

Review: “Aerosol dry deposition fluxes on snow during the ALPACA campaign in Fairbanks, Alaska”

Donateo et al.

This paper reports on calculated modal and size-resolved deposition fluxes from 2-months of surface measurements at a site in the urban Arctic (Fairbanks, Alaska). They find an increase in deposition flux with increasing diameter, consistent with previous work in a similar Arctic region and a model parameterization. Using temporal trends, meteorological regime classifications, and profiles of atmospheric thermodynamics and number concentrations, the authors attribute the variability in deposition fluxes to regional sources (anthropogenic pollution) with enhancements due to local meteorology and synoptic circulation.

This work provides very interesting insights on aerosol deposition in an understudied region that is strongly impacted by intraseasonal synoptic variability and distinct aerosol sources and transport. The discussions and presentation of results are prepared very well, and I am overall confident in the fidelity of this work. I can recommend this paper be considered for publication after the following minor comments, clarifications, and questions are addressed.

Comments:

- Lines 195-196: Can the authors please specify how the ultrafine particle mode was determined? Was this a subtraction of the OPC integrated number concentration from the total concentration measured by the CPC?
- Lines 283-287: Particle number concentrations in the tens of thousands seem exceptionally high for ambient measurements, even during Arctic hazy periods. It makes sense that the UFP mode would be the highest particle number concentration, but the value seems dramatically high. Can the authors please provide context for these number concentrations? First, as a comparison to previous measurements of total and modal number concentrations at Fairbanks/Urban Arctic and second with respect to the meteorology. Is there a sense of how high/different these aerosol number concentrations are from “background” conditions in Fairbanks or nearby/similar regions? Were the aerosol concentrations (total and modal) different between meteorological regimes? Additionally, typically in CPCs typically the laser becomes saturated above $10,000 \text{ cm}^{-3}$ and coincidence errors become quite large. Can the authors speak to and justify why they feel such high concentrations measured by the CPC can be trusted?
- Table 1: is the mean diameter (d_p) in this table taken as the average of the range of diameters in each mode/size range? I believe this should be specified somewhere in the text and table legend as not to confuse with a geometric mean diameter from a modal fit to a size distribution.

- Did the authors test for independence in the 3 modes considered? More specifically, did they find covariability between any of the modes and does that have any influence on the interpretation of sources and deposition?
- I believe it is appropriate to provide standard deviations when means are reported.
- Section 3.2: Figures 3 and A1 should be cited in lines 283-287 (preferably with Table 1) before their first citation later in this section so the reader can see the trends and ranges discussed.
- Lines 288-301: Can the authors please quantify the particle differences in this passage. Phrases like “enhanced pollution”, “concentration significantly smaller”, “close to levels” and reference to the figure panels are used, but it helps the reader glean differences when these values are quantified. Did the authors also consider performing statistical tests on the differences between number concentrations in different meteorological regimes?
- Lines 300-301: There is higher N_{Q-CRS} in periods before Feb 7-10 (gleaning from Fig. 3 and Fig. A1), but the authors state that a maximum is observed during Feb 7-10. Do the authors mean elevated number concentrations during that period? Please clarify. Additionally, the authors should clarify what is meant by “... is characteristic of this class of particles and may witness a contribution from regional transport (e.g. dust or sea salt particles).” I believe what the authors are saying here is that dust and sea salt are primarily in the coarse mode and an elevation in number concentration of these particles may suggest an increase in their transport. This should be clearly stated and with citations supporting these claims (1 - dust/sea salt in the coarse mode, 2 – elevation in number concentration from transport).
- Section 3.3: The authors should place the calculated values in context with previous measurements. How do these flux values compare to other Arctic or continental regions? A diel cycle of modal flux is an interesting result that needs to be compared to similar and dissimilar calculations.
- Lines 336-337: Please state the quantified difference between FACC during ACC compared to the other periods that is referenced here.
- Figure 3 and 4: Please denote that (if I assume correctly) dots in the box plots represent the mean and line in the box is the median.
- Lines 358-361: The authors suggest wind-driven resuspension as a potential mechanism contributing to the variability in FQ-CRS. Did the authors observe more variable FQ-CRS with variable or stronger wind speeds?
- Lines 360-361: The authors spend little time discussing the differences in the flux diurnal cycle between synoptic regimes. I think the larger fluxes throughout the day

during the anticyclonic regime is consistent with the overall finding of higher number concentrations and higher fluxes during the anticyclonic regime and should be stated.

- Line 384-385: Citation to previous literature should be provided here for this claim.
- Line 397-400: Are the deposition velocities cited from previous measurements in these lines from Arctic/polar regions? Please clarify.
- Line 401-402: Are the authors able to correlate the parameterized deposition velocities using Slinn (1982) with the calculated median values shown in Fig. 5b from this study? How well do they agree? Further, are the author able to speculate on why the Slinn parameterization has such (visually) great agreement with your calculated values from these measurements?
- Figure 8 caption: it should be clarified that V_n is the deposition velocity normalized by the friction velocity.
- Line 435: I might suggest referring to the 0.54-0.89 μm size range as the “large” accumulation mode here.
- Line 438: I understand the suggestion that physical characteristics (like different particle densities) may affect the deposition behavior in this mode, but can the authors clarify why hygroscopicity would affect the deposition behavior? A citation might be helpful here.
- Lines 438-441: Citations are needed here to allude to these suggested effects.
- Fig 9: The authors should provide units for the y-axis in panel d).
- Fig 10 caption: It should be specified that panels (b,d) are zoomed in versions of panel (a,c).