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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of conventional Lagrangian stochastic footprint models against LES driven footprint estimates" *by* T. Markkanen et al.

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This manuscript provides comparison results of three different footprint models. Two conventional Lagrangian stochastic footprint models are validated with a reference model basing on large eddy simulation of turbulence fields. The property of two-dimensionality of footprint is emphasized. A method is proposed to classify the agreement of the 2-D results between models.

Footprint is a useful tool in experimental data analysis. Footprint validation is still an open question recently. However, the present work is far from a systematic validation. Furthermore, there are major shortcomings in the design of the model comparison.

General comments:

1) This is only a first-step work with just two atmospheric stability conditions, both in unstable state. Wider range of stabilities should be considered for a systematic validation.

2) There is a major deficiency in the design of models comparison: three different models are directly compared their results. Therefore it is hard to interpret the difference among results. For example, is it raised from different turbulence parameters? Or is it just caused by different numerical schemes and boundary treatments among the models? To simplify/modify the respective models into a common state will provide better bases for model comparison.

3) There is not enough information for the large eddy simulation case used in this paper. For example, the running length in time, the coupling method of the LES and the LS scheme, the details of particle release and footprint calculation, etc. Though the authors do refer the information to a previous paper (Steinfeld et al. 2008), key information in this independent work is still need for readers.

Specific comments

1) Pg.4196, line 6-7: "We assess their performance in unstably and neutrally stratified boundary layers...", the two cases are both very unstable, the case2 is just convective or unstable, but more neutral case, with Obukhov length L=-77 m, boundary layer height zi˜500 m. So zi/-L ˜ 7 $>$5, very unstable. All related saying about "near neutral case 2" need modify.

2) Pg.4197, line 19: "andthe" should be "and the".

3) Pg.4199, line 28-29: "The results are normalised by the fraction of the boundary layer lying below the source height", Do you mean they use a special normalization? It seems not so.

4) Pg.4200, "footprint model by Univ. of Helsinki (Rannik et al., 2000) which derives the footprints from dispersion data according to Kurbanmuradov et al. (1999) the concen-

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tration footprint functions tend to infinity when normalised only by the simulated particle number", Why for the difference?

5) Pg.4201,line 6-8: need to add more details of the LES-LS model running and footprint calculation.

6) Pg.4205, line 10-20: the negative flux footprint, why does not it tend to be zero in far away? 5000 m is about 10 time of zi in the case1, the particles should be well mixed vertically in the CBL. So the negative flux footprint should tend to zero again, as we expected. On the other hand, what is the influence of the increasing height of the CBL? And what is the magnitide of CBL height variation during the footprint calculation?

7) Pg.4205, line 22: "Under the nearly case 2", nearly what?

8) Pg.4205, line 26-27: "we normalise the footprints by the total contribution from the whole domain area of fixed size", if the negative contribution is physically real, why do not include their role?

9) Pg.4208, line 8: "As the Coriolis force (CF) was not considered in the basic LES runs", if there is not horizontal pressure gradient yet, the mean flow in the CBL should be decaying. What is its effect on footprint result? Particularly if you use it as a reference run to "validate" the conventional footprint models.

10) Pg.4208-4209: "As under influence of the CF the mean wind direction turns clockwise from the surface towards the top of the ABL, the mean wind direction at each observation height was separately set identical to that of the respective simulations without the CF". This treatment complicates the matter in discussion. The LES information put for footprint calculation may be confused or even illness: since the final mean flow is that without CF, but the the turbulence is in the state with the influence of CF. So the comparison may be meaningless. In the LES run with CF, what is the mean pressure gradient and how it balances with CF in the CBL? And the run without CF, how the mean flow and resolvable turbulence develop temporally? These might be key

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issues in the comparison.

11) Pg. 4210, line 20: "near neutral case 2", unstable but more neutral case 2, or weaker unstable case 2.

12) Pg.4212, line 6-7: "Again the obvious reason is the disagreement of peak positions among the two models", This shows the un-comparablity of this two models, perhaps the difference in mean winds is more inportant for this disagreement.

13) Pg.4214, line 9: "under near neutral coitions", please see 11)

14) In figure captions: "in case 2 (neutral)", please see 11)

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4195, 2009.