Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-302-RC1, 2017

Geoscientific
Model Development
Discussions

Interactive comment on "Numerical Framework for the Computation of Urban Flux Footprints Employing Large-eddy Simulation and Lagrangian Stochastic Modeling" *by* Mikko Auvinen et al.

Anonymous Referee #1

Received and published: 22 February 2017

General considerations

In this contribution the authors present a methodology to calculate footprints in urban areas based on (very) high-resolution large eddy simulation (grid resolution 1 m) in combination with Lagrangian stochastic particle modeling. The need for this endeavor is motivated by stating that the usually employed analytical footprint models cannot be expected to yield useful results in urban areas. This is a more-than-valuable argument – despite the fact that there are less simplified options than analytical models and despite, more importantly, the astonishingly good performance (at least in the judgment of the present reviewer) of such an analytical footprint model, see major comment below. Due to the very high resolution employed in their modeling approach, the novelty

of their approach and some of the assumptions (see below) the authors encounter a number of problems ('numerical difficulties' (p6, l.7) to be worked around, or negative footprints (Fig. 5) to be avoided, etc.). These are all addressed and for each a 'solution' is presented - that can be defended (and is somehow defended by the authors), but is not in every case very obvious or unique. Still, the authors treat their results 'as the truth' (Section 3, see major comments 6, 7 and 8) rather than addressing the sensitivity of their results to their assumptions. I consider this to be a quite timely and important contribution to the problem of urban footprint modeling – but given the missing verification (which is indeed very difficult to achieve, as the authors state) I suggest to focusing more strongly on the sensitivity of the obtained results to the assumptions made, rather than presenting them as the final solution. In the major comments below I point to some of these assumptions.

Major comments

1) Quite influential treatment of the building - called 'topography' - model, (p6, points 1-3) and the flow simulation (precursor simulation with slip condition at the top, some sea surface roughness assumption, I presume, etc.). Probably, these are the result of some testing, e.g. point 3 of the building model. Can the authors comment on the impact on the simulated flow characteristics – and thus realism?

2) Release of particles 1 m 'above topography' (p8, l. 16): with the use of the word 'topography' in this paper, this means that surface emissions (traffic, say) and roof-top emissions (e.g., domestic heating) are treated to yield the same 'source' height'. In other words: the footprint function is clearly a function of all three spatial coordinates; if the height is defined in the way the authors do, this implies that '1 m above street' and 1 m above roof' (are treated to) experience the same physical processes, despite fact that one is indeed close to a solid surface below while the other is situated 'in the middle of the roughness sublayer', and possibly located above a slated roof. This refers to a very specific understanding of 'surface' that is distinctly different from other possible treatments (e.g., treating the roughness elements a porous surface) which is

possibly defendable – but has to be explicitly defended.

3) Size of sensor box: all the reasoning on p9 is understandable but at the same time the sensor box's size is quite arbitrarily chosen. Why is it 8 m in x, 20 m in y and 12 m in z (especially the latter choice is crucial!)? How sensitive are the results on these choices? How do these dimensions compare to the mentioned dominant scales of turbulence in the given example? Do the authors claim that these are general relations? Also, the authors state that "it pays off to obtain ... a large dataset accepting that it contains certain percentage of particle hits whose contribution will be discarded." (p9, l. 27) How (based on what) will certain particles be discarded? How can this be justified?

4) Mean vertical wind and associated 'far field correction' (p13, l. 35): quite some effort is dedicated to produce some 'plausible results' (no negative footprints). One would assume that the predominantly negative far field footprint results (as the authors state, 'due to the coordinate rotation', p13, l.25) from the local flow deformation (and associated gradients) in the vicinity of the target. In experimental work this issue is addressed with filtering of the data (trend removal, running mean) or applying what is called a 'planar fit' (Wilczak et al. 2001) – sometimes with different planes for different approach flow sectors etc. In the present framework this would (approximately) correspond to reducing the sizes of the sub-boxes of the target volume. Apparently, this did not work out (p13, l. 5) due to the restriction in the number of particles, but nevertheless it would be interesting to learn up to which discretization this has been tried (and with which results). Also, maybe a smaller number of particles (in a sub-box) would suffice to obtain an overall (spatial) trend of mean vertical velocity? Can the authors comment on any of these (apparently performed) trials and tests? The proposed far field correction – while apparently producing useful results in this very case – appears to be anything but general: for example if for another target box the resulting far field would be (slightly) positive, it would be quite difficult to argue that this would be removed by the correction.

5) There are a number of quite subjective judgments and choices. These include

- for example the number of required particles that is mentioned several times, but never substantiated.

- the size of the sensor box (major comment 3)

- p16, l. 25: 'encompassing only the near field', i.e. ca. 30 % of the total length of the LES domain. Why 30%?

