We thank Dr. Smeets for his careful and critical reading of our discussion paper. Below is a point by point response to his comments.

General and Major Comments

Reviewer's comment:

The authors test turbulence statistics in the framework of the concept of local scaling using turbulence data from the South Pole region. They use publically available data from a region with scarce data coverage. This makes the contribution valuable. Furthermore, they apply very strict quality control criteria. Given the small turbulent fluxes in polar regions the latter certainly adds to the quality of the results presented in the manuscript. Nevertheless, I have my reservations concerning the use of these data to validate Nieuwstadts local scaling hypothesis. The latter considers universal functions of dimensionless variables throughout the whole stable boundary layer. However, the data presented were obtained over a very limited height interval close to the surface (3.1 and 7m).

Turbulence data obtained at only 2 heights close to the surface (3.1 and 7m) are used to validate Nieuwstadts local scaling hypothesis (i.e., dimensionless combinations of local values of turbulence variables are demonstrated to follow universal functions throughout the SBL). I have objections against the validation of this hypothesis with the data presented here. The height difference between the measurement heights is simply too small for a rigid validation of z-less scaling. As I see it now their measurements were obtained within the surface layer and they merely test M-O similarity ("constant flux layer relations"). If the authors would demonstrate the presence of substantial flux divergence within the lower 7m of the atmosphere this could add some justification for using this data to validate local scaling.

Authors' response: To address the reviewer's concern, we calculated the normalized flux differences (δ) from measurements at 3.1 m and 7 m, as follows:

$$\delta_{u_*^2} = \left| \frac{(u_*^2)_{7.0\ \mathrm{m}} - (u_*^2)_{3.1\ \mathrm{m}}}{(u_*^2)_{3.1\ \mathrm{m}}} \right| \times 100 \qquad (1)$$

$$\delta_{\langle w'\theta_c' \rangle} = \left| \frac{\langle w'\theta' \rangle_{7.0\ \mathrm{m}} - \langle w'\theta' \rangle_{3.1\ \mathrm{m}}}{\langle w'\theta' \rangle_{3.1\ \mathrm{m}}} \right| \times 100 \qquad (2)$$

We had 689 runs for which both the 3.1 m and 7 m sonic anemometer data were available (after quality control and post-processing). If constant surface flux layers exist, the δ values should be small (typically much less than 10%). However, Fig. 1 and Table 1 clearly demonstrate that quite often (more than 50% of the runs) the δ values were significantly larger than 10% for both momentum and sensible heat fluxes. In other words, the assumption of constant flux layer is not appropriate for the dataset we analyzed. Therefore, we did not test M-O similarity, rather we validated the local scaling hypothesis.

Table 1. Summary Statistics of Normalized Flux Differences

Normalized Flux Differences	50th percentile	75th percentile	90th percentile
$\delta_{u_*^2}$	10.5%	17.5%	25.3%
$\delta_{\langle w'\theta_c' \rangle}$	13.6%	21.9%	32.8%

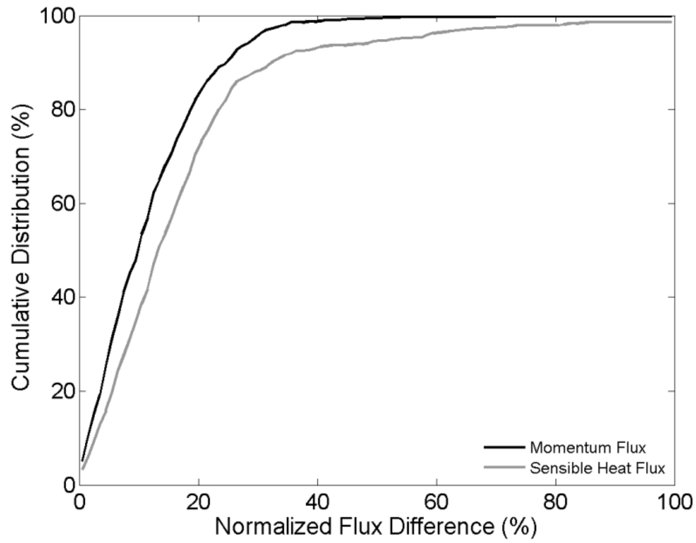

Figure 1. Cumulative distribution of normalized flux differences (see Eqs. 1 and 2).

Minor Comments

Reviewer's comment: Introduction: Nieuwstadts local scaling hypothesis and e.g. the essential stability parameter ξ (= z/L) are introduced using only a few lines but the authors should realize the wide audience of The Cryosphere (TD). I suggest a more elaborate introduction. With respect to parameters such as ξ and L, they need to be specified at the location where they are introduced in the document (i.e. this is done later on in the document or not at all).

Authors' response: The point is well taken. We will elaborate on these parameters in the revised manuscript.

Reviewer's comment: Introduction, p410, line 17: Sorbajn should be replaced with Sorbjan

Authors' response: Thanks for catching the typographical error. It will be corrected.

Reviewer's comment: Introduction, p411, line 3, 10 (maybe more): the references to Basu et al., 2006 are not unique and should be better identified as it is listed twice in the reference list.

Authors' response: We have Basu and Porté-Agel (2006) and Basu et al. (2006) in the reference.

