Comments on

Near-Surface Profiles of Aerosol Number Concentration and Temperature over the Arctic Ocean
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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).

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, V_g 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 semi-logarithmic 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.

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.

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, F_h/u_*k, and the constant T_0. With seven profiling heights, there would be seven points to fit with a least-square relation.

The authors, instead, choose to plot $T_2 - T_1$ against $\ln(z_2/z_1)$, 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 $\ln(z_2/z_1) = 0$. See Figure 7. Equation (2), on the other hand, allows two fitting parameters, F_h/u_*k and T_0.

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

$$dF = \frac{\partial F}{\partial T} dT + \frac{\partial F}{\partial z} dz \tag{5}$$

because T_0 is a constant. From (4), on the other hand, the error or uncertainty in the determination of F is

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

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].

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.

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."

References:

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