Atmos. Chem. Phys. Discuss., 9, C8883–C8890, 2009
www.atmos-chem-phys-discuss.net/9/C8883/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A semi-analytical solution for the mean wind profile in the Atmospheric Boundary Layer: the convective case" *by* L. Buligon et al.

L. Buligon et al.

lidianebuligon@gmail.com

Received and published: 22 December 2009

First of all, we would like to thank the reviewer for the careful and deep analysis he presented of the manuscript. Answers to each of the points raised by the reviewer are listed below:

Anonymous Referee 2

General comments:

I believe this manuscript to be suitable for publication in ACPD, subject to the co-

ments and technical corrections listed below. The scientific and technical quality of the manuscript is good, and it is clearly written. A new approach (GITT) to an old problem (calculation of mean wind profile in ABL) is presented, and it is discussed in sufficient detail to enable readers unfamiliar with the method to follow through. The references are adequate and useful, and the reader benefits from a clear and well-written introduction that sets the context of the problem and outlines the methodology of the paper. The one main problem I have with the manuscript is to do with the discussion of the results in comparison with the experimental data - I feel that the authors should discuss the discrepancies in more detail, and make suggestions of how the model could be improved.

Specific comments:

Item 1. *Please include a brief discussion of the physical assumptions behind equations* $(2\,a)$ *and* $(2\,b)$.

The equations are valid if the deformation components of the wind field are neglected. This statement has been added to the manuscript.

Item 2. *Section* 5.1*, last paragraph, sentence beginning "The horizontal variation . . . ": I am not sure I can see this from the plots. Perhaps more cases need to be computed to support the authors'conclusions*.

Lines for ($x = 0.4L_x,\ y = 0.4L_y$) and ($x = 0.45L_x,\ y = 0.45L_y$) were added to Figure 2, allowing the visualization of the convergence.

Figure 1

Item 3. *Section* 5.2*, 2nd paragraph, first line: The statement that the simulated profiles "are similar" to the observd ones is too vague. There are clearly large differences (eg. the shape of the profile is not captured well). These must be listed and discussed*.

This comment has also been addressed by reviewer 1 and this is the reply we sent him/her.

Indeed, the affirmation that the profiles are "similar to those observed in Wangara" is incorrect. What we meant, and should have been written, is that the mean wind magnitudes are similar between model and observations. It has been corrected in the revised manuscript. Anyway, it is still important to understand why the model is incapable of solving the detailed shape of the observed vertical wind profile. The following paragraph, included in the manuscript, addresses this question.

The mean wind magnitudes simulated by the model are similar to the average magnitudes observed at Wangara (figure 3). It is important to stress that such agreement concerns only the vertical overall average, but not the local maxima and minima observed at day 33, which characterize an unmixed wind profile. Indeed, such vertical variability is quite difficult to capture with a simplified model, as stated by Wyngaard (1988): “unfortunately, our knowledge of PBL physics does not yet allow us to calculate the wind profile from first principles . . . ”. Unmixed wind profiles, such as those observed at day 33, may be attributed to a number of reasons, such as local baroclinicity or vertical eddy diffusivity variability. Any of these reasons are, however, case-specific, and cannot be reproduced by a model where thermal wind is assumed to be constant.

Item 4. *Section* 5.2*, 2nd paragraph, sentence beginning “However, an analysis based on statistical indices . . . ”: please explain in more detail, referring to specific indices.*

Again, this has been suggested by reviewer 1, to whom we replied this way:

The following appendix was added to the manuscript.

Appendix

Following Hanna (1989) the statistical indices used in this study are defined as:
NMSE= $\frac{\overline{(C_o - C_p)^2}}{\overline{C_o}\ \overline{C_p}}$ (Normalized Mean Square Error)
$FB = \frac{(\overline{C_o} - \overline{C_p})}{0{,}5(\overline{C_o} + \overline{C_p})}$ (Fractional Bias)

$FS = 2\frac{(\sigma_o - \sigma_p)}{(\sigma_o + \sigma_p)}$ (Standard Fractional Bias)
$R = \frac{\overline{(C_o - \overline{C_o})(C_p - \overline{C_p})}}{(\sigma_o \sigma_p)}$ (Correlation Coefficient)
$FA2 = 0,5 \leq \frac{C_o}{C_p} \leq 2$ (Factor of 2)

where C is the analyzed amount and the subscript o and p refer to observed and predicted quantities, respectively, the over bar indicates an averaged value. The statistical index FB says if the predicted quantity underestimates or overestimates the average observed ones. The statistical index $NMSE$ represents the quadratic error of the predicted quantities related to the observed ones. The statistical index FS indicates the as the model gets to simulate the dispersion of the observed data. The statistical index $FA2$ supply the fraction of the data (%) for the ones which $0,5 \leq \dfrac{C_o}{C_p} \leq 2$. The best results are expected to have values near zero for the indices $NMSE$, FB and FS and near 1 in the indices R and $FA2$.

The following paragraphs, with the interpretation of the statistical indices, were also added.

