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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-

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, zT, and the surface temperature, T0, actually are. zT is analogous to the roughness height, z0, the latter being the height at which the wind speed extrapolates to the surface wind (= 0 m/s). Tz 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 (zT , ,T0) pair. For instance, in terms of definition, three choices are possible 1. Use a known function, f(z), with zT to define T0 . 2. Use a known function with T0 to define zT. 3. Choose T0 and zT 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.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 zT is identical to z0, and thence is concerned about difference between T0 and the "measured" surface temperature, Ts. Method 2 assumes T0 = Ts and then discusses the differences between zT and z0. The paper is not clear on this point, but I believe it effectively attempts Method 3, using a number of assumptions to estimate zT and Ts, 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 z0 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 z0 was estimated from what the surface looked like. Sonic anemometry will measure aerodynamic roughness in the order of 50 um, despite observable sastrugi of 0.05 m (King and Anderson 1994). Similarly, zT is then derived from equation (6) (using a mean u*), instead of from either profiles or (better) from sonic anemometry measurements. Note that we would assume z0 and zT 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16913, 2009.