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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.

Anonymous Referee #4

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General:

This paper attempts at finding a suitable methodology for comparing footprint estimates from different types of 'conventional' footprint models, namely forward and backward type Lagrangian stochastic particle approaches. This methodology is then applied to 'validate' these models against LES. While the comparison contains quite some arbitrariness and will most likely not become THE new standard approach, it still provides useful information and is worth being considered. I have, however, two rather major comments and quite a few minor comments to be addressed before full publication can be recommended.

Major or specific comments:

1) The first comment concerns the Coriolis force (CF). The motivation for taking CF into account is briefly introduced in Section 2.4 by stating that the areas of positive and negative (mean) vertical wind were 'erroneously' distributed symmetrically across the mean wind direction, which was due to the CF. Basically the authors claim that the sensitivity to introducing CF or not is on the order of 10-30% for the distance of the peak. For 'sensor' heights close to the surface they furthermore find (up to) a factor of three difference for the footprint areas at certain percent levels. Still due to the fact that the two stochastic Lagrangian models to be validated do not include CF, the comparison is presented to the non-CF LES runs. While (or because) I can follow this argumentation, I ask myself why then the runs with the CF are presented at all. This in particular because the authors do not in any sense refer to the CF-including LES when discussing the results. In fact in the Conclusions section these are not even mentioned.

2) The authors show in Fig. 1a a cumulative cross-wind integrated footprint (for heights 100m and 350m, respectively) which decreases with increasing up-wind distance (between 3000m and 5000m). If the cumulative footprint decreases, this means that the footprint is negative (!) in this range. The authors explain this with areas of dominantly positive and negative vertical velocities in the convective case. This is certainly to be expected in a CBL. However, the total sampling domain should be such that many up- and downdrafts are sampled at each distance in order to make the total cross-wind integrated footprint at least to be zero. Otherwise we are faced with a negative total cross-wind integrated footprint and this is physically extremely hard to explain for homogenous terrain. It appears that the lateral domain size of 1000m is too small in order to sample a sufficiently large amount of up- and downdrafts, given the typical size of these structures in a CBL. This issue should be addressed by either extending the lateral domain size or by adding convincing arguments on why, for a range as large as 2km, the total cross-wind integrated footprint can be negative.

Minor or technical Comments:

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P4200, l.23 In any case, it would be desirable to learn what the height of the domain top is.

P4200, l.25 'simulated online': even if one understands what is meant, usually this is called a coupled simulation.

P4201, l. 4 'exceptionally high number of particles': it would be interesting to know how large the number of particles actually was.

P4201, l. 12 '...which satisfied the WMC...': it probably still does satisfy that condition.

P4201, l. 21 Please add information on where (and how) reflection is effectuated in this model. Also: how many particles were released?

P4202, l. 6 How did the authors determine the roughness length of 0.14m?

P4202, l. 7 '...and the particles were released...': The 'case to be modeled' has not even been introduced at this stage of the paper, so it is hard to judge this statement. In any case, it would be appropriate to at least add the information that this datum was considered to be 'the surface' for the particle release in the forward model.

P4202, l. 9 Please add information on where (and how) reflection is effectuated in this model. Also: how many particles were released?

P4204, l. 19 ON the order of (not of the order of)

P4204, l. 25 Add colon: '...to compare both, sizes of the areas...'.

P4205, l. 22 'Under the nearly case...': replace by: near-neutral case. Within the paper, the near-neutral case is some times referred to a neutral, sometimes as near-neutral. This should be made consistent.

P4206, l. 16 replace 'grid' by 'grid cell'.

P4206, l. 20 consists, not consist

P4207, l. 27 replace Table 4 by Table 2

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P4207, l. 28 replace schema by scheme.

P4208, l, 14 '.produced peaks...': of what?

P4209, l. 15 'up to threefold...': this is extremely hard to see in the figure...;

P4210, l.2 Figures 5-8 are mixed up. Figs. 5/6 reach up to 400m but in the caption Fig 6 is referred to as being for the FW model, which is supposed to only reach 100m. The same is true for Figs. 7/8. Either the captions are wrong or the wrong data are being displayed.

P4213, l. 21 In the references it seems that some of the authors are inconsistently cited. Leclerc is sometimes Leclerc M., and sometimes Leclerc M.Y., Horst is T. or T.W., Rotach is M. or M.W., Weil is J. or J.C., Finnigan should be J.J., Rannik is Ü. or S.R.Ü. This should all be made consistent by using the first and middle names.

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

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Interactive Comment