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Interactive comment

# *Interactive comment on* "A One-Dimensional Model of Turbulent Flow Through 'Urban' Canopies: Updates Based on Large-Eddy Simulation" *by* Negin Nazarian et al.

**Anonymous Referee #1**

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General considerations

In this contribution, the authors use high-resolution LES simulations over an idealized urban-type surface configuration to assess the turbulence characteristics and exchange efficiency for neutral stratification. Results are used to improve a number of scales and parameters, which form the basis for a multi-layer urban surface exchange parameterization for meso-scale atmospheric models and had previously been determined based on RANS-type simulations (with similar surface configuration). A number of configurational improvements (e.g. minimum domain size) are discussed, and also the inclusion of dispersive stresses is emphasized.



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Discussion paper

The paper is sound in its analysis (sometimes a little too optimistic in its conclusions, though) and the experimental plan, but not very carefully written (see the long list of minor comments). Also, the language could be improved (I have only indicated the obvious language errors). I have got two ‘rather major’ comments, of which the first is easily addressed, and the second may help to extend the speculations on why even the LES-based parameterization fails to reproduce some of the characteristics in real-building urban configurations. Once the comments are properly addressed, the paper can certainly be recommended for publication in GMD.

Major comments

1) The paper is based on an earlier finding of the authors (or some of them) that the BEP-tree underestimates vertical exchange of momentum and energy in the canopy (p3, l.25). This is essentially concluded from comparison to one experiment in Vancouver. I trust that the authors have also assembled other evidence from earlier validations of the model. It would be more convincing to support the motivation for the paper to briefly summarize this additional evidence – or at least make a reference to where it can be found.

2) P5, l.14 (u and w…): by neglecting the lateral component (and the corresponding covariances) the authors only consider frictional stress and not directional stress (v’w’ – including its dispersive contribution). At the top of the canopy – and depending of course on the wind direction relative to the orientation of the canyon axis – directional stress might be quite important as well. Can the authors comment on this?

Minor comments

P2, l.7 …from the street-scale to synoptic scales.

P2, l.13 ‘is being treated’ may be better

P3, l32 I’d use ‘hypothesize’ rather than speculate

P6, l. 13 is calculated..

Eq 8 I think, the ending | of the ‘absolute value’ (<u>) is missing here. Also, the rhs has dimensions of ms-2 (which corresponds to the usual acceleration due to the pressure gradient force), but the lhs is from eq. (1), which is multiplied with the density – so the rhs needs to be multiplied by density as well.

P7, l. 5 the simulation domain

P7, l. 17 the canyon height

P7, l.18 uniform grid resolution: but what it is? Also 0.03H in the horizontal?

P8, l.5 Fig. 4: is this the aligned or the staggered configuration from which the modeled data is averaged?

P8, l. 9 compared to the wind tunnel results: it has not yet been mentioned that the Brown et al. study has used wind tunnel experiments.

P8, l. 17 7.4H is used to ensure…: so, is Fig. 4 presenting results with the 7.4H domain height? Same (below) for the number of obstacles in the domain. Both should be mentioned in the caption of Fig. 4.

P8, l.17 what is ‘the lack of solution [for the entire BL]’?

Fig. 4 legend: what are the letters ‘M’, ‘O’ and ‘Q’ referring to? Also, in the text, k is used for TKE – so, it should be so in the figure.

Fig. 5 caption: ‘compared with’. WHAT is compared to RANS? (I assume the PALM output…but needs to be mentioned)

P10, l. 17 a more accurate flow model: but still, Fig. 4 suggests that PALM underestimates TKE (by a factor of 2 or so in the mid- canyon). Can the authors comment on this?

Figs. 5 and 6 the two figures share the same information for the three cases of Fig. 5 but they have different colors for the same configuration (what is yellow/brownish dots in Fig. 5 is a green dashed line in Fig. 6. It would be very helpful if these colors were the same.

P11, l 7 the total flux is given as <overbar(u’w’)> + <overbar(u’w’)>. One of the two terms should be the dispersive flux. (same in the caption of Fig. 7)

Fig. 7, caption ‘a)’ and ‘b)’: the panels do not have the labels a) and b). Also, it is a little disturbing, that the two panels do not have (exactly) the same vertical axis.

Eq (10) one is inclined to assume that ‘h’ in the integrals corresponds to the canopy height - but this has been denoted ‘H’ before. This must be consistent. Further, the notation: C_deq is what? The sectorial drag coefficient or an ‘equilibrium’ drag coefficient? Also, the notation ‘DELTA <u(z)> |<u(z)>| for the horizontally averaged mean pressure deficit seems to be wrong. It should rather be something like <DELTAp(z)> (with an overbar on p)

P14, l.13 are in good agreement: first, the authors probably want to refer to Fig. 8 (which is never mentioned in the text). Second, this figure shows that the ‘good’ agreement is rather qualitative than quantitative for small lambdas (e.g. for lambda=0.11, the difference in C_deq is almost a factor of two. . .)

P14, l.3 are discussed

Eq 11 in this equation, and together with the text, the length scale l_kM is introduced without explicitly mentioning it. In eq (7), this was l_k (as it is in the text), and in (11a) it is l_t. All this must be carefully introduced, so that the reader knows what is ‘generic’ (I assume l_k) and what is particular.

Fig.9 legend refers to eqs. 11 and 12, but should be 11a and 11b.

P14, l. 32 the length scale. . .

P14, l.33 h-d: I assume ‘d’ is the zeroplane displacement height, but this has not been introduced so far. And again, canopy height is now ‘h’ rather than ‘H’ as previously.

P15, l.1 correspond to

P15, l. 4 are then defined

P15, l.5 L_eps is now capitalized (also in Fig. 9 and its caption), while it was 'l_eps' in eqs (6) and (7) and on p15, l.1 (and last paragraph of p14). All this must be consistent throughout the paper.

Fig. 11 what are the 'double overbars' referring to here? (eq. (15), for example, does not have it. . .)

P17, l.21 Fig. 12 shows (does not demonstrate)

P17, l.27 especially at the canopy level: if you mean with this, z/H=1 exactly, this is probably not true (at z/H=1, for all three densities, the red line is closer to the blue circles. So, where exactly do you mean?)

P17, l.32 what is the 'turbulent equation'?

Fig.14, caption in the legend, new abbreviations are introduced ('G2017', 'N2019' etc. which need to be explained in the caption).

P19, l.11 result in a substantial

P19, l.16 results in relatively

P20, l.2 parameterizations

P20, l9 of the spatially. . .

P20, l.14 dissipation length scale

P21, l.2 assumption that the diffusion. . .

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