Reviewer's comment: Section2, p412, line 12: with respect to earlier results on the local scaling aspects of SBL in polar regions a reference to King 1990 is made and an "…. amazing agreement …." is claimed. I cannot agree with this. In general the comparison of the polar night data from King with mid-latitude SBL can be characterized as problematic for most aspects except for the values of normalized variances for which the lower bounds agree with those found by Nieuwstadt, 1984.

Authors' response: The sentence in question reads as: "In spite of these physical differences, the local scaling properties of SBLs over an antarctic ice shelf (King, 1990) and the Greenland ice sheet (Heinemann, 2004) were found to be in amazing agreement with corresponding mid-latitude values."

King (1990) reported that the mean value of normalized vertical variance at 5 m was approximately 1.42±0.01. At 17 m and 32 m, corresponding values were 1.57±0.01 and 1.55±0.01. These numbers fall within the range of reported mid-latitude values. King (1990) did not provide mean values of other normalized variance statistics, only lower bounds were reported. The correlation coefficients reported by King (1990) were also in the vicinity of the corresponding mid-latitude values. For example, at 32 m, r_{uw} was found to be independent of stability and was constrained within the range of -0.3 to 0.

Heinemann (2004) wrote: "The local scaling concept was found to be valid for these strong katabatic wind conditions, and the KABEG results generally agree with mid-latitude results."

In the revised manuscript, we will rephrase our sentence. Furthermore, we will add more information from King (1990) and Heinemann (2004) to make this statement clear.

Reviewer's comment: Furthermore, the results of King 1990 are not used for comparison in Table 2, please include as these are amongst the very few results in polar regions.

Authors' response: King (1990) reported mean values only for normalized vertical variances. For other statistics, they usually reported a range for all stabilities. For this reason, we did not include them in Table 2.

Reviewer's comment: Section 4, p414, line 17: spikes in the raw 20 Hz found in the 7m sonic data set are claimed to be manifestations of blowing snow/ice crystals (besides spikes related to electronic interference). For cases with blowing snow the 3.1 m sonic data set would also suffer at least equally from spikes I guess.

Authors' response: We will rectify our assertion in the revised manuscript for TC.

Reviewer's comment: Section 4: A comprehensive data quality control as employed and described by the authors should also include instrumental corrections that are usually applied to eddy correlation data as a standard procedure by the flux community. I'm thinking of high frequency corrections due to the physical limitations of the instruments or its set-up. Corrections such as path length averaging are appropriate to sonic anemometer measurements e.g.. Crosswind velocity contamination of the sonic anemometer wind components is used to correct the sonic temperature signal (i.e., for this type of anemometer the correction is applied internally if I am correct mention this please). All of these corrections were not mentioned please include in your list and comment on them.

Authors' response: We are aware that Campbell CSAT3 and a few other sonic anemometers have internal cross-wind correction for temperature. By perusing the ATI-K manual online, it does not appear to have this internal correction (see also Mauder and Foken, 2004).

High frequency losses due to path length averaging, spatial separation of sensors, dynamic frequency response etc. can be computed if (i) one knows the transfer function for each correction, and (ii) one makes assumptions regarding the theoretical form of turbulence (co-)spectra under stable and unstable conditions. In the literature, spectral corrections and their associated uncertainties were found to be site specific (e.g., Massman and Clement, 2004). Furthermore, since there is no general consensus regarding the exact form of turbulence (co-)spectra (especially under non-neutral conditions), from our perspective, it is not desirable to introduce additional uncertainties in the flux estimations.

We would like to note that the published papers documenting ISCAT-2000 (e.g., Davis et al. 2004 and Oncley et al. 2004) as well as ISCAT's website (http://www.eol.ucar.edu/isf/projects/iscat2000/) do not provide any information regarding the aforementioned corrections.

References:

Massman, W. and Clement, R. (2004). Uncertainty in eddy covariance flux estimates resulting from spectral attenuation. In *Handbook of Micrometeorology*, Eds. Lee et al., pp. 67-99.

Mauder, M. and Foken, T. (2004). Documentation and instruction manual of the eddy covariance software package TK2. University of Bayreuth.

Reviewer's comment: Section 4, p 416, point 8 of quality control: though the quality control presented by the authors is strict and elaborate I would advise to change the following constraints: u*>0.1 m/s and w'T' > 0.01 K m/s (or at least 0.05 and 0.005, resp.). The reason for this is the accuracy of the eddy correlation measurements. This will of course result in less very stable runs thereby limiting the z/L range. The same is appropriate for the temperature gradient criterion: T7 – T3.1 > 0.1C or so (or at least 0.05C).

Authors' response: Given that we have very little scatter in the results, these additional constraints will not really improve our results. For this reason, we would like to keep our original constraints.

Reviewer's comment: Section 5, p 417, line 14: identify which "Basu et al., 2006" is meant

Authors' response: As mentioned before, we have only one Basu et al. (2006) in the reference. The other reference is Basu and Porté-Agel (2006).

Reviewer's comment: Figure 1, caption. Please leave out the last 2 sentences, these comments should be put in the text.

Authors' response: We will make this correction in the revised version.

Reviewer's comment: Figure 3, 4, 5: for reasons of clarity, please identify which normalized variances or correlation coefficients or third order moments you plotted inside the subplots. Indicate the specific variable in each subplot.

Authors' response: The figures will be amended as requested in the revised manuscript.