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

General Comment

Reviewer's comment: In this paper, the authors use surface-layer turbulence data collected at the South Pole to examine the local scaling similarity hypothesis proposed by Nieuwstadt. Stringent quality control procedures (clearly described in the paper) are applied to the data. Analysis of the quality-controlled data then shows that scaled variances and correlation coefficients are well described by local scaling. The paper is well-written, with the methodology clearly described. However, I do have some concerns (described below) over the authors' conclusions, which I would like to see addressed before the paper is published. I also have reservations about whether The Cryosphere is the appropriate forum for this paper. While the data used were collected over an ice sheet, the paper does not directly address the area of atmosphere-ice sheet interaction and, furthermore, the results of the paper should be applicable to stable atmospheric boundary layers over other surfaces. An atmospheric journal, such as Atmospheric Chemistry and Physics, JGR-Atmospheres or Boundary-Layer Meteorology might be a more appropriate forum for this work.

Authors' response: In the past, the first author published local scaling-related studies in atmospheric journals like *Boundary-Layer Meteorology* and *Journal of the Atmospheric Sciences*. These earlier studies focused on mid-latitude boundary layers. We agree that our present work on polar boundary layers could have been submitted to one of these journals. Instead, we chose *The Cryosphere* primarily because of its growing readership from the polar science community. The interactive and open access format of *The Cryosphere* was also appealing to us.

Major Comments

Reviewer's comment: The authors' main claim is that these data strongly support the local scaling hypothesis. While I would not dispute this, I am not convinced that the results as presented represent a strong test of that hypothesis. Given that both measurement levels were fairly low (3.1 m and 7 m), fluxes at the two levels will be similar for much of the time, so there will be little difference between locally- and surface-scaled quantities. If the authors want to substantiate their claim that the results strongly support local scaling they need to: i) demonstrate that their dataset does include occasions of significant vertical flux divergence and ii) show that, on these occasions, using local scaling gives "better" (i.e. more universal) results than scaling using surface (or 3.1 m) fluxes.

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

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

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

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. These results will be incorporated in the revised manuscript.

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%

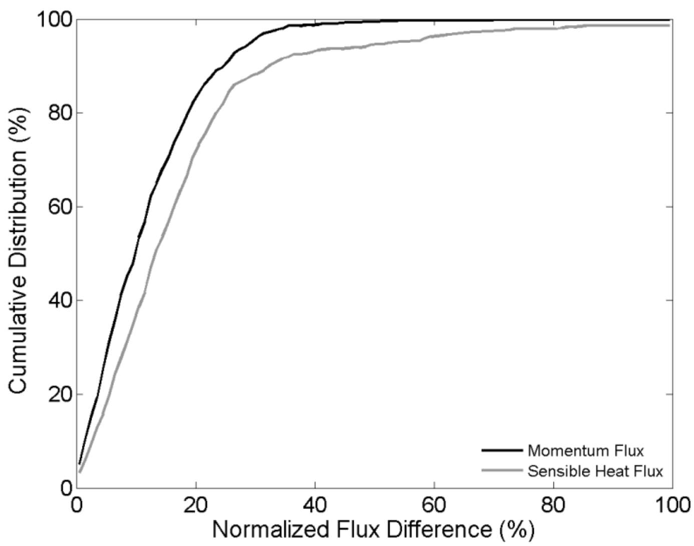

Figure 1. Cumulative distribution of normalized flux differences (see Eqs. 1 and 2).

We found that it is quite challenging to prove in an objective manner that "local scaling gives “better” (i.e. more universal) results than scaling using surface (or 3.1 m) fluxes". As an illustrative example, in Fig. 2, we compare local scaling (top panel) with surface scaling (bottom panel) for normalized vertical velocity variance. The only difference we are able to detect is that for stronger stabilities, the inter-quartile range becomes slightly larger (representing a bit more data scatter) in the case of surface scaling in comparison to local scaling.

