Biogeosciences Discuss., 7, C2145–C2148, 2010
www.biogeosciences-discuss.net/7/C2145/2010/

Biogeosciences Discussions

Interactive comment on "Local flux-profile relationships of wind speed and temperature in a canopy layer in atmospheric stable conditions" *by* G. Zhang et al.

Anonymous Referee #2

Received and published: 2 August 2010

Advise: accept subject to minor revisions

General comment:

The authors investigated local flux-profile relationships in a pine forest under stably stratified conditions. The present their results in a solid, clear and (rather, could be improved) concise way, which leads to clear take home messages to the reader (this is not trivial as usually canopy turbulence is subject to a large amount of scatter, usually blurring clear results). Due to the fact that they pay specific attention to the (indeed dangerous) self-correlation between e.g. Phi_M and Ri, their results attain credibility and as such those results are interesting to the community of boundary layer meteo-

rology. Finally, the authors succeed in reviewing large part of the relevant literature on this topic.

Minor comments

-pp 3, last two lines: I think could be more subtle here and make a distinction between idealistic and non-idealistic cases: from direct numerical simulation of the Navier-Stokes equations Van de Wiel et al. (2008) show that in *true homogeneous and stationary conditions* the log-linear Phi-m,h remains valid even for strong stability (!). However, as indeed indicated by Mahrt (2007), in atmospheric practice the functions tend to level off (in a non universal way) mainly due to effects on non-stationarity and non-homogeneity.

-pp 5, line 7: *"It suggests that,. . . .measurements".* Indeed, probably you also refer to the fact that in the unstable boundary layer (mainly aimed for when formulating MO originally), fluxes are generally large and gradients of mean variables small (thus Phi vs. z/L works better than Phi vs Ri) , whereas for stable boundary layers the reverse is true (thus Phi vs Ri works better). . . see indeed Baas et al. (2006).

-pp 7: I just wonder: by using discretized version of the local Ri, one generally somewhat overestimates the actual value. Did you look into this, or do you think the number of observational levels is sufficient? At least this might explain some quantitative deviations from other studies mentioned later in the paper.

-pp10, figure 2. This figure really would benefit from two extra graphs viz. U(z) and Theta(z). Now immediately gradients are given. In the paper you often implicitly refer to the shape of those (not shown) graphs.

Pp12, line 14: *"preventing the loss of long-wave radiation from the ground".* To my opinion radiation itself cannot be limited by stability, probably you refer to the fact that exchange of cold air from below (generated indeed by radiative cooling) is limited.

Pp12, line 18, figure 2D: In this figure I observe a large amount of scatter in the very stable case (large error bars). In the very stable case one expects some kind of reversed (convective) boundary layer within the canopy, indeed with counter-gradient transport (sinking cold air from top canopy). I recall from a study by Jacobs et al. that they showed that for those cases convective scaling with w* (based on canopy height and turbulent heat flux, or alternatively net-radiation/or alternatively ground heat flux), worked much better than u* scaling (u*-scaling indeed is more applicable for weakly stable cases). Could you check if this type of scaling (using either is indeed more physical in the very stable case and reduces the scatting.

-same lines: the way figures 2d and 2c are presented seem a bit in contra-diction. Where does the counter gradient transport come from (as gradient of temperature seems positive even at top of canopy? I think the result is blurred by averaging. The very stable class could be subdivided in:

-cases with negative dT/dz at the top (sinking cold air-counter gradient possible in the middle)

-cases with positive dT/dz ($\sim$ as in the weakly stable case)

Or at least discuss this point in the text.

-pp14 lines13-22: wording is a bit unclear to reader; could use clarification

-pp18, lines 20-22: I really appreciate this comment, as the reader suspects a remark on this.

-pp21, lines 15-17: *"This suggests . . . .otherwise valid local phi_h and Ri relationships."*. This is a serious issue: to my opinion if SBL scaling between Phi_h and Ri does not work because of counter gradient turbulence (which I can understand) it cannot still apply to Phi_m (why should it then physically speaking). Please give your opinion on this interpretation.

-pp22-23: this section really improves the credibility of the results!

Literature:

Jacobs, A. F. G., B. J. H. van de Wiel, and A. A. M. Holtslag, 2001: Daily course of Skewness and Kurtosis within and above a crop canopy. *Agric. Forest Meteorol.,* **110**, 71-84.

Interactive comment on Biogeosciences Discuss., 7, 4505, 2010.