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The Cryosphere Discussions

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

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General

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

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Interactive Discussion

Discussion 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.

Major points of concern

1.) 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.

2.) 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. 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.

Minor points

1.) 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. 2.) 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. 3.) 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 varaible on all days to obtain a well-constrained planar fit? 4.) p416, l 7: Obukhov length not yet defined. 5.) 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.

John King

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