Review of "Numerical framework for the computation of urban flux footprints employing large-eddy simulation and Lagrangian stochastic modelling" by M. Auvinen, L. Jarvi, A. Hellsten, U. Rannik, and T. Vesala

This is a worthwhile study and paper, and I support the approach of using a high fidelity LES and Lagrangian stochastic (LS) model to determine footprints in an urban area. I have two main comments and several detailed ones for the authors to address, but overall believe that the paper should be published subject to the attention of the comments.

Main Comments

1) The first issue centers on: How well can one determine the $\overline{w}$ at the target volume and the uncertainty associated with it since this affects the uncertainty in the footprint through Eqs. (4) and (5). The authors argue that a well-defined value of $\overline{w}$ cannot be determined at the target site for reasons discussed on pages 9 (lines 10 - 21) and 13 (lines 24 - 33), and that this limitation leads to the negative bias in the far-field footprint (Fig. 5). They go on to develop the far-field correction approach (pages 13 to 15), which involves a correction coefficient c_{ijk} applied to the original $\overline{w}$.

I believe that the authors should provide further information to quantify the uncertainty in $\overline{w}$ at the target site (volume) and also upstream of it. In particular, they should compute and provide: $\overline{w}$, σ_w, and the uncertainty in $\overline{w}$ (in a statistical sense) from their numerical wind field data: 1) over the urban landscape upstream of the target site but at the same height (or over the same heights) as the target volume [an areal average over the upstream domain or volume using a few heights to match those of the target volume and extending from the leading edge of the city, $x \simeq 2$ km (Fig. 2), to the Hotel Torni], and 2) over the target volume. The difference in the raw w values over the target volume (larger) and upstream area (smaller) presumably should lead to a larger $\overline{w}$ and greater variance (σ_w^2) over the target volume, fewer number of numerical data points, and greater uncertainty in $\overline{w}$ over the target volume (e.g., uncertainty estimated as some factor λ depending on the w PDF times $\sigma_w/\sqrt{N}$, N being the number of points).

Furthermore, it would be useful to compute the subgrid-scale (sgs) turbulent kinetic energy (TKE, E_s) and the LES resolved scale TKE (E_r), and their ratio or an sgs rms velocity, $\sigma_{sgs} = (2/3)E_s$ and the ratio, σ_{sgs}/σ_{wr}. This ratio would quantify the importance or not of the sgs velocities in determining the footprint; as with the $\overline{w}$, σ_w, etc., this should be done for both the upstream area/volume and the target volume to make the distinction if there is one.

2) Section 2.3.2. This section is one of the more limiting parts of the paper in being somewhat arbitrary and subjective for generating the final LES-LS footprint. I believe that it is sufficient as an initial approach/procedure. However, for use by other researchers or as a standalone method for generating footprints (by others) for multiple EC sites in a city, a more rigorous, robust, and less subjective approach is necessary. For example, on page 16, is the $f^{(0)}$ with 4×10^5 particle entries deemed insufficient as a

reference footprint, f^{REF}, because there is too much blue (negative footprint value) in the far field of the footprint (Fig. 6)? Conversely, is $f^{(1)}$ judged to be sufficient and act as the reference because there is less blue? There should be some quantitative method, index, or variable that stipulates the adequacy of the reference (and why); e.g., requiring the far-field negative fooprint to be less than some small fraction of the maximum footprint. Such a criteria is used in deciding whether a candidate footprint, $f^{(k)}$, should be included in the final assembly (page 16, lines 31 - 33) with the $||\Delta f||_{max}$ "determined according to the case-specific requirments." These requirements should be spelled out. However, the maximum values, $f^{(k)}_{max}$, associated with these difference maxima, δf_{max}, and the fraction, $\delta f_{max}/f^{(k)}_{max}$, should be given. The $||\Delta f||_{max}$ here was chosen from the $f^{(3)}$ value, but it is not clear why. Also, the maximum footprint values in Fig. 6 appear to be about 10^{-6} (bright red), but the $||\Delta f||_{max} = 6.24 \times 10^{-3}$ is much larger. I don't understand this.

Detailed Comments

1) page 2, line 4. "...which relates the value of a measurement (of flux or concentration) at location ..." The parenthetical words would make this clearer.

2) page 2, Eq. (1). It should be made clear here that the footprint, $f(\mathbf{x}_M, \mathbf{x}')$, has the dimensions inverse of the integration unit(s). In Eq. (1), this is just length (L) since the integration is along a line in the domain, e.g., f could be the crosswind-integrated footprint, f^y, used later. But often, the footprint pertains to an elemental area $dxdy$ (L^2) as in Eq. (4).

