Review of

On the Potential Contribution of Open Lead Particle Emissions to the Central Arctic Aerosol Concentration
(#acp-2010-695)

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General Comments

This is an interesting paper that reports some of the first direct particle flux measurements over compact sea ice in the Arctic. Because of how the instruments were sited, the upwind surface for the measurements ranged from an open lead; through a mix of ice and open water; to smooth, compact, snow-covered sea ice, depending on wind direction. The differences in roughness among these several surfaces and the differences in particle concentrations and fluxes over these surfaces is the main theme of the manuscript.

The manuscript is fairly authoritatively written, although I do have a few questions about data processing and analysis that are relevant to the authors' interpretation.

The manuscript also needs a bit of editorial work. For one thing, the authors tend to write in very long paragraphs; dividing these long passages into more comprehensible chunks would help the readers.

Specific Comments

1. On page 24964, in the paragraph that begins with "Previously, turbulent," the authors may want to cite Scott and Levin (1972), who were the first to observe particle fluxes over leads.

2. In the first paragraph on page 24965, the authors mention that the particles measured were larger than 11 nm in diameter. Here, I'd also like to know the largest particles that the authors feel the CPC can reliably measure. This size range is important for subsequent discussions.

3. In line 2 of page 24966, the text mentions that "No additional corrections were applied." That is, it sounds as if the authors did not apply a Webb correction. Businger (1986) suggests that it is important to apply the Webb correction to eddy-covariance measurements of particle fluxes. Because the particle fluxes were small but the sensible heat and water vapor fluxes over the lead, especially, may be quite large, the authors need to apply this correction or explain why they have not.

4. On page 24966 and in Figure 1, the ogive analysis of the particle fluxes does not seem to converge for an averaging time of 30 minutes. In both panels in Figure 1, the particle ogives approach one from below at a fairly steep angle. This behavior suggests to me that the 30-minute averaging was not long enough to yield statistically reliable particle fluxes. Am I missing something?

5. Still on page 24966, in lines 26 and 27, I do not think Thomas and Foken (2002) gave a "theory" for σ_w/u_*; they just do data fitting with a new independent variable. Moreover, that independent variable, zf/u_*, is not well chosen because u_* also appears in the dependent variable (i.e., σ_w/u_*). Thus, plots of σ_w/u_* versus zf/u_* naturally increase (as Thomas and Foken report) because of the built-in fictitious correlation between the nondimensional variables. See Andreas and Hicks (2002) for a further discussion of these types of flawed analyses.

6. Could you provide a reference for Equation (5).

7. The first term on the right side of Equation (6) is not dimensionally correct.

8. The discussion on page 24970, lines 4–11, is not very precise. The authors have sampled at least four different surface types (with distinct roughness regimes), but the discussion here is fairly general and groups previous measurements of roughness length over sea ice into a single category—water AND sea ice. A more meaningful discussion would be to separate the previous measurements into categories that depend on the predominant surface type and compare these with the authors' surface types. For example, Persson et al. (2002) and, especially, Andreas et al. (2010) sampled snow-covered sea ice presumably typical of Sectors C and D. Andreas et al. (2010b) sampled surfaces that were mixes of ice and water, like Sector B. Smith et al. (1983) and Andreas and Murphy (1986) reported the drag coefficient (and thus the roughness length) over leads and polynyas, like Sectors A and F.

9. Continuing on this comment, line 6 reads that "Tjernström (2005) estimated a mean value of . . . ." It's not clear what this number is the "mean value" of.

10. Similar imprecise reporting occurs on pages 24971–24972, lines 19 to 3. The authors review here a host of previous estimates of the deposition velocity but do not mention the size range of the particles in each study. Because deposition velocity is a strong function of particle size [see Equations (5), (6), (8), and (9)], this is a crucial omission. These comparisons are also why I asked earlier for the authors to state the upper limit of their size measurements.

11. On page 24972, lines 16–17, the text mentions the possibility that sectors with only compact ice can also emit particles. It would be useful for the authors to speculate on how this is possible. Their wind speeds seem to be too low for these particles to be blowing snow. What else could they be? Or are these positive particle fluxes erroneous or a consequence of ignoring the Webb correction?

12. Compare the observations of Scott and Levin (1972) with the discussion in the middle of page 24973. They also observed the emission of particles from leads without obvious bursting bubbles.

13. The introduction of the mixing layer height on page 24976, line 5, is a bit confusing. The manuscript explains in the next paragraph that this is not the traditional height of the atmospheric mixed layer (order hundreds of meters). Either make that distinction right where you introduce this mixing layer height or choose a new name for this layer that does not already have a common turbulence definition.

14. Figure 2 is a very good figure.

Technical Corrections

1. The authors write in very long paragraph that would read better if broken into smaller pieces. Here are some examples.
Page 24963, line 14: New paragraph with "Optically"
Line 20: New paragraph with "Model projections"
Page 24967, line 20: New paragraph with "Normalizing"
Page 24968, line 6: New paragraph with "We restrict"
line 12: New paragraph with "Applying"
Page 24970, line 11: New paragraph with "Figure 2b"
Page 24971, line 19: New paragraph with "The magnitude"
Page 24975, line 20: New paragraph with "In this simplified"
Page 24976, line 1: New paragraph with "It should"
line 17: New paragraph with "The emission"
line 26: New paragraph with "The same general"

2. On page 24962, lines 20–26 include one long sentence that should be broken into smaller pieces or rewritten to be more concise. For example, in line 23, "during summer from June to August" is redundant. Either "during summer" or "from June to August" is sufficient—not both.

3. Line 14 on page 24964 has two problems. "and quantifying" should be "to quantify" to be parallel with "to evaluate." "its", which follows this construction, does not have an antecedent. "its" must refer to a singular noun; but I think the authors are referring here to "particles."

4. Line 19 on page 24967 includes the meaningless [-].

5. Line 10 on page 24974 seems incomplete. It doesn't read quite right.

6. On page 24976, line 14, begin a new sentence with "in scenario 1."

7. On page 24977, line 21, Orsini et al. is not in the reference list.

8. In Figure 1, remove the meaningless [-] from the labels on the vertical axes. Also, in the caption, explain what the vertical dashed blue lines are.

9. In Figure 3, remove [-] from the label on the vertical axis.

10. The caption for Figure 4 mentions panels a and b, but the two panels have no corresponding labels. Remove [-] from the label on the right vertical axis.

References

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Smith, S. D., R. J. Anderson, G. den Hartog, D. R. Topham, and R. G. Perkin, 1983: An investigation of a polynya in the Canadian Archipelago: 2. Structure of turbulence and sensible heat flux. *J. Geophys. Res.*, **88** (C2), 2900–2910.

—Edgar L Andreas, 7 December 2010