**The Cryosphere
Discussions**

Interactive comment on "Local scaling characteristics of Antarctic surface layer turbulence" *by* S. Basu et al.

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General

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

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(3.1 and 7m).

Major comments

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.

Minor comments

Introduction: Nieuwstadts local scaling hypothesis and e.g. the essential stability parameter ς (= 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 ς 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).

Introduction, p410, line 17: Sorbajn should be replaced with Sorbjan

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.

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

C239

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. 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.

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.

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.

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).

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

Figure 1, caption. Please leave out the last 2 sentences, these comments should be put in the text.

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.

Interactive comment on The Cryosphere Discuss., 4, 409, 2010.