Atmos. Chem. Phys. Discuss., 9, C8330–C8332, 2009
www.atmos-chem-phys-discuss.net/9/C8330/2009/

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.

Anonymous Referee #2

Received and published: 14 December 2009

General comments:

I believe this manuscript to be suitable for publication in ACPD, subject to the comments 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:

1. Please include a brief discussion of the physical assumptions behind equations (2a) and (2b).

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.

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

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

5. Section 5.2, 2nd paragraph, last sentence "... when both the divergence and vorticity are positive": there are no such cases listed in Table 3.

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".

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

8. Conclusions: 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.

Technical corrections:

ACPD

9, C8330–C8332, 2009

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

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

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

4. Section 3, equation (8): psi and lambda_{pq} need to be defined.

5. Section 5, last line: "wind profile" instead of "wind field".

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

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

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

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