Response to review by Referee 2:

The review is in general positive. We greatly appreciate the reviewer's thoughtful comments that helped improve the manuscript. We trust that all suggestions have been addressed accordingly in a revised manuscript. In the following, we give a point-by-point reply to the comments of referee 2:

This manuscript reports some unique, preliminary measurements of the heat, momentum, and aerosol fluxes over an Arctic lead and snow-covered sea ice in the early autumn. I particularly like the simplicity and utility of the system for profiling near surface temperature and aerosol gradients. Although the profile and eddy-covariance results do not agree very well, this is not surprising: Both the aerosol and temperature signals are small at this time of year. The study is nevertheless a useful proof of concept and, thus, appropriate for AMT.

I am a bit concerned, though, with some of the science issues. A flux-gradient relation has not been established for aerosols, but the authors assume this relation without justification or adequate caveats. Their analysis of the profiles is also not standard.
The language, presentation, and quality of tables and figures are generally good.
Let me elaborate.

Scientific Issues:
1. The authors should probably mention some key previous work over Arctic leads.
Scott and Levin (1970) were evidently the first to observe particles emanating from open leads. Andreas et al. (1979, 1981) used a profiling system—not unlike the one in this study—to measure temperature, wind speed, and condensate profiles over Arctic leads.
Their measurements were in winter, however; hence, it would be interesting to contrast the magnitudes of the heat fluxes from leads in winter and those reported here (namely, two orders of magnitude less here).

We extended our discussion of previous studies of aerosol flux profile-relationships at the end of section 1 and included references to Scott and Levin (1972), Andreas et al. (1979), and Andreas et al. (1981). We also included a reference to Andreas et al. (1979) at the end of section 5.1 and compare their winter heat flux values to our summertime observations.

2. The authors assume that the aerosol concentration follows a semi-logarithmic flux gradient relation [i.e, (7)], as do wind speed, temperature, and humidity in the atmospheric surface layer. This is perhaps a useful assumption—but one with little theoretical or experimental support. In fact, the theoretical form for the aerosol concentration above a surface source was established over 40 years ago and is still widely used and has not been refuted (e.g., Fairall et al. 2009):

$$c(r,z) = c(r,h)\left(\frac{z}{h}\right)^{-V_g/ku_*} . \qquad (1)$$

Here, c(r,z) is the concentration of aerosol particles of radius r at height z, c(r,h) is the reference concentration at arbitrary height h, g V is the settling velocity of particles of radius r, k is the von Kármán constant, and u is the friction velocity. Notice, (1) is not a flux-gradient relation; it says nothing about how the concentration profile is related to the vertical flux of the aerosol.*

Because (1) is our current best understanding of how aerosol particles are distributed above a surface source, the authors need to do a much better job of explaining why they instead use (7). The fact that the aerosol concentration is semilogarithmic with height is one argument in favor of (7)—but a very weak argument.
Semi-logarithmic profiles are very robust features of the atmospheric surface layer and occur even when the other assumptions of Monin-Obukhov similarity are violated—that is, even when the flux is not constant with height or the surface is not horizontally homogeneous.

We appreciate the reviewer's considerations, however, the theoretical form of the aerosol concentration above a surface source as presented in the reviewer's equation (1) seems to be more appropriate for super-micron particles and droplets than for the aerosol number concentrations measured with a condensation particle counter in this study. 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 (e.g. Covert et al., 1996; Leck and Bigg, 2005), we expect the number concentration to be dominated by sub-50 nm particles for most of the time. In this size range, the reviewer's equation (1) cannot explain the observed concentration differences. As an example, for a typical friction velocity of 0.1 m s^{-1}, a particle concentration of 100 cm^{-3}, and a particle diameter of 100 nm, the concentration difference between height z_1 = 2 m and height z_2 = 0.01 m is less than 0.02 cm^{-3}, while the observed concentration differences were on the order of 1 - 5 cm^{-3}.
Moreover, the application of flux-profile relationships presented in the manuscript is based on similarity assumptions for wind, temperature, and particle number. This assumption may not (always) be valid but it is the basis of the presented analysis, and the fact that the aerosol concentration is semilogarithmic with height is consistent with our assumption.
We agree that the presence of a logarithmic profile alone is not a sufficient condition for fluxes constant with height or a horizontally homogeneous surface, but it is a necessary condition for the presented analysis.

3. Equation (6) is true only for potential temperature. From the discussion, it is not clear whether the authors use the actual air temperature or the potential temperature in their analysis of the temperature profiles.