- p16, l.30 : 'in this case study the threshold was set to include f(3)...' Why 3? What is the consequence of this choice?

6) The authors claim to have developed a 'technique' to estimate the error of a simpler, analytical footprint model when applied over an urban area. The technique, however completely relies on the assumption that their footprints are correct. Given the quite subjective assumptions they need (see previous comment), this assumption may not necessarily be very good. This 'technique' should therefore rather be labeled as a sensitivity test, thus avoiding to claiming the own results to be 'correct' when this cannot be demonstrated. It may also be noted that the similarity of the cross-wind integrated footprints (Fig. 9) is striking – given the simplification in the KM approach. On the other hand, the KM based much larger cross-wind dispersion (Fig. 8) is largely due to the larger (relative) source height. This should also be mentioned (see major comment 2).

7) Further on assessing differences between LES and KM footprints (Section 3.1, specifically p22, l.10ff): '....which has been modified to include the relevant streets...': this is another example of a subjective choice (see major comment 5): why are not all streets in the domain included? Can the authors comment on this? What impact would it have on the KM results?

8) The 'CO2 example' is extremely non-conclusive since it includes so many additional, and also not explained, assumptions (no impact of water sources, vegetation is uniformly distributed over land and has the same height [even if there are 'high vegetation' and low vegetation' areas], etc., etc.). The very same conclusion could have been

obtained by changing some parameters in the KM model alone (e.g., the roughness length being 10% of the mean building height). It is suggest to completely remove this entire section.

Minor comments

P2, l.2 such a sensor's. . ..

P2, l.10 'topography' is usually employed in connection with landscapes (hills, etc.) while here and in the following (apparently) a 'building topography' is referred to. To avoid misunderstanding either the wording should be changed (throughout) or the use of this expression should be made explicitly clear at this early stage.

P2, l. 17 of the turbulent flow field

P2, l.23 measurements cannot be 'extracted'

P3, l.19 'just above the roughness sublayer': at 2.3 m above the nearest building, this statement cannot be true, when taking the definition of the roughness sublayer as a reference (e.g., Raupach et al. 1991). These authors define "The term 'Roughness Sublayer' will indicate the entire layer dynamically influenced by length scales associated by roughness elements. . ..". Clearly, any flow property at 2.3 m above a roughly 60 m tall building will be locally influenced. Can the authors comment on this?

Fig. 2 caption: please mention that this is the urban grid. For better understanding of the turbulence recycling approach it would probably be helpful to indicate also the precursor grid.

P6, l.10 EC measurements IN Torni? The authors probably mean on top of the Torni building.

P6, l.13 by means of . . .

P7, l.4 with a constant value

P8, l.10 the release of particles is activated: one would need to know how many particles are released (per time step, say and (probably) per grid cell.

P9, l.5 I don't think 'fixating on the exact location. . .' is very clear – please reformulate

P10, l.2 reference simulation is run for 1 h 'to develop ABL turbulence sufficiently' (what is sufficiently?) and averaging is performed for the last 45 minutes. In other words, the effective spin-up time is 15 minutes. How does this compare to some eddy turnover time for the given situation? Can the authors comment?

P10, l. 14 to what do the computational costs amount in absolute terms?

P11, l.2 'each lth particle's coordinate. . ..': as this is formulated, not every particle is sampled (conditional on its position within the box) but only every lth particle. Is this what the authors want to say? And if so, what is 'l' and how is it determined? What is the reasoning not to sample all particles but a subset equally spaced in l?

P12, l.3 how large is delta_xf chosen?

P13, l.1 close to the top

P13, l. 12 what is 'negative far field'? Negative vertical velocity in the far field? Or negative footprint in the far (upwind) field)? Please specify.

P15, l.10 the criterion

P16, l.25 which is not sufficient. . .: based on what?

Fig 7, caption: incomplete (caption must include all information to understand the figure without reading the text).

P19, l. 30 . . .which would otherwise be employed: how can the authors state this? There are many different models of this kind, so this is only one of the models that possibly might be used.

Fig.9 caption: I don't think I can see any white circles. . ..

P22, l.1 that leads to

Fig. 11 the entries (labels) ‘vegitation’ (high and low) probably mean to refer to ‘vegetation’ and should be changed.

References

Raupach MR, Antonia RA and Rajagopalan S: 1991, ‘Rough-Wall Turbulent Boundary Layers’, Appl. Mech. Rev., 44, pp 1-25

Wilczak JM, Oncley SP, Stage SA: 2001, Sonic anemometer tilt correction algorithms. Boundary-Layer Meteorol 99:127–150. doi:10.1023/A:1018966204465

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