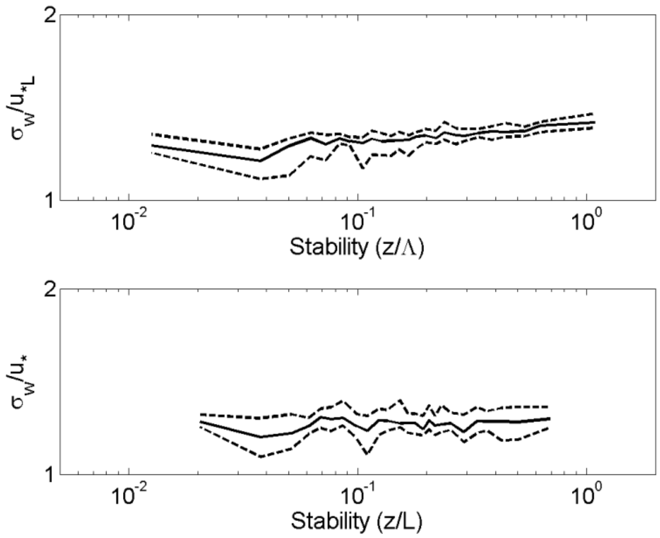

Figure 2. Normalized vertical velocity variance as function of stability. For both the panels, σ_w is calculated from the 7 m sonic anemometer data. The top panel utilizes local (i.e., 7 m) values of friction velocity and Obukhov length. In contrast, the bottom panel uses surface (i.e., 3.1 m) values of friction velocity and Obukhov length. The 50th percentile (median) is represented as a solid line in both the panels. The 25th and 75th percentiles are depicted as dashed lines.

Minor Comments

Reviewer's comment: The normalized variances shown in figure 3 show remarkably little scatter. This contrasts to similar plots shown in King (1990) and King (1993) (referenced in the manuscript). The contrast may reflect the stringent quality control procedures and the removal of "mesoscale" motions applied in the current paper and is worthy of further comment.

Authors' response: We agree with the reviewer. The effects of removing mesoscale motions on local scaling were clearly shown in Figure 2 of Basu et al. (2006), *Boundary-Layer Meteorology*, 119, 473-500. We will mention this fact in the revised manuscript for TC.

Reviewer's comment: On a related matter, I do not like the presentation of figures 3, 4 and 5 as line graphs and would prefer to see each data "bin" plotted as a point, with error bars indicating the interquartile range.

Authors' response: Following the editor's request, we reduced the total number of figures by introducing multiple subplots for each figure. In this format, boxplots or error bars make subplots extremely cluttered. In order to maintain the readability of the figures, we would prefer not to change the representation of the figures. We would like to emphasize that we do plot 25th, 50th, and 75th percentiles in Figures 3, 4, and 5. Thus, the readers should be able to assess the interquartile ranges.

Reviewer's comment: p 413, l 21: It is not very surprising that no diurnal cycle was observed as there is no diurnal cycle in radiative forcing at the South Pole.

Authors' response: We will rephrase this sentence in the revised manuscript for TC.

Reviewer's comment: p 414, l 17: Spikes observed at 7 m, but not at 3.1 m, are unlikely to be caused by blowing snow, as blowing snow particle concentration decreases with height.

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

Reviewer's comment: p 415, l 1-7: The planar fit method of tilt correction requires the wind direction to vary during the period over which the fit was applied. Was the wind direction sufficiently variable on all days to obtain a well constrained planar fit?

Authors' response: For the early part of the campaign, the wind was coming from the clean air sector. After December 12, large fluctuations of wind direction were present.

Reviewer's comment: p416, l 7: Obukhov length not yet defined.

Authors' response: It was our oversight. In the revised manuscript, we will make sure that all the variables are defined at appropriate locations.

Reviewer's comment: I presume that you have used the standard surface-layer convention that the u-component of wind is aligned with the mean wind direction, but I don't think this is mentioned anywhere.

Authors' response: Our yaw rotation approach closely follows the original methodology outlined by Wilczak et al. (2001) [specifically their Eqs. 45 and 46; also refer to Eqs. 3.42 and 3.43 of van Dyke et al. (2004)]. Essentially, we rotate the first coordinate axis into the mean velocity over the collection of 48 runs of 30 min durations (i.e., one day). This information will be added in the revised manuscript.

Reference: Van Dijk, A., Moene, A.F., and De Bruin, H.A.R., 2004: *The principles of surface flux physics: theory, practice and description of the ECPACK library*, Internal Report 2004/1, Meteorology and Air Quality Group, Wageningen University, Wageningen, the Netherlands, 99 pp.