I further note that the authors have not identified the working fluid employed in their

CPC. I suspect that they have copied existing CPCs and used butanol, but have no way of knowing for sure from the information provided. This should be a very easy oversight to correct.

All of the data presented should be accompanied by explicit statement of the operating conditions in sufficient detail that the experiments could be reproduced. The temperature difference between the saturator and condenser is incomplete without specifying one of those two temperatures. Again, this is a small revision that should be easy for the authors to address.

The authors note that the CPC grows particles into 3.16 μm droplets, but provide no hint as to how they have determined this size. The instrumentation that they report is incapable of directly measuring particles of this size. I suspect that they have made the inference from the scattered light intensity detected by their OPC, but OPC measurements in this size range are highly uncertain owing to Mie resonances. A precision of 3 significant figures is highly unlikely.

The precision reported for accuracy of counts also seems excessive. A claim is made that the concentration accuracy is 4.1%, and later that it agrees with a reference CPC within 91.5%, suggesting double the uncertainty. Error bars are needed on the data plots, and uncertainties on the quoted efficiencies. The uncertainties in the reported efficiencies need to take into account the Poisson counting statistics. The counting time is another piece of information that needs to be documented. It is quite reasonable to operate with a longer counting time than the TSI instrument with which they compare their data - as a trade-off in producing a much smaller instrument.

The orientation of the instrument also needs to be specified, as well as its sensitivity to orientation. I suspect that it must be operated with the saturator oriented vertically, with the reservoir at the bottom. What is the sensitivity to motion or tilting, as this is likely with a small, highly portable instrument? Also, if the optics are protected from flooding with working fluid, that would be of interest in ascertaining the instrument's suitability

for different applications.

Specific points:

The introduction makes a range of broad statements about the importance of ultrafine particles in different situations, but these are based mostly upon two-decade-old reports. I do not dispute the importance of measuring such particles, but the specific arguments made have been superseded by more recent studies. The first paragraph of the introduction needs to be written with appropriate reference to the current understanding rather than that of two decades ago. The reference to clean room particle measurements is problematic because the flow rates through a miniature CPC will lead to poor counting statistics at the concentrations at which clean rooms operate. The introduction needs to be rewritten, but the amount of explanation of the need for simpler and cost-effective instrumentation can be reduced significantly, however.

On p. 2, l. 13 reference is made to electrical techniques. I can only guess to what technology the authors are referring. If they wish to refer to a specific technology, they must define it in sufficient detail that the reader can understand it without having to trace through the literature citations that they give.

l. 35. The authors claim a lower detection limit on concentration of 8 particles/cm^3. At the quoted flow rate, this concentration corresponds to about 20 counts/s. For a 1 s integrating time, this corresponds to a statistical (based on Poisson statistics) uncertainty of order 1/4. To claim uncertainty of 4%, a long count integration time is required. The authors need to be explicit about the parameters of the measurement, i.e., what is the integration/counting time?

On p. 3, l. 18, the authors note that a customized circuit was implemented for pulse-width-modulation. It would be highly desirable to document the nature of the control algorithm employed: is it P (proportional), PI (proportional-integral), or PID (proportional-integral-differential). Was the control algorithm implemented in software or in analog, hardware circuitry?

p. 4: The experimental setup section describes the system in reasonable detail. It is unclear, how the quantitation was performed. The data presented in Fig 6 shows an asymptotic approach to 100% detection efficiency. Were no large particles lost or otherwise not counted. Again, the nature of the optical detector and the way that the flow interfaces to it becomes an issue; How does the design ensure that all activated and grown particles pass through the view volume so that they can be counted. The analysis of the experimental results needs to clearly document all assumptions and approximations made in the data analysis. How were losses measured?

P. 5, l. 28: The authors report on the detectable concentration range as measured with the temperature difference between the condenser and saturator set at 30^{o}C. Specify the conditions of operation of the CPC fully. The temperature difference between the saturator and the condenser are only part of the information that is needed. What was was the saturator temperature? Of course, all temperature information is meaningless unless the authors specify their working fluid.

Fig. 5 is very difficult to interpret as there is little contrast difference between the region where liquid is present and where the wick is dry. A bit of guidance as to how to interpret the picture is appropriate.

Fig. 6. The data shown reveal three groupings of data points, one below 10 nm, one 10-15 nm, and one for larger particles, but only up to 40 nm. What was changed for these data sets that show slight offsets and differences in noise levels? Data are shown down to 3 nm which is not possible with the DMA that the authors report using. Did they use a different DMA for the sub-10 nm particles? The calibration should be extended to larger particles since the tortuous path through the saturator could lead to losses, and the small dimensions might make those losses orientation dependent.

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