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Interactive Comment

Interactive comment on "On the potential contribution of open lead particle emissions to the central Arctic aerosol concentration" *by* A. Held et al.

Anonymous Referee #2

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This paper describes an analysis of aerosol covariance fluxes and mean concentration with an upwind footprint over different types of surfaces to obtain information on deposition velocity and/or infer a surface particle source strength. In idealized conditions the eddy covariance method gives an unbiased estimate of the turbulent flux of some variable and has seen extensive application to understand air-surface interactions. However, the use with particles has been confounded by several factors including the contribution from mean fall velocity, the strong size dependence of the relevant time scales, and the difficulty of unraveling cases with surface vs non-surface sources. This paper makes a relatively focused – if confusing – attempt at this. The authors wisely point out [P24962] that "Still, the problem remains that eddy covariance flux measurements yield a net flux which is a superposition of particle emission and deposition fluxes. In order to derive the emission flux, an independent estimate of the deposition flux is required.” They present some interesting new data that complement an earlier study by Nilsson and Rannick and go a few steps beyond in the interpretation. On balance the data are valuable and their efforts are meritorious. I recommend publication but I did find the paper a bit difficult to follow suggest fairly extensive improvements in clarity.

Good Points. *The introduction is nicely written has really sets the stage. *The measurements description is adequate but reasonably brief. *The use of different surface types vs wind sectors is very good. The tables and figs 2-4 help a lot. *Conclusion section is tight and a good summary.

Not so good points. *Based on the type of sensor, I think these observations pertain principally to rather small particles – on the order of 0.02-0.05 micrometer dia. I think the size relevance should be made clearly in the abstract and the body of the paper. Don’t just give us the sensor specs – give some interpretation. *The presentation of the relationship between covariance flux vs source and deposition is confusing. I suggest a brief section starting with the basic conservation equation and then simplifying the cases. The net flux is F=<w’n’>-Vg<n>=Snz –Vt<n> Where Snz is the effective surface source strength realized at measurement height z. The turbulent flux is the net flux when Vg is negligible.

Steady state implies F is constant with height whilst equilibrium implies F=0. The mixed layer equation [line 8, P24976] comes from integrating the conservation equation. In steady state and when there is no surface source and the particles are very small, then <w’n’>=-Vt<n>.

Very small particles behave a lot like more conventional scalars (temperature,. . .) and the equilibrium case rarely occurs. Perhaps refer to paper Xia and Taylor, BLM 105, 471-482, 2007.

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*The discussion of different formulae for deposition velocity is about the right mix of references and equations. Personally, I think the paper would benefit from a few lines describing how they are related (i.e„ turbulent, gravitational, vs inertial terms). *I found the discussions associated with figs. 5-7 rather hard to follow.

Here are a few other comments to consider.

P24966, L26. Not sure what 'theory' you are referring to. Do you mean the empirical result that sigma_w/u* is on the order of 1.25 under neutral conditions in the surface layer? P24967. You have neglected stability in Eq 1. Usually a decent approximation with an instrument at 2 m height. Did you check?

P24967. Eq5 doesn't look quite right to me. Is this an approximation?

P24967. Eq6 appears to have a sign error in the first nu exponent.

P24972, first paragraph. Perhaps worth noting that <w'n'>=0 is not the dividing point between surface production and deposition. It is ambiguous without knowing the other terms of the budget.

P24976. I am not sure about the statement "It should be noted that the net particle flux will be zero as soon as a uniform distribution with height is reached."

P24976. The use of mixed layer heights comparable to the measurement height would violate the conventional assumption z«h that is invoked to claim the flux measurement represents the surface flux. I am not sure this section gains a lot by considering mixed-layer heights much lower than observed. Nothing wrong with just saying the mixed layer height was 25 m and the flux accounts for 10% of the observed concentration change.

Fig. 1 The ogive is presented in conventional form with the integral of the cospectrum initialized as 0 at high frequency and integrated to low frequency. However, in the case of significant loss of high frequency signal from various lowpass filter effects, it is better to start at low frequency and integrate to high frequency. You can also play around with the normalization to clearly show the lowpass effects.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24961, 2010.

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