Reply to review by Edgar Andreas

Dear Dr. Andreas,

We greatly appreciate your thoughtful comments that helped improve the manuscript. We trust that all of your comments (in particular about data processing and analysis) have been addressed accordingly in a revised manuscript. Thank you very much for your effort.
In the following, we give a point-by-point reply to your comments:

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.

We followed the reviewer's suggestion and added a reference to Scott and Levin (1972).

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.

The nominal upper size limit of the CPC is 3 µm, and we added this information in the revised manuscript. We also did a rough calculation of the particle losses in the sample inlet which suggests an upper cut around 5 µm in the sampling lines. Thus, we are confident that our CPC counted particles in the size range from 11 nm to 3 µm. However, taking into account typical particle size distributions measured during ASCOS onboard the ice-breaker, and earlier observations of a distinct Aitken mode as a general feature of aerosol size distributions in the central Arctic Ocean (Covert et al., 1996; Leck and Bigg, 2005a), we expect the number concentration (and thus the number fluxes) to be dominated by sub-50 nm particles for most of the time. This has been indicated in the original manuscript on p.24969, l.4, and is now clarified in the revised manuscript (also on request of reviewer 2).

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.

We fully agree that the Webb correction can be important, especially if the turbulent fluctuations are small compared to the mean scalar concentration. Indeed, we checked the magnitude of the Webb correction for a subset of our data when concurrent water vapor (and carbon dioxide) measurements were available. We found that for the aerosol number flux, the Webb correction was less than 2.5 % of the observed fluxes in more than 50 % of the cases, and less than 21 % in more than 90 % of the cases. In comparison, the median Webb correction was around 5 % for water vapor fluxes, and around 80 % for carbon dioxide fluxes. While it seems absolutely necessary to apply the Webb correction for carbon dioxide fluxes, we chose to report the non-corrected aerosol number fluxes because (1) the Webb correction is relatively small compared to the Horst correction and other uncertainties, and because (2)

the data set is further reduced when using only time periods when high-quality sensible heat and water vapor fluxes are available.
We added a brief section on the typical magnitude of the Webb correction to the revised manuscript.

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?

We agree that the ogive analysis does not only show high frequency loss but also some low frequency contribution to the aerosol flux, and in the case of the aerosol ogives, high variability in all frequencies. We attribute some of this variability to changing footprints when small changes in wind speed and direction result in quite different fetches. Some individual aerosol ogives deviate considerably from the expected shape, in particular when data quality is evaluated as poor and the measurements are discarded. Median aerosol ogives as presented in Fig. 1 tend to smooth out some of the variability. However, if the averaging period is longer than a few hours (as in the original Fig. 1), due to non-stationary conditions and the spatial heterogeneity (open lead, ice ridge, ice floe) even the median ogives will not converge at low frequencies. Also, if we extend the averaging period of an individual ogive beyond 30 min, a low frequency contribution remains without detrending. This indicates that long-term trends sometimes contribute to low frequencies, but they can be removed by linear detrending. We added an extended discussion of the general variability of individual aerosol ogives, high frequency losses, and low frequency contributions in the revised manuscript. Also, we now present median ogives of fairly stationary three-hour intervals in a revised Fig. 1 to show both the problems discussed above (Fig. 1a) and rather well-behaved median ogives (Fig. 1b).

5. Still on page 24966, in lines 26 and 27, I do not think Thomas and Foken (2002) gave a "theory" for sw/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., sw/u*). Thus, plots of sw/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.*

We agree that Thomas and Foken (2002) did not give a theory, but a parameterization, and changed the text accordingly. We also acknowledge the reviewer's point about fictitious correlations as further discussed in Andreas and Hicks (2002) in a similar type of example.

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

The original Equation (5) was based on Slinn (1982), Eq. 19. with a separation of the "canopy resistance" into the quasi-laminar sublayer resistance, R_b, and the surface resistance, R_c. However, on request of reviewer 2 we expanded this section to clarify the presentation of the turbulent flux components. The gravitational settling velocity is now neglected in the new Eq. 7 which is a valid simplification for submicron particles as explained in the revised manuscript.

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

We corrected original Equation (6), adding a negative sign to the exponent of nu. Thank you!

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.

As suggested, we expanded the discussion in section 3.1 and compared our observations in individual sectors with previous studies over similar surfaces.

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.

We changed this line to clarify that the mean value of z_0 is discussed.

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.

We agree that deposition velocity is a strong function of particle size, and therefore, the particle size ranges of the previous studies are now included for comparison in the revised manuscript. In addition, we corrected our reference to Grönlund et al. (2002) who report the deposition velocities in cm s^{-1}, not mm s^{-1}.

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?

We do not think that the particles found during emission-dominated periods from the ice are blowing snow. Some of these positive particle fluxes are indeed within the range of uncertainties, and could be discarded as erroneous. However, because the Webb correction is relatively small compared to the uncertainties due to counting statistics and fluctuation dampening, ignoring the Webb correction alone cannot explain these positive values. In addition, a few of the positive particle fluxes from sectors C and D must be considered valid, even though we can only speculate about potential emission sources. While the wind speed is probably too low for resuspension of snow, resuspension of particles previously deposited and accumulated on the snow surface may be a possible explanation. We added a brief discussion of the positive particle fluxes over the ice floe in the revised manuscript.

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.

We added a comparison with the findings of Scott and Levin (1972) in section 3.2.

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.

We acknowledge that the term "mixing layer height" may be confusing and therefore changed it to "effective mixing height". We now introduce it as the height up to which effective mixing occurs. Also note that we revised Fig. 7 to include scenarios corresponding with the best estimates of the effective mixing height in both examples.

14. Figure 2 is a very good figure.

Thanks!

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”
We followed the reviewer's suggestions in the revised manuscript.

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.
We broke the sentence into three smaller pieces and re-arranged it in the revised manuscript.

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.”
We revised the manuscript accordingly.

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

5. Line 10 on page 24974 seems incomplete. It doesn't read quite right.
We modified the sentence to read: "Replacing this formulation with a parameterization suggested by EMEP (2003) for gas-phase species yields deposition patterns similar to the parameterization by Zhang et al. (2001), but the absolute values are much smaller."

6. On page 24976, line 14, begin a new sentence with "in scenario 1."
We followed the reviewer's suggestion.

7. On page 24977, line 21, Orsini et al. is not in the reference list.
We replaced the reference and modified the line as follows: "Simultaneous and independent gradient measurements of particle concentrations which will be presented elsewhere corroborate our finding…"

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.
Done!

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

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.
We added labels a) and b) and removed [-] from the label on the right vertical axis.

Additional references:

Covert, D.S., Wiedensohler, A., Aalto, P., Heintzenberg, J., McMurry, P.H., and Leck, C.: Aerosol number size distributions from 3 to 500 nm diameter in the arctic marine boundary layer during summer and autumn, Tellus B 48, 197-212, 1996.