Regarding the vertical profiles for day 33 (Figure 3), the analysis based on statistical indices shows that, when $\delta = \zeta = 0$ and $\delta = \zeta = -f_c$, the model overestimates the mean observed wind magnitude (small negative values of FB). On the other hand, the statistical index FB shows that the horizontal wind direction is underestimated regardless of δ and ζ, meaning that the modeled winds are rotated counterclockwise with respect to the observations. The statistical index FS indicates that, except for the case $\delta = \zeta = 0$, the dispersion of the mean wind magnitude underestimated the experimental data. For the wind direction, this same index is negative in all cases, a consequence of the very small wind direction variability with height in the observed data, while the model results indicate a slight wind rotation with height. Other indices, such as $NMSE$, and $FA2$ are similar for all cases, and indicative of good agreement between model and observations. Finally, the correlation coefficient R was more variable, and therefore, serves as a measure of the best agreement in each case.

A similar analysis of the statistical indices as that made for day 33 can be made for day 40 (Tables 4 and 5).

Item 5. *Section* 5.2*, 2nd paragraph, last sentence "... when both divergence and vorticity are positive": there are not such cases listed in Table 3.*

The Coriolis parameter is negative, causing the confusion. A remark on this fact has been added to the manuscript.

Item 6. *Section* 5.2*, 3rd paragraph, line 2: "even better" suggests that the comparison for day* 33 *is quite good (which I do not believe it is). I therefore suggest removing the word "even".*

We followed the reviewer's suggestion.

Item 7. *Conclusion, 2nd paragraph, first sentence: please elaborate by showing and/or discussing the comparisons with Wilson and Flesh (2004) and Stull (1988).*

The following sentence was replaced to the conclusion.

The model provided a good comparison to the observed data from Wangara experiment. The mean wind magnitudes are similar, although the model is not able to reproduce the unmixed character of the profiles. This is, however, a very complex task, not achieved by previous analytical results that provide a very good approximation, such as those by Wilson and Flesch (2004).

Item 8. *Conclusion, 2nd paragraph: it would be useful at this point to discuss how the method or results could be improved to better reproduce the experimental measurements.*

The following sentence was added to the conclusion.

The results may be improved further employing more realistic boundary conditions.

Besides, the selection of other eddy diffusivity profiles may lead to improved solutions. In this sense, the methodology developed in the present study is generic, allowing other eddy diffusivity profiles to be considered.

Technical comments:

Item 1. *Abstract, line 3: "combines " instead of "joins "*

The suggestion was accepted.

Item 2. *Section* 2.1*, equations* $(1\,a)$ *and* $(1\,b)$*: several symbols in the equations have not been defined.*

The symbols will be defined.

Item 3. *Section* 2.2*, line* 2*: for clarity, I suggest moving the* 2 *sentences beginning "Laterally ... " to after equations* (5).

The suggestion was accepted.

Item 4. *Section* 3*, equation* (8)*:* ψ *and* λ_{pq} *need to be defined.*

The following sentences have been added to the manuscript (after of the equation (13)).

with $\psi\left(\lambda_{pq}, x', y'\right) = \psi_1\left(\beta_p, x'\right)\psi_2\left(\gamma_q, y'\right)$ and $\lambda_{pq}^2 = \beta_p^2 + \gamma_q^2$.

Item 5. *Section* 5*, last line: "wind profile" instead of "restrained".*

The suggestion was accepted.

Item 6. *Conclusions, line* 4 *from end: "restricted" instead of "restrained".*

The suggestion was accepted.

Item 7. *References: Hanna (1989) is not cited in the text.*

Hanna (1989) is cited in the appendix B (see **Item 4.** - Specific comments).

Item 8. *Tables* $2-5$*: There is no explanation of what "NMSE", "FB" etc stand for.*

The comment presented in **Item 4.** (Specific comments) applies here as well.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 19817, 2009.

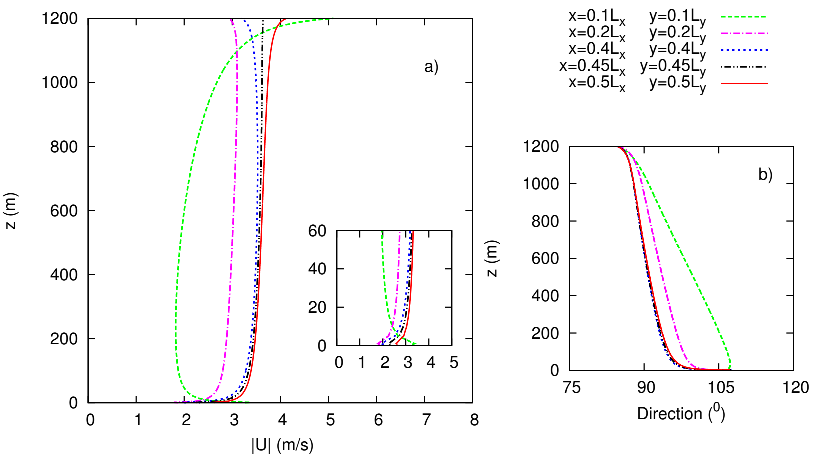

Fig. 1. Simulated vertical profiles of a) wind magnitude and b) wind direction, for different positions within the domain, as indicated in legend.