3) page 3, lines 8, 9. "...conduct tracer gas experiments, which are nearly impossible to arrange in residential areas." However, this computational framework could be tested in some way in a large wind tunnel such as WOTAM at the University of Hamburg (e.g., Leitl and Schatzmann, 2010). This tunnel with dimensions of 4m × 3.2m × 25m (width, height, length) has been used to study many aspects of dispersion in large European cities. I would recommend that testing of the LES-LS numerical Helsinki footprint model be tested in some way in such a tunnel. It would give greater credibility to the approach especially when using the approach to suggest/demonstrate limitations in analytical footprint models.

4) page 11, line 11. The authors talk generally about the target subvolumes, size, and number, but this should all be guided the size/scale of the turbulent flow structures at the EC sensor site. This was mentioned earlier in the paper, but no specifics were given. For example, on page 9 (line 32), it is merely stated that "the target box reasonably represents the sensor site." Should the target box scale or total volume be of the order of or less than (say some fraction of) the characteristic flow structure dimension/volume at the EC site? If we assume very crudely that the characteristic length scale is $\ell_c = \kappa z$, with κ being the von Karman constant (0.4) and z is height, $\ell_c = 24$ m; i.e., for a neutral boundary layer over homogeneous terrain. The characteristic dimension of the sampling volume is $\ell_{sc} = (\ell_{sx} \cdot \ell_{sy} \cdot \ell_{sz})^{1/3} = 12.4$ m, which is half of ℓ_c. I don't know the logic of choosing the ℓ_{sc} in the paper, but requiring it's overall dimension to be less or much less thant ℓ_c may be a useful criterion. Clearly, there may be others.

5) page 12, line 19. “distributions” Do you mean individual “footprint” distributions?

6) page 14, line 3.“negligbly small offset.” But that offset is 20% to 25% of the maximum $\overline{f}^y$ in Fig. 5, and thus is not small.

7) page 15, Eq. (9). It might be noted that a similar approach is often used in determining the vertical tilt axis of a sonic anemometer on a meteorological tower; i.e., due to the mounting imprecision and/or possible variability in the sensor vertical axis over time (wind loads, vibrations, etc). Assuming that over a long time record above flat terrain the $\langle \overline{w} \rangle = 0$, the tilt axis ($\mathbf{c}$ or c_{ijk}) (relative to the 3 fixed coordinates) is chosen by ensuring $\sum_n^N \mathbf{c} \cdot \mathbf{u}_n = 0$ (sum of the vertical velocity components is zero), where n is the measurement (realization or time) index and N is the total number of measurements.

8) page 20, lines 8 - 29, Fig. 9. Why is the numerical footprint, $\overline{f}^y$, so much more peaked than the analytical Korman and Meixner (KM) (2001) model? There are at least two potential reasons for this as suggested by the authors. 1) The real urban terrain/fetch upstream of Hotel Torni is only about 1600 m or about 39% of the total LES domain (4096 m; Fig. 2). The real terrain which is heterogeneous with a marked change in roughness at the upwind edge of the city is accounted for in the LES, whereas the KM presumably assumes homogeneous conditions upstream. These very different mean wind and turbulence fields could possibly be accounted for in the KM model in a crude way (point 9 below). 2) The mean wind and rms lateral turbulence component (σ_v) for the KM model were extracted from the measurements at the top of the Hotel Torni. The authors should repeat the KM calculation using the LES mean wind and σ_v at the height of Hotel Torni. This could be done using both the LES values 1) within the target volume and 2) the upwind U and σ_v values. That is, there should be consistency in the meteorological inputs to the LES-LS and the KM model.

9) For future development of the KM or other analytical footprint models, it would be interesting to run KM or other model for an x-direction terrain inhomogeneity, sea-to-land, with a large increase in roughness at the interface to see if that would give a zeroth order change/improvement in the analytical footprint. [Note, this is not the authors’ responsibility and need not be addressed by them.] Horst and Weil (1994) and perhaps others have shown how a step change in the surface flux can be accomodated. Here, however, it would be necessary to account for a change in the mean wind and turbulence profiles at the land-sea interface. This may be possible to do in a simple way (parameterization) based on an internal boundary layer model characterized by a sudden roughness change (and/or heat flux) (e.g., Brutsaert, 1984; Garrett, 1992) where new wind and turbulence profiles can be developed downwind of the change.

Additional References

Brutsaert, W., 1984: *Evaporation into the Atmosphere*, Reidel Publishing Co., Dordrecht, 299 pp.

Garrett, J.R, 1992: *The Atmospheric Boundary Layer*, Cambridge University Press, 316 pp.

Horst, T.W., and J.C. Weil, 1994: How far is far enough?: The fetch requirements for micormeteorological measurement of surface fluxes. *J. Atmos. Oceanic Tech.*, **11**, 1018–1025.

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