We agree with the reviewer that Eq. 6 is true only for potential temperature. However, in our study performed in the lowest 2 m of the boundary layer at sea level, we used the differences of actual air temperature at two heights as a good approximation of the differences of potential temperature at these two heights. For the conditions encountered during our measurement period, we estimate the absolute error of the temperature differences by approximating potential temperature with actual temperature to be less than 0.05 K. This is considerably less than the estimated uncertainty of our temperature measurement.

4. Analyses of flux-gradient relations in the forms (6) and (7) are non-standard and may produce some misleading results. With potential temperature as an example, the relation to analyze usually takes the form

$$T(z) = -\frac{F_h}{u.k}\ln(z) + T_0 \ . \qquad (2)$$

*Now, plotting T(z) against ln(z) yields the slope, Fh/u*k, and the constant T0. With seven profiling heights, there would be seven points to fit with a least-square relation.*
The authors, instead, choose to plot T2 – T1 against ln(z2/z1), where subscripts 1 and 2 denote every combination of measurement heights—21 combinations in the present case. I think the issue with the authors' method is that it produces more uncertainty than (2).
*Furthermore, to fit (6) and (7), the authors forced the fitting line to go through ln(z2/z1) = 0. See Figure 7. Equation (2), on the other hand, allows two fitting parameters, Fh/u*k and T0.*
To see the difference in uncertainties in my (2) and the authors' (6), rewrite these, respectively, as

$$-\frac{F_h}{u.k} \equiv F = \frac{T(z) - T_0}{\ln(z)} \ , \qquad (3)$$

$$-\frac{F_h}{u.k} \equiv F = \frac{T_2 - T_1}{\ln(z_2/z_1)} \ . \qquad (4)$$

From (3), the error or uncertainty in the desired flux, F, is

$$dF = \frac{\partial F}{\partial T}dT + \frac{\partial F}{\partial z}dz \qquad (5)$$

because T0 is a constant. From (4), on the other hand, the error or uncertainty in the determination of F is

$$dF = \frac{\partial F}{\partial T_2}dT_2 + \frac{\partial F}{\partial T_1}dT_1 + \frac{\partial F}{\partial z_2}dz_2 + \frac{\partial F}{\partial z_1}dz_1 \ . \qquad (6)$$

In these, the differentials can be thought of as errors or uncertainties in the measured quantities.
Clearly, using the authors' (6) leads to an uncertainty in the desired flux that depends on the accumulated errors in two temperatures and two heights [i.e., (6) above]. The more standard approach, given in (2), does not suffer from these accumulated errors [i.e., (5) above].

We calculated the slopes both using our equations (6) and (7) and using the reviewer's equation (2) and obtained identical results. This is true only if we force the fitting line to go through zero, which means the following: For ln(z2/z1) = 0, i.e. z2 = z1 (a measurement at a certain height z1 = z2), T2 - T1 = 0, i.e. T2 = T1 (the measurement result at that height is identical). In fact, if we were to use all possible combinations of data pairs, i.e. each height difference is considered twice with opposite signs (e.g. T3 - T1 and T1 - T3), the regression line would always go through zero for reasons of symmetry. However, we chose to consider the difference of the same two data points only once (neglecting the difference of the same two data points with opposite sign) and force the fitting line through zero. The main benefit of this procedure becomes obvious in Fig. 7a, where two diverging trends indicate that the data point at the lowest height level may be inconsistent with the logarithmic model. As mentioned in the manuscript, with this representation we gain insight into the variability of the data with respect to the logarithmic behavior. With respect to the estimated flux value, there is no difference to the standard analysis according to the reviewer's equation (2). Therefore, we are convinced that equations (6) and (7) do not produce misleading results. In the revised manuscript, we add a paragraph at the end of section 4.3 to clarify that our results are in agreement with the more standard approach.

Language Issues:
5. In technical writing, the word data is usually treated as a plural. The authors, however, use it as a singular. See page 3018, line 5, and page 3032, line 23. In both cases, the authors write "data was," while I suggest "data were" is preferable.

We revised the manuscript accordingly. (abstract, l.5, and in addition on page 12, l.5, l.8, page 13, l.20, l.21, l.25, l.27, page 23, l.4, l.9)

6. The sentence that begins on page 3019, line 12, is a bit contorted. The final words, "as much as a factor of 200," seem out of place. I'd try something like "Bezdek and Carlucci (1974) showed that seawater droplets can concentrate, by as much as a factor of 200, bacteria that exist in the surface layer."

We revised the manuscript accordingly.

Thank you very much for your time and effort!