

***Interactive comment on* “The effect of misleading surface temperature estimations on the sensible heat fluxes at a high Arctic site – the Arctic turbulence experiment 2006 on Svalbard (ARCTEX-2006)” by J. Lüers and J. Bareiss**

Anonymous Referee #2

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Reviewers comments on "The effect of misleading surface temperature estimations on the sensible heat fluxes at a high Arctic site – the Arctic turbulence experiment 2006 on Svalbard (ARCTEX-2006)" by J. Luers and J. Bareiss

This paper addresses the problematic issue of parameterising turbulent fluxes from bulk data or measurement; such parameterisations are required within all atmospheric numerical models, from Global Circulation, through Numerical Prediction to mesoscale models. The subject matter is therefore of great importance, especially in polar regions where the existing schemes for stratified boundary layers are deemed to have insuffi-

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cient accuracy. The paper, I think, suffers in a two main areas. Firstly I would ask the authors to dwell on what the aerodynamic roughness length for temperature, z_T , and the surface temperature, T_0 , actually are. z_T is analogous to the roughness height, z_0 , the latter being the height at which the wind speed extrapolates to the surface wind ($= 0$ m/s). T_z is the extrapolated temperature at this height. The relation between these terms, therefore, depends on the form of the extrapolation function. As the authors point out, there is uncertainty, both in definition and in measurement techniques for the (z_T, T_0) pair. For instance, in terms of definition, three choices are possible 1. Use a known function, $f(z)$, with z_T to define T_0 . 2. Use a known function with T_0 to define z_T . 3. Choose T_0 and z_T to constrain the extrapolation function. Historically, (1) and (2) have been attempted, as the log-lin profile for temperature is assumed in the surface layer: $T = a + bz + c \cdot \ln(z)$: in implicit assumption here is that there is no radiative divergence. This function is used either directly (fitting $[a \ b \ c]$ to $T(z)$ data) or indirectly through similarity functions and direct measurements of u^* and $w'T'$ from sonic anemometry. Method 1 usually assumes z_T is identical to z_0 , and thence is concerned about difference between T_0 and the "measured" surface temperature, T_s . Method 2 assumes $T_0 = T_s$ and then discusses the differences between z_T and z_0 . The paper is not clear on this point, but I believe it effectively attempts Method 3, using a number of assumptions to estimate z_T and T_s , which are then used to validate a more complex "3LM" model. The paper needs to be very clear as to what is being measured and what is being tested or validated.

Secondly, once the methodology of the paper is clarified, I still have concerns regarding experimental and instrument error. For instance, measuring z_0 using profiles of cup anemometers generates large error. These estimates should be compared to that derived from the sonic anemometer under neutral conditions. Further concern results from the sentence at the top of page 7 starting: "Based on the on-site observed geometric roughness...", which implies that z_0 was estimated from what the surface looked like. Sonic anemometry will measure aerodynamic roughness in the order of 50 μm , despite observable sastrugi of 0.05 m (King and Anderson 1994). Similarly, z_T is then

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derived from equation (6) (using a mean u^*), instead of from either profiles or (better) from sonic anemometry measurements. Note that we would assume z_0 and z_T to be site specific and calculated over the whole data set, and this does not therefore imply circularity or self-correlation when comparing various methods of estimating sensible heat flux.

King JC and Anderson PS (1994) Heat and Water-Vapor Fluxes and Scalar Roughness Lengths over an Antarctic Ice Shelf. *Boundary-Layer Meteorology*. 69(1-2):101-121.

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